This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.
+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.
+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

COUNTWAY LIBRARY
HC 3ЬYI 4

Digitized by Google

FROM
WM. F. FELL & CO.,
ELECTROTYPERS, PRINTERS
AND BOOKBINDERS,
1220-24 SANSOM STREET,
PHILADELPHIA, PA.

BOSTON
MEDICAL LIBRARY
8 THE FENWAY

Digitized by Google

Digitized by Google

George Cowan.
Danville,
Ky.

Digitized by Google

Digitized by Google

Digitized by Google

Digitized by Google

George Bowan

TRANSACTIONS

OF THE

INTERNATIONAL MEDICAL CONGRESS.

NINTH SESSION.

EDITED FOR THE EXECUTIVE COMMITTEE

BY

JOHN B. HAMILTON, M.D.,
Secretary-general.

BOSTON MEDICAL LIBRARY
DEC 27 1950

VOLUME III.

WASHINGTON, D. C., U. S. A.
1887.

Digitized by Google

6
A
77
1877
v. 3
copy 1

PUBLISHED BY AUTHORITY OF THE EXECUTIVE COMMITTEE.

PUBLICATION COMMITTEE:

JOHN B. HAMILTON, M. D.,
Secretary-General.

A. Y. P. GARNETT, M. D.,
Chairman Local Committee of Arrangements.

C. H. A. KLEINSCHMIDT, M. D.,
Librarian American Medical Association.

WM. F. FELL & CO.,
ELECTROTYPERS AND PRINTERS,
PHILADELPHIA, PA.

Digitized by Google

ERRATA.—VOLUME III.

Page 2. For H. G. Beyer, Surg. U. S. "Army" read Navy.
Page 101. Third paragraph, first line, for "Resorcin-Cocaostäbchen" read Resorcin-Cacaostäbchen.
Page 136. For Professor "Rabb-Rückhard" read Rabl-Rückhard.
Page 160. French title, for "Defformité" read Difformité.
Page 175. French title, for "Sétro" read Rétro.
Page 181. For "Demonstrater" read Demonstrator.
Page 211. French title, for "Fundamentales" read Fondamentales.
Page 224. French title, for "Méthods" read Méthodes.
Page 536. End of line commencing "Brown-Séquard," for "on" read ou.
Page 543. Tenth paragraph, first line, for "livré" read livres.
Page 695. Last line, for "permis" read parmi.

Digitized by Google

Digitized by Google

Digitized by Google

Digitized by Google

SECTION VI—THERAPEUTICS AND MATERIA MEDICA.

OFFICERS.

PRESIDENT: DR. TRAILL GREEN, Easton, Pa.

VICE-PRESIDENTS.

Dr. JUSTUS ANDEER, Munich, Germany.
Dr. DUJARDIN BEAUMETZ, Paris, France.
Dr. DUDLEY W. BUXTON, London, England.
Dr. J. M. G. CARTER, Waukegan, Ill.
Dr. THOMAS EDMONSTON CHARLES, Rome, Italy.
Dr. HIRAM CORSON, Pennsylvania.
Dr. J. G. S. COGHILL, Ventnor, Isle of Wight, England.
Dr. HY. M. FIELD, Dartmouth, N. H.
Dr. THOS. A. FRASER, F. R. C. P., Edinburgh, Scotland.
Dr. A. FRICKÉ, Philadelphia, Pa.
Dr. SIDNEY H. C. MARTIN, London, England.
Dr. RICHARD W. MOTT.
Dr. WM. MURRELL, London, England.
Dr. CHAS. D. F. PHILLIPS, London, England.
Dr. RALPH STOCKMAN, Edinburgh, Scotland.
Dr. CHAS. WEBBER, Rome, Italy.

SECRETARIES.

Dr. F. DRONKE, Berlin, Germany.
Dr. ALFRED S. GUBB, London, England.
Dr. L. LEWIN, Berlin, Germany.
Dr. FRANK WOODBURY, Philadelphia, Pa.

COUNCIL.

Dr. A. E. Adams, Danbury, Conn.
Dr. W. E. Casselberry, Chicago, Ill.
Dr. J. H. Etheridge, Chicago, Ill.
Dr. E. P. Fraser, Portland, Ore.
Dr. Wm. B. Hazard, St. Louis, Mo.
Dr. W. F. Kinsman, Lancaster, Ohio.
Dr. J. W. H. Lovejoy, Washington, D. C.
Dr. J. F. B. Payne, Galveston, Texas.
Dr. Charles Rice, New York (Bellevue Hospital)
Dr. F. E. Stewart, Wilmington, Del.
Dr. A. B. Tadlock, Knoxville, Tenn.
Dr. J. B. Vanvelsor, Yankton, Dak.
Dr. De W. C. Wade, Holly, Mich.
Dr. Robt. D. Webb, Livingston, Ala.
Dr. Theo. A. Weed, Cleveland, Ohio.

Digitized by Google

FIRST DAY.

The Section in Therapeutics and Materia Medica met at the Columbian University, Corner of H and 15th Sts., on Monday, September 5th, at 3 P. M. Among those in attendance were President Traill Green, M.D., of Easton, Penna.; Vice-Presidents Justus Andeer, M.D., of Munich, Germany; J. M. G. Carter, M.D., of Waukegan, Ill.; Thomas Edmonston Charles, M.D., Rome, Italy; J. G. S. Coghill, M.D., Ventnor, Isle of Wight, England; A. Frické, M.D., of Philadelphia, Pa.; Wm. Murrell, M.D., London, England; Chas. D. F. Phillips, M.D., London, Eng.; Ralph Stockman, M.D., Edinburgh, Scotland; Secretaries Drs. L. Lewin, Berlin, Germany; Alfred S. Gubb, London, England; and Frank Woodbury, Philadelphia, Pa. Among those in attendance upon the Session were: Drs. J. Gnezda, Berlin; F. E. Stewart, Wilmington, Del.; De Witt C. Wade, Holly, Michigan; J. Z. Gerhart, Harrisburg, Pa.; Gasper Griswold, New York; J. N. Upshur, Richmond, Va.; John E. Brackett, Washington, D. C.; Hugh Hamilton, Harrisburg, Pa.; Wm. Ward, Washington, D. C.; A. S. Hull, Chambersburg, Pa.; H. G. Beyer, Surg. U. S. Army; J. Solis Cohen, Philadelphia, and others.

Vice-President Dr. Phillips, of London, took the chair during the delivery of the Address of the President of the Section.

HISTORY OF THE PROGRESS OF MATERIA MEDICA IN THE UNITED STATES.

HISTOIRE DU PROGRÈS DE LA MATIÈRE MEDICALE DANS LES ÉTATS-UNIS.

GESCHICHTE DES FORTSCHRITTES DER MATERIA MEDICA IN DEN VEREINIGTEN STAATEN.

OPENING ADDRESS.

TRAILL GREEN, M.D., LL.D.

President of the Section.

GENTLEMEN OF THE SECTION OF MATERIA MEDICA AND THERAPEUTICS OF THE NINTH INTERNATIONAL MEDICAL CONGRESS:—

Coming together after the very cordial welcome to this City which has been given to the Congress, we can enter cheerfully upon the work which we have undertaken to do by appointment of the Congress, as cultivators of the science of Materia Medica and Therapeutics. We are not strangers, as Mr. Bayard said this morning, but of the brotherhood of science, always and in every land united by a common bond. It is proper that we should hold in high esteem this department of

Digitized by Google

our professional studies, and consider how large and important is the place which it occupies. Those who are engaged in the practice of other departments of our profession are dependent, in a very large degree, upon the student of Materia Medica, who investigates the properties of medicines and makes known the results of his studies. The surgeon, the practitioner of medicine, the obstetrician, all are dependent upon him who pursues the study of Materia Medica.

We believe that our associates in the profession do not fail to appreciate the aid which they receive from us. The active practitioners of the profession learn much of the application and properties of medicines, but there remains beyond what they learn, which can be acquired by special experiments only on the lower animals, the study of the physiological action of remedies; and this the busy practitioner cannot find time for. We believe that the assent of the profession will be given to the truth of this statement. It is a noble service which we render to the other departments of medicine; let us give it most heartily.

It is interesting to observe that at a very early period in the history of this country, when the colonies were seeking to establish a government in the presence of a savage people, and while laboring to supply their physical wants, arrangements were made for giving instruction in Materia Medica.

As our history of the study of Materia Medica illustrates the statement made as to its importance in the profession, I will devote to this subject the brief time allotted to me in the midst of pressing work of the Section. In November, 1765, Dr. John Morgan commenced a course of lectures on Materia Medica, in Philadelphia. He graduated M. D., at Edinburgh, in 1763, and was a member of the R. C. P., Edinburgh, and F. R. S., London. It was announced that the lectures would be given three times a week and would last between three and four months. Dr. Morgan was eminently qualified to give instruction in Materia Medica. Writing to Dr. Cullen, he remarked, "I am now preparing for America, to see whether, after fourteen years' devotion to medicine, I can get my living without turning apothecary or practitioner of surgery. My scheme of instituting lectures you will hereafter know more of."

These lectures were designed for young men who were engaged in the study of medicine. The Medical School of the College of Philadelphia sanctioned the lectures, and in 1766 Dr. Morgan gave a course of lectures on the Theory and Practice of Medicine, in the College, which afterward became the University of Pennsylvania. He gave at the same time preparatory courses in Botany, Chemistry and Materia Medica.

Dr. Adam Kuhn, a native of Germantown, Pa., pursued his medical studies in Europe, commencing the study of Botany and Materia Medica under Linnæus, in Sweden. He wrote with great delight of his teacher, and the study of Botany in excursions with him in the early hours of the day, and other studies after these excursions. Linnæus imbued his pupils with his own enthusiasm, and Mr. Kuhn seemed to have been a favorite, for it was said of him "*Linnæo ex discipulis acceptissimus.*" Leaving Sweden, he continued his studies in the University of Edinburgh, where he was graduated in 1767. Three months after his return to America, he entered upon his first course of instruction in Botany, in May, 1768, and for twenty years taught Materia Medica in the Philadelphia College. This history shows us that these studies were introduced and conducted by the most thoroughly educated men of the time, and their education was conducted by the faculty of the University of Edinburgh; so early was there professional unity between the members of the profession on the Western and Eastern sides of the Atlantic; and this

Digitized by Google

has been strengthened by intercourse, in these later days, with the teachers of the schools and hospitals of London, Paris, Vienna, and Berlin. In 1788 the matter of forming a pharmacopœia was brought before the College of Physicians of Philadelphia. A committee to consider it was appointed, but nothing further was done. It proved, however, that there was a need felt in the profession for a work of this kind.

The first Dispensatory was published by Dr. John Redman Coxe, who was Professor of Materia Medica and Pharmacy in the University of Pennsylvania. At the early age of ten years he went to Europe, and completed his classical education in Edinburgh. He then attended a course of medical lectures there. His medical studies were continued in Philadelphia, and he was graduated at the University in 1794. He then returned to Europe, was a pupil in the London Hospital for a year; studied again in Edinburgh, as well as in Paris, and returned to Philadelphia in 1796.

Dr. Carson, the historian of our Pennsylvania University, says, "The American Dispensatory (Dr. Coxe's work just named) was largely derived from Duncan's Edinburgh Dispensatory." The edition which was in use when I entered the University was published in 1831, and was the ninth edition. There is nothing in the preface of this edition from which one may learn when the first edition was published. It is very certain, however, that it held a very high place among the physicians and pharmacists of this country for many years. It is possible that Dr. James Thacker wrote as early as Dr. Coxe, for I have found that he published "The American New Dispensatory," second edition, 1813.

Dr. Nathan Chapman, in 1817, published the first American work on Materia Medica and Therapeutics. I first became acquainted with this work in 1832, in the University. At that time the sixth edition was our text-book. I remember the great delight the study of it afforded me; the style was beautiful, and the instruction was very practical. The pupils of the University of that day have not forgotten this excellent treatise, nor the interesting lectures on the Practice of Medicine which its author delivered. Dr. Chapman refers with high commendation to Dr. Benjamin Smith Barton, as one who had done much to make known the medical botany of the United States. Dr. Jacob Bigelow, of Boston, succeeded Dr. Barton as a cultivator of the same field.

The Pharmacopœia of the United States was published in 1820, and was a valuable work, making known to the physician and pharmacist the properties of the officinal articles of the Materia Medica, and the method of preparing the standard preparations. Its regular publication and constant improvement every ten years from the first issue of it, has done much to advance the proper study of our science.

The very able work of Drs. Wood and Bache, "The Dispensatory of the United States," appeared in 1833. The sale of no medical work ever equaled the sale of this; the 15th edition appeared in 1883, "rearranged, thoroughly revised and largely rewritten, edited by Dr. H. C. Wood, Joseph P. Remington, PH.G., and Samuel P. Sadtler, PH. D. and F. C. S."

There were other treatises in the earlier years of the professional history of our science which were valuable helps to the study of Materia Medica, of which I should make mention: A third edition of Cullen's "Materia Medica" was published in this country in 1808; "Indigenous Medical Botany," three volumes, by Jacob Bigelow, M.D., 1817–1820; John Eberle, M.D., "Materia Medica and Therapeutics," two volumes, fourth edition, 1834; and Dr. Thos. D. Mitchell's "Materia Medica and Therapeutics," 1850.

Of the works of American authors, those of Drs. Geo. B. Wood and H. C. Wood

Digitized by Google

are worthy of the highest commendation. Dr. Geo. B. Wood's "Therapeutics and Pharmacology" appeared in 1856. Dr. Wood had filled the chair of Materia Medica in the University as well as that of the Theory and Practice of Medicine, and as a teacher had occupied an important place in the institution. He was able, financially, to do much for his class. He erected a hot-house and from it exhibited rare living plants which had medicinal properties. His nephew, Dr. H. C. Wood, now Professor of Materia Medica and Therapeutics, published his work on "Therapeutics, Materia Medica and Toxicology" in 1875, and new editions have appeared almost annually. The work has been highly educational through its presenting so well the physiological action of medicines. The publication in this country of Pereira's "Elements of Materia Medica and Therapeutics" marks an important era in the issuing of works of this class.

Our American edition was beautifully dedicated to Dr. Geo. B. Wood, and was the most complete work published up to that period in the United States. It passed through three editions, the lamented author dying while the second volume was passing through the press in England. The work is characterized by the exhaustive study of the subjects of which it treats. It is pleasant to read, in the author's preface to the third edition, that "Messrs. Blanchard and Lea, the respectable publishers of Philadelphia, having informed me of their intention to bring out a new edition of my 'Elements of Materia Medica' for the American market, I undertook the correction of the last London edition."

In 1879 Wm. Wood & Co. published in their Library, "Materia Medica and Therapeutics—Vegetable Kingdom," by Dr. Charles D. F. Phillips, of London; and in their Library, 1882, "Materia Medica and Therapeutics—Inorganic Substances," by the same author. Dr. Phillips gives the physiological action of remedies as far as they have been determined, and the publication of his works in Wood's Library has given them a wide circulation in this country. In 1882 Messrs. Wood published an edition of Trousseau and Pidoux's "Treatise on Therapeutics," from the ninth French edition.

In 1884 they published a "Manual of the Medical Botany of North America," by Dr. Lawrence Johnson, Lecturer on Medical Botany, University of the City of New York; Robley Dunglison, M.D., "General Therapeutics and Materia Medica," two volumes, fourth edition, 1850.

Dr. John B. Beck, Dr. Harrison, Dr. Biddle, Dr. Riley, added to our knowledge of Materia Medica, and there are many living teachers whose names should be mentioned. Profs. Stillé and Maisch, are the authors of a popular Dispensatory. Roberts Bartholow, Professor of Materia Medica and General Therapeutics in Jefferson Medical College, in 1876 published a "Practical Treatise on Materia Medica and Therapeutics." This work has passed through several editions.

Prof. H. C. Wood, of Philadelphia, and Isaac Ott, of my native city, have done much valuable work in determining the physiological action of medicines, and other living teachers and students of Materia Medica have added much to our knowledge of these properties of medicines within the few years past. Our fellow-laborers in Europe have done much to increase our knowledge of the physiological action of medicines, at this time the most important part of our study. We shall have reports in this line of work during the meetings of our Section, especially from Dr. Phillips, of London, now present with us; Dr. Stockman, of London, and Dr. Atkinson, of Edinburgh.

It should be stated, as important in the history of the subject now presented, that at the first meeting of the American Medical Association, 1848 ("Transactions Ameri-

Digitized by Google

can Medical Association," vol. I, p. 341), a paper was read on the Indigenous Medical Botany, and it was recommended by the Committee who made the report, that Botany should be made an indispensable study in a medical preliminary education. Dr. N. S. Davis was the Chairman of that Committee, and has taken great interest in everything connected with medical progress, and now is honored as President of this Medical Congress.

In 1849 another report was made by Dr. Davis, vol. II, "Transactions American Medical Association;" one by Dr. Porcher on the "Medicinal Plants of South Carolina." This was a very elaborate report, covering 186 large octavo pages.

In the same year Dr. Williams read a paper on the "Indigenous Medical Botany of Massachusetts," vol. II, "Transactions of the American Medical Association." In 1852, vol. V, Dr. A. Clapp's report was published on the "Medical Botany of the United States." It occupied 217 pages of the volume. This is a brief history of the progress of our science in this country. It is highly flattering to those who were so active in their work. No department of medical science was carried forward with equal energy, and none of them produced so many and such valuable works.

Permit me to say in conclusion, that here, as in all our Societies, we shall find that we mutually so help each other that we gain strength for future work. The social advantages are not the least of the advantages obtained in associations of this kind, friendships and acquaintances being formed which memory will recall in future years, and the brotherhood of the profession strengthened in every land represented in this Congress.

Entering upon the business of this Section, I look for your support in the discharge of the duties to which I am called as your Presiding Officer.

References made in the preparation of the above history to "History of the Medical Department of the University of Pennsylvania," by Dr. Joseph Carson; "A Century of American Medicine, 1776–1876;" "Literature and Institutions," by John S. Billings, M. D.; "Transactions of the American Medical Association."

Dr. PHILLIPS.—I wish to express my own satisfaction, and the pleasure of all who listened to the address of Dr. Green, with the tenor of his remarks. For myself and my friends in England, I desire to say that we are greatly indebted to American physicians, to Dunglison, Wood, Stillé, and many others, which have stimulated us to greater industry in the field of Materia Medica and Therapeutics. Personally, I have for many years taken great interest in therapeutics. The paper which I propose to read to-morrow, on the action of certain drugs on the circulation, will illustrate Dr. Green's views, since it is based upon exact observations upon the size of the kidney under the effect of different drugs, by the use of Roy's onkometer.

On motion, a vote of thanks was tendered Dr. Green for his address.

Digitized by Google

The following abstract was read :—

INTRODUCTION TO A SYNOPSIS OF THE MEDICAL BOTANY OF THE UNITED STATES.

INTRODUCTION À UNE SYNOPSIS DE LA BOTANIQUE MÉDICALE DES ÉTATS UNIS.

EINLEITUNG ZU EINER ÜBERSICHT ÜBER DIE MEDICINISCHE BOTANIK DER VEREINIGTEN STAATEN.

BY J. M. G. CARTER, M. A., M. D., Sc. D., PH. D.,

The study of medical plants growing in the United States is exceedingly interesting and important. Many physicians have worked in this field and much has been accomplished toward furnishing the medical profession a valuable list of medical plants.

The writer has prepared a synopsis of the medical plants of this country, which includes 140 orders, 620 genera, and more than 1300 species and varieties of native and naturalized plants. The list of vegetable remedial agents in this country has been increased to this number by the combined efforts of physicians and botanists who have made extensive investigations during the past few years in the Western and Southwestern States and Territories.

Much of the work has been done by the aid of the Government. Among the most eminent botanists to whom we are indebted for success in this work are Warren Upham, Minnesota Report on Botany; Prof. J. M. Coulter, Botany of the Rocky Mountain Region; Prof. Gray, and Brewer and Watson, in Botany of California and Pacific Region, and Dr. Rothrock, in Report on Botany of Southwestern States and Territories, in Wheeler's Geographical Survey. Prof. Gray's standard works and Chapman's Botany of the Southern States are invaluable to those who wish to examine medical plants.

It is well known that many of our medical plants are immigrants, coming to this country from across the Atlantic in most instances, though we have many introduced from warm southern climes.

Of the 500 plants described by that great Greek medical botanist, Dioscorides, nearly 2000 years ago, 100 or more are found in the United States as native or naturalized species. The naturalized plants embrace, among others, *Conium Maculatum*, *Solanum Dulcamara*, *Aconitum Napellus*, *Marrubium vulgare*, *Anthemis Cotula* and *Datura Stramonium*. The following are some of the plants described by Dioscorides found native in the United States: Achillea Millefolium, Brunella vulgaris, Juniperus communis, Polygonum Hydropiper, Lycopodium clavatum, Convallaria majalis, Geranium Robertianum, and Galium Aparine.

Of the order Ranunculaceæ, or Crowfoot family, we have in the United States some 17 genera and 40 species used in the treatment of disease. This number includes both native and naturalized species. The most familiar of these are, Aconitum, Hydrastis, Coptis, Cimicifuga, Hepatica, Caltha, or Marsh Marigold. Others, as Clematis, Anemone, Ranunculus, contain valuable species.

The order Cruciferæ, or Mustard family, has 16 genera and 30 species known to possess medical properties.

The order Leguminosæ, or Pulse family, is represented by 27 genera and 45 species. The most familiar genera are: Trifolium, Glycyrrhiza, Baptisia and Cassia, or Senna.

The order Rosaceæ, or Rose family, has only about 15 genera, but 57 species, some of which are very important. Prunus, Geum, Fragaria, Rubus, and Potentilla belong to this order.

The order Umbelliferæ, or Parsley family, is represented in the United States by 43

Digitized by Google

species and 26 genera. Of these I have lately found Conium maculatum growing wild in Illinois, probably introduced from Eastern States.

The most important family, as regards the number of medical plants it contains, is the Compositæ. Of this order there are in this country more than 180 species of therapeutic value, represented by 67 genera. Some of the most valuable of these are Eupatorium, with 14 species and varieties; Grindelia, with four species; Erigeron, with four species; Anthemis, with four species; Senecio, with eight species and varieties; and Taraxacum.

Ericaceæ, or Heath family, supplies our Materia Medica with 43 plants in 19 genera. Besides some of our edible berries, some of the important genera we have in this order are, Arctostaphylos, Gaultheria, Chimaphila, and Pyrola.

The Nightshade family, Solanaceæ, embraces Stramonium, Hyoscyamus, Capsicum, and Tobacco with other genera, altogether 29 species.

The Figwort family, Scrophulariaceæ, although not so important in a medical point of view, contains 15 genera and 31 species, used to modify or mitigate disease. Mullein, Toad-flax and Scrophularia are familiar plants of this order.

The order which contains the greatest number of native species, after Compositæ, is Labiatæ, or the Mint family. There are at least 65 species in the United States, in 31 genera. Some of the most common and important genera are Mentha, with eight species; Lycopus, with three species; Hedeoma, with one species; Monarda, with three species; Nepita, two species; Scutellaria, with five species; Marrubium, one species.

The Cone family gives us 30 species of medical value; the Lily family, 38; the Grass family, 18, and the Fern family 24 species.

The remainder of the 140 orders have from one to eleven genera and from one to twenty-three species.

It will be seen from this that the vegetable Materia Medica of our country is very extensive. Yet the study may be greatly simplified if we keep in mind the fact that species of the same genus have similar medical properties, and differ only in degree. On account of the greater strength of one property above another in different species of a genus, one plant may be very popular for certain diseases, while the other species are scarcely known. In the genus Grindelia, the G. robusta is a popular remedy for asthma and bronchial affections; while G. squarrosa is less known, and G. hirsutula and G. glutinosa are scarcely known at all out of their own locality. The principle mentioned just now, that all species of the same genus have similar properties, would make us understand that all the Grindelias are useful in the treatment of asthma, but that the G. robusta leads the list; likewise it teaches us that they are all valuable in malarial affections, but the G. squarrosa heads the list as a remedy in these diseases. The same may be said of many other genera, some species of which are popular remedies, while others are very little known. Since the therapeutic value of a plant so frequently resides in its fluid or exudative products, whatever affects these, by changing in any way their composition or modifying their elemental combinations, must modify the remedial action of the agent.

That plants vary in their therapeutic virtues, especially when grown under different conditions, has been shown by Prof. A. Vogel. He has observed that characteristic alkaloids are sometimes absent, when plants are grown under other than their natural conditions. For instance, hemlock does not yield conine when grown in Scotland; cinchona plants growing in hot-houses are nearly free from quinine; and tannin is found in greatest quantities in trees exposed to a full supply of sunlight. These observations, which it is hoped may be extended, lead us to infer that the different results obtained by different physicians, when administering the same vegetable drug, may be due often to the difference in the preparations themselves, produced, perhaps, by the atmospheric, climatic and geographical differences in the places of growth.

Digitized by Google

According to the principle just stated, we should expect the Grindelia hirsutula and G. glutinosa to have medical properties more nearly corresponding to G. robusta than to G. squarrosa, for the first three grow in California while the last—G. squarrosa—is found chiefly upon the plains east of the Rocky Mountains, although to some extent growing in the valleys of the mountains.

Of the many new remedies that have been introduced into our Materia Medica during the past few years, I wish to invite special attention to the American Mistletoe—the Phorodendron flavescens, and the Black Haw—Viburnum Prunifolium. I consider these plants two of the most important that have been added to our Materia Medica list. They are both, in the main, sedatives; the former being a sedative to urethral irritations, and the latter a sedative to uterine irritations.

DISCUSSION.

Dr. F E. Stewart, of Wilmington, Del.—The fact mentioned by the speaker, with regard to the variation in medicinal activity of drugs owing to circumstances attending their growth, reminds me of the statement made by Dr. Rusby with regard to the alkaloids of the Erythroxylon Coca. This gentleman has just returned from a scientific expedition across the South American Continent, during which he paid particular attention to the collection, mode of preparation and chemistry of commercial coca leaves. It is a current belief among those engaged in collecting the leaves, that there is contained in them a volatile active principle, which can be extracted from the leaves recently gathered, but is missing in the dried leaves. Dr. Rusby is, therefore, of the opinion that the ordinary preparations of coca do not represent the full therapeutic qualities of the drug, since they contain, besides cocaine, a volatile principle which is decidedly stimulating in its effects. Dr. Rusby has apparently confirmed this by his own investigations. He has analyzed the fresh leaves, and finds a new alkaloid to which the peculiar stimulating properties ascribed to the drug in South America are attributable; it is not present in the preparations made from the leaves in this country, and merely a trace can be detected after the leaves have been gathered for a few days and exposed to the air.

Dr. J. G. S. Coghill, of Ventnor, Isle of Wight.—In common with my professional brethren on the other side of the Atlantic, I feel very much indebted to the recent additions to our Materia Medica from the native plants of the American Continent. I have used most of them with much satisfaction, but what seems to me to be wanting is more precise knowledge of the purely physiological actions of these remedies as distinguished from their therapeutic actions. This is greatly wanting, as a remedy can never be used with that confidence and persistence in diseased conditions which is essential to success, unless we are thoroughly acquainted with their physiological action. I am sure this want of knowledge and uncertainty as to their effects on health has greatly interfered with their acceptance by the profession.

Dr. Chas. D. F. Phillips, of London.—I must also compliment the speaker upon the able presentation of an important subject. American drugs should be better known than they are. Some years ago, having had my attention called to Grindelia as a remedy in asthma, I used it with remarkably good effect. It seems most useful in cases of asthma with dilated heart, and is a good addition to the armamentarium of the physician.

Dr. Wm. Murrell, of London.—There is a remarkable difference in the strength of some drug preparations which are sent for experimentation as samples, and the

Digitized by Google

same agent when dispensed by the apothecary. As a rule, the sample bottles are much more effective in the same doses.

Dr. FRANK WOODBURY, of Philadelphia.—The subject of the indigenous medical botany of the United States is one which should not be allowed to pass without discussion by American physicians. The mistletoe of Europe is a powerful cardiac stimulant; and probably the American variety (Phoradendron flavescens) will be equally effective, since it has been used for its contracting influence upon unstriped muscular fibre, in uterine hemorrhage, and as an oxytocic. Rhamnus purshiana clinically is an acceptable substitute for rhubarb, and possesses decided cholagogue effects. It is useful in the treatment of a coryza by revulsion, twenty drops of the fluid extract being administered every two hours until several loose evacuations are obtained. This breaks up a cold in twenty-four hours or less.

Dr. F. E. STEWART read—

A PROPOSED INVESTIGATION OF THE MATERIA MEDICA OF THE WORLD BY THE GOVERNMENT OF THE UNITED STATES. A PLAN TO PROMOTE PROGRESS IN THE SCIENCE OF DRUGS.

UNE INVESTIGATION PROPOSÉE DE LA MATIÈRE MÉDICALE DU MONDE PAR LE GOUVERNEMENT DES ÉTATS-UNIS. UN PLAN POUR PROMOUVOIR LE PROGRES DANS LA SCIENCE DES DROGUES.

EIN VORSCHLAG ZUR UNTERSUCHUNG DER MATERIA MEDICA DER WELT DURCH DIE REGIERUNG DER VEREINIGTEN STAATEN. EIN PLAN ZUR FÖRDERUNG DES FORTSCHRITTES IN DER PHARMAKOLOGIE.

BY F. E. STEWART. M.D., PH.G.

We are overrun with "new therapeutic agents." They come to us from various sources. Teachers in therapeutics, physicians in every clime, missionaries all over the world, enterprising drug concerns, patent medicine houses (aided by the clergy), have united in one grand effort to discover some new agent to benefit the cause of humanity, until the market is flooded with a host of medicines bewildering to the senses.

Some of these "new therapeutic agents" are of undoubted merit; some of them are of little value; some of them are trash. Some have been introduced by honest means; some have been introduced by fraud.

Medical, religious and secular press are united in one grand system of advertising to create a demand for these new articles. They are introduced directly to the public, or indirectly through physicians' prescriptions.

The science and practice of pharmacy is being rapidly swallowed up by this business, and the practice of medicine seriously injured by it. What reasons have I for making these serious charges, and what plans have I for reform? It is the object of this paper to state the reasons and the plan.

I wish it to be clearly understood, from the start, that I oppose the introduction of new therapeutic agents by anybody, whether physician, pharmacist, manufacturing chemist, wholesale druggist or patent medicine house, except it be through some channel which shall thoroughly prove their merits before such powerful agents for good or evil are thrown open to the indiscriminate use of an innocent public.

Digitized by Google

1st. The manufacture and sale of medicines are being diverted from the legitimate channel, which consists of the physician's prescription compounded by the educated apothecary, and directed into the hands of uneducated drug merchants ignorant alike of disease or its treatment. These mercantile concerns have as their object money making, and in this respect they differ from the professional ideal that service to suffering humanity is of primary importance, and money making secondary to that. Therefore, they do not hesitate to enter a field for which they are in no way fitted, and have become competitors of both the physician and pharmacist, for they furnish the public with ready-made compounds labeled with directions for use and accompanied with circulars describing the disease for which their prescriptions are highly recommended as positive specifics.

2d. The pharmaceutical profession, who suffer much through this competition, are, to no small degree, usurping the prerogative of the medical profession, and many pharmacists prescribe over the counter, and even go out of their shops to visit patients. This has caused a feeling of extreme jealousy between the two professions.

3d. Though poor therapeutists, the great manufacturing houses, by employing skilled labor, are turning out elegant goods. But, being animated by a trade motive only, they do not publish their formulas and processes, but retain them as trade secrets, thus hindering progress in this branch of the science of medicine. Pharmacy, in their hands, is in danger of injury for want of this publication, and the time may come when valuable processes now in use will be lost arts.

4th. Pharmacopœias and text-books depend upon this knowledge for their growth and improvement. Much of modern pharmacy has no place in literature whatever, for the knowledge is retained in the private formula books of the manufacturers.

5th. Students in our medical colleges are not instructed in the composition and uses of the medicines which they are to use afterward in their practice. The reasons for this are obvious.

6th. The names of many of these compounds are not only unscientific, and therefore unsuited for scientific nomenclature, but they are also claimed as the private property of the manufacturers. How can pharmacopœias and text-books be founded on such a basis as this?

7th. If the knowledge of drugs is to be a science, it must be classified in scientific form and protected by a changeless nomenclature. "Science professes to exhibit what is actually known or may be learned in scientific forms, viz.: in the forms of exact observation, precise definition, fixed terminology, classified arrangement and rational explanation."* How is it possible to rationally explain the action of a compound whose exact composition is not known? Exact observation is impossible under such circumstances. How can partial knowledge be accurately defined? How can a fixed terminology be determined when the prefixes and suffixes on which it so greatly depends are used indiscriminately to represent active principles and compounds without reference to the plain requirements necessary to a scientific nomenclature?

8th. Physicians who employ the drugs and compounds brought to their notice by the manufacturing houses do not, as a rule, report the results of their observations concerning their effects to the medical journals. There are several reasons for this. In the first place, publishers of medical journals object to receiving such reports. First, because they are afraid that readers will think admittance to their columns has been gained through bribery; second, because they prefer that all such mention shall go through the advertising columns and be well paid for. The result of it is, that very little is actually known regarding the true therapeutic value of a large proportion of the drugs and medicines introduced during the past fifteen or twenty years. Even if

* This definition of what constitutes a science is drawn from *Porter's Psychology*.

Digitized by Google

this mention were made, unless the knowledge of the composition and art of manufacture of these medicines is published in standard literature, it would avail but little to future generations, for the time would finally come when many of the articles referred to would exist only in name.

9th. Medical journals derive their principal support not from their subscribers, but from their advertisements. Naturally, they are loth to say anything to injure their advertising patronage. For this reason they are usually silent in all matters relating thereto.

The secular and religious press also receive a large subsidy in this way from the manufacturing houses, and they, too, are silent. Therefore, to a greater or less extent, the entire press of the country is under the control of their medical advertisers.

10th. The manufacturers are twisting the patent, copyright and trade-mark laws around, for the purpose of obtaining a legal protection never intended by those who framed them.

"The policy of the patent law is, primarily, a selfish one on the part of the public, and only secondarily intended for the benefit of inventors, and then as a means to an end only. The Constitution of the United States gives Congress the power 'to promote the progress of science and the useful arts, by securing, for limited times, to authors and inventors, the exclusive right to their respective writings and discoveries;' thus showing, in this fundamental legislation, that the object sought is a benefit accruing to the public."*

"The theory of the law is, that the promotion of science and the useful arts is of great benefit to society at large, and that such promotion can be attained by securing to inventors and authors, for limited times, the exclusive right to their inventions and writings. That such theory is correct, it is needless to say. It is almost self-evident, or, at any rate, susceptible of proof, that the magnificent material prosperity of the United States of America is directly traceable to wise patent laws and their kindly construction by the courts."

"The patent laws promote the progress of the useful arts in at least two ways: first, by stimulating inventors to constant and persistent effort in the hope of producing some financially valuable invention; and, second, by protecting the investment of capital in the working and development of a new invention from interference and competition 'till the investment becomes remunerative.'"†

"A patent is a contract between the inventor and the Government, representing the public at large.‡ The consideration moving from the inventor is the production of a *new* and *useful* thing, and the giving to the public of a full knowledge thereof by means of a proper application for a patent, whereby the public is enabled to practice the invention when the patent expires. The consideration moving from the Government is the grant of an exclusive right for a limited time, and this grant the Government protects and enforces through its courts."

But the manufacturers are not satisfied with a control over their products that is limited in time, and to gain which they must publish their formulas and processes. They desire an unlimited control which shall last to all time, and they also desire to retain the knowledge of their processes as trade secrets. Furthermore, it would be a difficult matter for them to prove that their alleged inventions and discoveries are new and useful therapeutic agents deserving a patent. So, without conforming to the just requirements of the patent law, they endeavor to create lasting and secret monopolies

---

* Simond's "Manual of the Patent Law," containing quotation from Day *vs.* Union Rubber Co., 3 Blatch., 500; Kendall *vs.* Winsor, 21 Howard, 327.

† Simond's "Manual of Patent Law."

‡ Ransom *vs.* New York, 1 Fisher's Patent Cases, 252.

Digitized by Google

by law, contrary to the beneficent idea of the patent laws. In fact, if the requirements of the patent laws were enforced, it would soon end their monopolies.

The method adopted by the manufacturers to secure the protection of the law is this: When it is desired to market a compound, a name is coined for it and the article introduced under the coined name. This name the introducer claims as his trade-mark, and says to the public, "you have no right to market a similar product under that name." All the advertising machinery is now put in motion to create a demand for the article under the coined name, and, of course, the owner of the name practically monopolizes the sale of the article so named.*

As a trade-mark can be owned forever, the monopoly is practically everlasting, and as the use of a trade-mark does not necessitate the publication of the formula or process for preparing the article, this knowledge is retained indefinitely by the manufacturer.

The trade-mark law, however, was never designed to take the place of the patent law. It was never intended that the use of a trade-mark should give any exclusive control over the article upon which it is marked. A trade-mark is merely a mark of trade to distinguish one brand of an article from another brand of the same article. If I mark the letter "A" on iron manufactured at my establishment, that can in no way prevent you from manufacturing iron. But it is the intent of the manufacturers in the cases mentioned to prevent any one else from making the articles referred to.

It is an axiom of law, however, that a proper or descriptive name cannot be a trade-mark. And the courts have defined the descriptive name of an article to be the name used by the public when purchasing it.† Now, these coined names are the names by which the public distinguish the articles referred to when purchasing them. They cannot, therefore, be trade-marks. For example, "Cosmoline," "Vaseline," "Petrolina," etc., are synonymous names for the Petrolatum of the U. S. P., and the article is well known to the public by these names. They cannot, therefore, be trade-marks, as claimed.

Again, the public owe their safety to the fact that these names are the proper or descriptive names of the articles in question. If they are trade-marks they can be used with equal propriety on other classes of goods. It would be equally proper to use the name "Ingluvin" as a trade-mark on *guano*, as upon the excellent compound of chicken gizzard now known by that name, and claimed as a trade-mark by the introducer. Imagine the consternation that would occur if a physician wrote a prescription for "Ingluvin," and *bird lime* were furnished to his patient by the apothecary. "Iodia" might be used with equal propriety as a trade-mark on Paris green or rat poison, as on the well known sedative prescribed under that name. No, these names are descriptive, and it is fortunate this is a fact.

Most of these compounds are mere aggregations of old and well known drugs, and are not new and useful inventions for which a patent should be granted. As well might a patent be granted for every similar prescription written by a physician. It would not take long to lock up the whole Materia Medica to the exclusive control of a few monopolists at that rate.

The system under which these new remedies are usually marketed is known to the trade as the "Patent" or proprietary medicine system. From what I have already said it will be seen that the term "Patent" is a misnomer, and calculated to deceive. As I have already demonstrated, the patent law is scientific in its ends; and under its working secrecy is abolished, and monopolies can last only during the limited time that the patent holds. But the so-called "Patent" medicine system is one of secrecy and everlasting monopoly. The term "proprietary" expresses more properly what is claimed

* See claims made by Allen & Hanbury's, of London, against Parke, Davis & Co., in "Tonga" case.

† Brown on "Trade Marks."

Digitized by Google

by the manufacturers, namely, that their alleged remedies are property, in which they have a natural right, irrespective of law.

"The belief is very generally entertained, that inventors have a natural right to their inventions, of the same kind given by the statute, irrespective of the actual passage of the law.*

"Such is not the fact.†

"The right to the exclusive use of an invention is not a natural right—that is, pertaining to a man in a state of nature; but, when it exists at all, it is a civil right, pertaining to a man under the protection of a civil government.‡

"All will concede that one natural right of man is, to have an equal chance with his fellows to gather and amass the goods of this world. Suppose two men, under the protection and control of no human government, to be occupying and cultivating tracts of land side by side; for years they plough, sow and reap in the same manner and with the same rude tools. Finally, one of them invents a plow, with which he can cultivate twice as much land in the same time as before and do it better. There is no principle of natural justice which forbids the neighbor, upon seeing how well the plow works, from making and putting to use one like it. The doing so by the neighbor does not injure the inventor in any possible way. If the neighbor has not the right to make and put to use a plow like the inventor's, he is shut off from an equal chance with the inventor of amassing wealth, and this when his hindrance is no help to the inventor.‡

"Not only this, but the neighbor, at the time the inventor made his plow, might have already begun to ponder upon the poor work done by the old plow, and set about making a better one, and would have soon invented the plow himself, and thus acquired as good a title to the exclusive use of it as the prior inventor—a use, however, from which he would be debarred by a person having no better title than himself, a thing that would be clearly unjust.‡

"This last is by no means a merely suppositious case; for Patent Solicitors and Patent Office Examiners well know that the same inventions are made over and over again by independent inventors. . . . The frequency with which this is done would be most surprising, were it not another and a recognized fact, that the mind is governed by laws of action just as much as the body; so that, given a certain invention to produce, and two minds of similar knowledge and habits to produce it, they will be quite likely to travel through the same road to the same result."‡

An invention is merely knowledge crystallized into practical form. In other words, the inventor merely takes knowledge, which is the common property of us all, the results of the accumulated experience of ages, and by some fortunate thought puts that knowledge in a new and practical form whereby he can produce results hitherto unobtainable. Does the inventor owe nothing to the world for this knowledge which he had no share in discovering? What right has he to monopolize a new use of it forever simply because he discovered it? Morse discovered a method whereby electricity could be made to carry messages long distances over a wire. Had he the right, therefore, to monopolize the whole telegraph system of the world and hand it down to his heirs forever? Suppose that he had such a right, then the same right belongs to every inventor. The inventors of pens, ink, paper, cotton cloths, the printing press, steam engine, etc., etc., possess the same right. Under such ruling nearly every trade in the world is the property of some individual or his heirs and the rest of the world who use them are thieves. Suppose for a moment that all these trades were locked up to the exclusive monopoly of individuals, what would be the result? The many certainly would never permit the

---

* Simonds "Manual of the Patent Law."

† Simond's "Manual of the Patent Law;" Traite des Brevets D'Invention, par C. Renvivard. Phillips on Patents.

‡ Simond's "Manual of the Patent Law."

Digitized by Google

whole commerce of the world to be controlled by the few. They would rise in their might and soon put an end to such monopolies.

"In theory, his every neighbor is as strong as the inventor, and combined, they are much stronger. In a state of nature, therefore, the inventor would have no power to enforce his rights."

"An inventor," then, "has no right to his invention at common law. He has no right of property in it originally. The right which he derives is a creature of the statute and of grant, and is subject to certain conditions incorporated in the statutes and in the grants. If to-day you should invent an art, a process, or a machine, you have no right at common law, nor any absolute natural right to hold that for seven, ten, fourteen, or any given number of years, against one who should invent it to-morrow, without any knowledge of your invention, and thus cut me and everybody else off from the right to do to-morrow what you have done to-day. There is no absolute or natural right at common law that I, being the original and first inventor to-day, have, to prevent you and everybody else from inventing and using to-morrow or next day the same thing."*

Now, it is a plain thing to be seen that the proprietary medicine trade are attempting to create monopolies over various medicinal preparations by means of secrecy and by claiming exclusive ownership in the names of these articles. But the public have an equal right with the manufacturers to manufacture and sell the articles referred to. The proprietary trade are therefore arrogating to themselves rights which do not belong to them, but are public property. They, then, not the public who occasionally make and sell their alleged inventions, and who are bitterly denounced for infringement in consequence, are the thieves.

Finally, to sum the matter up, the proprietary medicine system is not only unscientific in that it is a system of secrecy and everlasting monopoly, illegal in that it is perverting the meaning of the patent and trade-mark laws, and attempting to obtain rights never contemplated by law, but it is unethical as well, for it deprives every man of his natural right, which is the free use of knowledge.

I have said enough to demonstrate the need of reform in our present methods of introducing new remedies. No reformatory measure will be successful, however, unless it be practical and progressive in its nature. It would be equally impossible and undesirable to stop the introduction of new remedies. But this introduction should be done in a proper manner. The present system is a hindrance to progress in the science of medicine. We need a system that will promote progress in this science, and in the useful arts connected therewith. It is the purpose of my paper to again call the attention of the profession to a plan that I have devised with this end in view.

My suggestions are as follows:—

1st. I would suggest that the medical and pharmaceutical professions unite in the formation of an association, to be known as the American Pharmacological Association. I would suggest that this Society have as its object a systematic investigation of the Materia Medica of the world. And I would suggest that this investigation be conducted under the auspices of the United States Government.

2d. For the purpose of testing the value of all drugs and compounds now in use, as well as all new introductions in the future, I would suggest that there be established, at the National Museum in Washington, a laboratory for investigating the physical structure, chemical composition, physiological actions and therapeutic properties of drugs, and that this laboratory be manned by physicians and chemists selected from the medical departments of the Army, Navy, and Marine Hospital Service.

3d. I would suggest that a national law be passed, requiring every manufacturer to

---

* Simond's "Manual of the Patent Law;" Am. H. & L. S. & D. Mach. Co. *vs.* Am. Tool & Mach. Co., 4 Fisher's Pat. Cases, 294.

Digitized by Google

publish the true formula and method of preparing medical preparations made by him, and that such publications be put on file at the laboratory, and a copy furnished to any one sending for and paying a nominal price for the same. A similar plan is adopted at the United States Patent Office.

4th. I would insist that patents should never be allowed by the government for a medicinal compound, unless it be first proved a new and useful invention in therapeutics in the hands of a commission of competent physicians appointed by the National Pharmacological Association for that purpose.

5th. To test therapeutic properties of a new drug or compound, I would suggest that it be first tested upon animals if possible, and then sent to the hospitals and dispensaries for further test, before it is placed on the market.* "Working bulletins" containing all obtainable information concerning the drug or compound to be tested should be sent with it.

6th. I would suggest that a monthly journal be published as the organ of this work.

To adopt such a plan as this, would unite the Medical and Pharmaceutical professions as members of an association. Both professions would have a common object and mutual interests. This would heal the breach now existing between them. It would place the manufacturer of medicines on a scientific basis conformable to the demands of science, and compatible with scientific nomenclature. It would free the press of the country from an incubus which sits like a nightmare upon it. It would do much toward sifting the good from the bad, and clearing our Materia Medica from trash. It would give the profession new and valuable medicinal agents, and in this way benefit not only the physicians, but it would benefit commerce as well, and be a great boon to the cause of suffering humanity. And last, but not least, it would be the means of saving a great many clergymen, as well as physicians, from the danger now menacing their eternal salvation, from the testimonial business.

I cannot close this paper in a more appropriate manner than by adding a compilation of the opinions and comments of leading physicians, pharmacists and scientists which I have received from time time, since my first paper referring to the plans which I have set forth herein.

Prof. Spencer F. Baird, Secretary of the Smithsonian Institution, when I submitted the plan to him originally, said that I might tell my friends that in his opinion it was one of the most important plans ever submitted to the Institution.

Prof. H. G. Beyer, Curator of the National Museum, in a letter written Oct. 20th, 1885, says: "I most sincerely sympathize with you in your plans of trying to organize the Association you speak of, and you may depend on my hearty coöperation in the matter, so far as I am able to assist you." "I think your conception of establishing a Bureau or Department of Pharmacology under the Government a grand one, and no doubt one that ought to be carried out. We have here all sorts of scientific Bureaus, and it seems to me not one which is calculated to be of such immediate benefit to mankind as a Department of Pharmacology would be to the American people, not to speak of the immense scientific value it would be to medicine and pharmacy. I, for one, should certainly hail the inauguration of such an institution with great delight. It is exactly what I have been having in mind for the last year and a half."

Dr. J. M. Flint, U. S. N., formerly Curator of the National Museum, in a communication addressed to the Smithsonian Institution, in March, 1884, said: "I would respectfully recommend that the organization of such a department be favored, as far as may be consistent with the general plans of the Institution, and such opportunities of special study offered as the means at your disposal will allow."

Horatio C. Wood, M.D., Prof. of Materia Medica and Therapeutics in the University

* I have been criticised for advising experiments on patients in hospital. Better that than experiment on the public at large, as is now the case. Patent medicine manufacturers, ignorant alike of disease or its nature, are now doing this.

Digitized by Google

of Pennsylvania, and author of a work on therapeutics which has already been translated into five different languages, in discussing my paper referring to this subject, read before the Philadelphia County Medical Society, June 25th, 1884, and published in the Proceedings of the Society for that year, said: "What seems to me the most important part of this paper of Dr. Stewart, is that in reference to the formation of a National Pharmacological Association, in which both doctors and pharmacists may meet. I think that that is probably practicable, as it is desirable. It would certainly be very desirable to have in connection with the National Museum at Washington a laboratory of original research in this regard. As is well known, the policy of this government is to maintain a speck of a regular army, which shall be the skeleton of an organization that shall expand indefinitely in time of war. It seems a necessity of this plan of organization that there should always be in the employ of the government numerous medical men for whom there is very little work, but who would be essential for the opening out of the executive minutiæ of hospitals in time of sudden war. Without expense to any one, some of these men could be well employed in connection with the National Museum, in Pharmacological research. It is well known that now at least one officer of the United States Army is employed in the National Museum, to collect drugs and do the materia medica part of the work.

"If we make a great ado about this matter, and attempt to found a great Pharmacological laboratory, which like Jonah's gourd will arise in a day, it will wither when the sun comes out. What is wanted is to get the attention of two or three men of great mind and equal influence at Washington, and having done this, allow this thing to grow, until it cannot be set aside."

Frank Woodbury, M. D., Prof. of Materia Medica and Therapeutics in the Medico-Chirurgical College of Philadelphia, and Editor of the Philadelphia *Medical Times*, in discussing this paper said: "With regard to the proposition for a national organization of physicians and druggists, it seems to me that there is a good nucleus for such an organization in the committee for the revision of the National Pharmacopœia. I am not aware that it was the fault of the physicians that they were not continued on the combined committee. I think that that should be the commencement of such an organization, and I should advocate the addition of physicians and pharmacists to that committee, in order to make a National Pharmacological Association.

With regard to a National Pharmacological Laboratory at Washington, the idea is a good one for utilizing some of the idle talent which is at the service of the Government in Washington, and it might be made useful to science. This seems to me the only practical proposition which has been submitted, and, perhaps, it is really the point which Dr. Stewart wished us to discuss. Such a laboratory would very properly come under the care of the National Board of Health. With it might be combined a Board of Public Analysis, which might furnish us with the composition of the various proprietary and patent medicines which are supplied to the people."

Joseph P. Remington, PH. G., Professor of Pharmacy in the Philadelphia College of Pharmacy, and one of the committee for revising the United States Pharmacopœia, in discussing the same paper, said: "Dr. Woodbury touched on a point which, I think, is the key-note of the whole discussion, that is, the formation of the National Pharmacological Association from a committee of physicians and pharmacists who have charge of the revision of the National Pharmacopœia. There is a definite aim. If a pharmacological association could be formed in which physicians and pharmacists could meet on a common ground, and in which there was a provision in the Constitution or By-Laws shutting out entirely any discussion about business or trade interests, and allowing simply the discussion of scientific subjects, I think we should find that through the influence of such a Society we should have in 1890 a Pharmacopœia which would not be the subject of so much adverse criticism as the Pharmacopœia of 1880.

Digitized by Google

Finally, in regard to the discouragements met with in advocating this plan for investigating the Materia Medica of the world:—

Prof. Beyer, whose letter I have already quoted, says: "I wish it were in my power to take steps in regard to establishing such an agency; but at present we have no rooms available for the purpose and no funds with which to sustain it."

Dr. Flint, the former Curator of the National Museum, says: "I infer, however, that at present neither space nor funds are available for such a purpose."

Prof. S. F. Baird, Secretary of the Smithsonian Institution, tells me the same thing in regard to money and means. How can the money be raised for such investigations? If an organization of earnest workers were effected with this end in view, the money would be forthcoming. There are experts enough in the employ of the Government who could be spared for work in the laboratory without extra expense to the Government. Very little expense would attend the collection of specimens through physicians in the employ of the Army, Navy and Marine Hospital Service. A building for doing the work would not be very expensive; exploring expeditions are continually being sent out by the Government for other purposes; and Government physicians and botanists could accompany them without costing much more; the main cost would be for animals, drugs and materials used for analysis, together with publishing the results of the investigation. An appropriation for this would not be hard to get if the matter were taken hold of in earnest by the right kind of workers.

DISCUSSION.

Dr. H. H. Rusby, on invitation of the Chair, spoke in favor of the proposed plan. No such opportune time has offered, or is likely to offer, as the present for taking action on the matter just brought to the notice of the Section, for the following reasons: (1) There is a general feeling on the part of the scientists of this country that the botanical department of this Government should be reorganized, with greater facilities for its work than it has hitherto possessed. (2) A committee to secure such a result was appointed at the last meeting of the American Association for the Advancement of Science in New York; and (3) A resolution will be passed by the National Association of Pharmacists at Cincinnati asking Congress to establish a department of Medical Botany. Action taken by the present International Medical Congress would doubtless add sufficient influence to produce the result desired.

Drs. Wade, Brackett and others spoke in condemnation of patent medicine legislation, as the business is one which seriously affects the health of the community.

Digitized by Google

SECOND DAY.

Dr. Traill Green, President, in the Chair.

Dr. Hugh Hamilton, of Harrisburg, Pennsylvania, read a paper on—

THE CHEMICAL PHILOSOPHY IN REMEDY.

LA PHILOSOPHIE CHIMIQUE DANS LE REMÈDE.

DIE CHEMISCHE PHILOSOPHIE IM HEILMITTEL.

BY HUGH HAMILTON, M.SC., M. D.,
Harrisburg, Pa.

Mr. President, Ladies and Gentlemen :—The activity in bacteriology and the advances in chemistry suggest that there is a chemical philosophy in remedy.

The lack of health may be termed disease. There is yet left the question, What is health? However, ill health may be divided into processes, at any time disturbing nutritive functions; or, secondarily, affecting them through traumatism.

The causes, have been a peculiarly tempting field for speculation in all ages. To name the numberless evanescent theories is unnecessary, because ingenious experiment has given a rational basis for etiology, insisting upon collective observation and the skillful use of instruments of precision, for determining the operation of physical law in, as well as without, the body.

Devotees of inorganic chemistry searched by analysis for the "elixir of life." To-day organic chemical synthesis performs on a grand scale some metamorphoses not long since dependent alone upon vegetable physiology and the circumstances of climate and locality. While even the animal physiological economy is forced to contribute its share to supply the wasted abstract or approximate elements of the human body for use in medicine.

The theory of the genesis of disease is now traced to micrococci, which seems reasonable, and the multiplication of specific germs for given-named diseases, when they occur synonymously, often permit great scope in generalization.

The brilliant deduction of Lister, from the investigation of Dr. Williams,* of London, in 1846, gave a practical impetus to this factor in disease.

The presence of masses of putrefying material, not among crowded human habitations, though filled with bacteria capable of producing disease, does not occasion sickness, while if oppositely situated may affect the organism.

Possibly these germs, to fulfill their life-cycle, require a longer or shorter residence in the body. The life-epoch may be slow or so rapid that the microscope, aided by electricity and photography, only can detect their changes.† They, when increased enormously, form *débris*, oppressing the function of the organs to dispose of it, thereby irritating the nervous system, creating the phenomena of fever, followed rapidly by other symptoms and signs of disorder.

---

* *Proceedings* Royal Soc. London, 1846, p, 1461.

† See article on Photographic Motion. *Century*, for July, 1887.

Digitized by Google

I. The chemical constituents of these lowly forms, whether vegetable fungi or animalcules, are a cell wall of carbon or carbon-hydrogen, enclosing a liquid more or less rich in nitrogen, possessing the physical property of endosmose and the vital functions of assimilation and multiplication; as soon as exosmose takes place disintegration and death of the germ ensues.

During life these minute creations must either absorb carbonic gases or oxygen, as the case may be; to the human organism the result is the same; a certain degree of [carbonic acid gas] blood poisoning.

Doubtless, then, germs are active in altering, by fermentation, the normal organic constituents of the blood into noxious ones, the result not unfrequently proving fatal. Exhibited in the following Table :—

TABLE I.

Names.	Approximate Elements.	Chemical Formula.	Remarks.
Lipacidæmia	Oleic Acid		
Lipæmia.....	Stearin	$C_{18}H_{36}O_2$	
Cholæmia.....	Cholein	$C_{26}H_{44}O$	
Uræmia	Urea	$2CH_4NO$	
Ammoniæmia	Ammonia	NH_4O	
Acetonæmia*.....	Acetone	$CH_3.CO.CH_3$	Di-*Methyl*-Ketone

Even their excessive life or extreme mortality exert similar influences, giving rise to the febrile movement, which may be defined as an irritation of the peripheral nerves, in turn acting inhibitorily upon a centre supposed to be in the bulb, allowing unlimited vital activity.†

Whether this irritation be applied to the skin, injected beneath it, absorbed by the receptive organs naturally, or through traumatic injury, increased appropriation of oxygen and expiration of carbonic acid gas is noted.

Exactly what the heat is caused by—a chemical action or reaction—is imperfectly known, *e. g.*—

1. Action $HO + SO_3$ evolves heat.

2. Re-action $HO + SO_3 + NaOCO_2$ evolves heat and gives off an unseen product, Carbonic acid gas.

Nevertheless, germs do lodge and multiply, compelling the organs to unwonted activity, develop fever and fermentive processes, cause inflammation inducing exudation, transudation and infiltration, until *hypertrophy* follows.

If, from some unknown cause, one organ neglects its duties, an *atrophy* is the consequence.

Germs appear to enjoy periods of prolific existence analogous to the vast zöological life-areas found in geology; the life epochs in scarlatina and variola seem to confirm this view, as they seldom again attack the system in such hordes.‡

* Klinische Diagnostik innerer Krankheiten mittels bakteriologischer, chemischer und mikroskopischer Untersuchungsmethoden. Von Dr. Rudolph v. Jaksch. Wien und Leipzig, Urban & Schwarzenberg, 1887. † On Fever. Prof. H. C. Wood.

‡ Drs. Edus & Jameson, in July number *Brit. Med. Jour.*, 1887.

Digitized by Google

The commencement and progress of convalescence from fevers marks the decadence of this vast internal parasitic population, whose death-débris of extra quantities of carbon and nitrogen, etc., in whatever shape they may be presented for elimination, greatly tax the individual organs.

II. To assist in clearing the system of this life activity, or to aid in rapidly removing these effete bodies, constitutes the aim of remedy; so that REMEDY might be defined *as the use of means to restore the body to healthy condition by prophylaxis, repair of injury or the correction of nutrition.*

Germs contain albumen; so, if subjected to the physical effects of vacuum, freezing, boiling or incineration, they suffer or perish. Chemically they succumb to the use of mineral acids, alkalies, certain salts and organic radicals. In a word, the deprivation of oxygen, either directly by oxidation of another substance capable of attracting and retaining it, by the loss of hydrogen, by the subtraction or substitution of elemental or approximate radicals. Consequently we can successfully exclude, arrest the development of, or totally destroy the bacteria. Therefore, prophylaxis occupies hygienic thought, and is the expression of ideal medicine. What advances! The germ discovery, disinfection, antisepsis, vaccination with the attenuated virus, internal antisepticism and the addition of certain animal digestive ferments to assist assimilation and nutrition.

Surgery points with pride to the protection of injured parts *from active oxygen*, so that no bacteria can exist; therefore, absence of irritation; hence *no fever.*

The general constitution of the blood may be altered through the presence of bacteria, and of free or nascent oxygen, realizing the same conditions of fermentive action as exist in the pile of refuse, capable of originating such pathological changes as are noticed in several anæmias. (See Table I.)

One more step in the process and we are introduced into the domain of partial and complete disintegration.

The ptomaines engendered by bacteria are divided into several classes, and upon subsequent elemental analysis show that they contain certain homologues of organic radicals.*

TABLE II.

PTOMAINES.	CHEMICAL FORMULA.
Choline	$C_5H_{15}NO$
Neuridine	$C_5H_{14}N_2$
Cadaverine	$C_5H_{16}N_2$
Putrescine	$C_5H_{12}N_2$
Saprine	$C_5H_{16}N_2$
Tri-*Methyl*-Amine	$(CH_3)_3N$
Mydaline	

Cornil et Babes, "Les Bacteries," Paris, 1887, p. 57.

The application of disinfectants suggested antiseptics, and leads us to anticipate their modified use in internal medicine.

* Remsen's Theoret. Chemistry, 1887, 3d rev. ed. "Animal alkaloids," Aitken, Lond., 1887. "Ptomaines," Brown, Lond., 1887.

Digitized by Google

TABLE III. *By Duclaux.*

Chemical Formula.	$\frac{1}{10000}$ in 1 of Liquid. SUBSTANCE.	Amount to stop development of germs.	Amount *not* sufficient to stop development.	Amount required to destroy the germs.	Amount in which germs could live but not multiply.	Amount required to preserve 1000 of soup.	Amount which would not preserve 1000 of soup.
$HgCl_2$.	Corrosive Sublimate	40	20	170	154	80	66
Cl.	Chlorine	33	24	44	33	2,320	2,170
$CaCl_2$.	Calcium Chlorid., 80°	90	76	268	224	5,880	3,575
SO_2.	Sulphurous Acid	155	117	500	200	5,265	3,660
SO_3.	Sulphuric "	170	120	500	300	8,620	4,900
Br.	Bromine	155	126	392	250	2,975	1,820
I.	Iodine	200	150	646	500	2,440	1,916
$Al_2O_3 + A$.	Acetate of Alumina	235	184	2,350	1,200	15,620	10,870
	Ess. of Mustard	300	175	1,690	1,220	35,700	25,000
	Acid Benzoic	350	250	2,440	1,960	8,265	4,760
	Borax-Sal.	350	264	13,890	9,090	33,330	20,000
	Picric Acid	500	330	1,000	700	6,660	5,000
	Thymol	145	450	9,175	4,715	50,000	27,780
	Salicylic Acid	1,000	893	18,660	12,820	...	28,570
	Pot. Permang.	1,000	700	6,060	5 000	6,660	5,000
	Carbol	1,500	1,000	45,450	23,810	376,000	250,000
	Chloroform	11,110	8,930	8,930	7,460	...	1,250,000
	Borax	15,140	12,990	20,830	14,500	...	83,350
	Alcohol	47,620	28,570	227,300	116,600	...	847,000
	Eucalyptol	71,400	50,000	8,900	4,800	...	171,570

Cornil et Babes, *loc. cit.*, p. 46.

III. Clinical experience has shown that remedies, although often empirically selected, are those containing efficient oxidizers, active appropriators of hydrogen, or by the substitution of radicals succeed in destroying the pernicious products of the germ, its spores, or the consequences of its mere existence in the vital fluid, *e. g.*, acetanilid, salol, quinine, atropine, and a host of others.

These facts, drawn from the domain of theory and the field of practice, bring to view an outline for the *philosophy* in the choice of *remedial* agents. When the exact laws of *physics* and *chemistry* are intelligently and successfully used to *restore nutrition*, the study of medicine will possess additional charms and the practice become a recreation.

DISCUSSION.

Dr. WM. MURRELL.—The subject is one of great interest. The branch of investigation is important, but there are very few workers in it. The contributions of Dr. Wormley in this country, and Dr. Stockman in Edinburgh, I regard as of special value.

Dr. RALPH STOCKMAN.—With regard to the part played by ptomaines in causing the symptoms in infectious diseases, I may here refer to some recent work by Dr. Phillip, of Edinburgh, on the formation of toxic bodies in phthisical cavities. He finds that if phthisical sputum be kept at the body temperature for some hours, and then treated by the Stas-Otto method for the separation of alkaloids, there is obtained an alkaloidal body which is an active poison.

This substance administered to frogs caused rapid general paralysis and slowing of the heart. In mammalia, a very large dose caused death, while small doses frequently repeated induced emaciation, rapid loss of weight and fever. One of the most interesting parts of the investigation is the fact that the toxic action of this body is, to a very large extent, antagonized by atropine, and this may account for the large use of atropine and belladonna in the treatment of phthisis. It seems

Digitized by Google

probable that further investigation will prove that in most of the infective fevers the symptoms are largely the result of ptomaine formation, and that the ptomaines result from the decomposition of albuminous matter by germs.

Dr. J. G. S. COGHILL.—The interesting fact just mentioned, showing antagonism between atropine and the products of the bacillus tuberculosis, receives some corroboration, clinically, since patients suffering with phthisis will bear much larger doses of belladonna without producing toxic symptoms than they could in health. It is by working on this line that we will attain to a really scientific treatment of disease.

CHLORATE OF POTASH.

CHLORATE DE POTASSE.

DAS CHLOR-KALIUM.

BY J. G. SINCLAIR COGHILL, M.D., F. R. C. P.

The history of Chlorate of Potash affords an interesting example of the vicissitudes of remedies in medical opinion. Late, comparatively, in therapeutic recognition, to many of the other potash salts, it very rapidly rose in professional esteem. At first lauded as a benign panacea in nearly every physical ill that human flesh is heir to, then almost as quickly consigned to the limbo of dangerous and doubtful drugs, latterly it has come, for the most part, to be regarded as either harmless or useless, except as a purely topical medicament. Prof. Oertel,[1] of Munich, declares it has no more therapeutic properties than common salt. An opinion very properly traversed by his learned translator, Dr. Burney Yeo. Dr. Ringer[2] concludes a meagre account of the chlorate with the discouraging remark, "Its influence, if any, on the organs of the body is unknown." According to Stillé and Maisch[3] it should only be used as a local remedy. Dr. Lauder Brunton,[4] however, gives it credit for a somewhat wider sphere of usefulness.

This paper is an endeavor to ascertain, as far as the present state of our knowledge of the subject will permit, the true value of Chlorate of Potash in the Materia Medica.

Chlorate of Potash was apparently the subject of notice by Glauber as early as 1657; it was first obtained by Higgins in 1786, and believed by him to be a kind of saltpetre; and later by Bertholet, who described it as the compound of a potash base with some unknown acid. It seems to have been first used in England, in the treatment of disease, by Dr. Garnett,[5] in 1795, and subsequently by Drs. Ferriar, Stevens,[6] and Kohler.[7] Its chemico-physiological properties were early investigated by Davy and by Stephens. It was made the subject of an elaborate series of experiments in animals by O'Shaughnessy,[8] in 1831; and on which probably the suggestions for its employment in medicine subsequently were, to a large extent, furnished. He found that when 30 to 60 grains dissolved in three ounces of tepid water were injected slowly into the cervical vein of a dog, no ill effects followed. The pulse rose in fre-

---

[1] *Ziemssen's Handbook*, Vol. III, p. 177.
[2] "Handbook of Therapeutics," p. 163.
[3] National Dispensatory," p. 1221.
[4] "Pharmacology," p. 539.
[5] Duncan's "Annals of Med.", p. 1797.
[6] "Pereira," Vol. I, p. 493.
[7] *Lancet*, 1836, p. 73, Vol. I.
[8] *Lancet*, 1831, Vol. I, p. 369.

Digitized by Google

quency and fullness, the blood of the tracheal veins became of a fine scarlet color, and in a short time traces of the salt were detected in the urine. The peculiarities of its chemical composition, the large proportion of oxygen, and the readiness with which, outside the body at least, it parts with that constituent; its great diffusion power; and the potential energy of many of its chemical combinations, all indicated, and no doubt led great expectations to be formed of, its medical efficacy. It was at first certainly employed in medicine on the theory that it yielded oxygen to the system, in diseases in which the blood was held to be deficient in that element. Hence, its earliest exhibition was in the various forms of scorbutus. Dr. Hunt, in 1843, seems first to have ascertained its value in cancrum oris, by whom also it was suggested to Herpin, of Geneva, and Bergeron of Paris, in mercurial salivation. It was first recommended by Dr. West, in membranous and ulcerative stomatitis in children. Mr. Corner,[1] of the "Dreadnaught," found "it acted like a charm in follicular, ulcerative and phagedenic stomatitis;" also in scurvy more rapidly than any other remedy. Drs. Budd and Sankey[2] remarked its effects in the cyanosis and dyspnœa of pneumonia and croup, with depressed circulation. They concluded the salt must be decomposed, from the marked and rapid effect. Weedon Cooke[3] spoke of it as a tonic of the highest value in all adynamic conditions, especially in hectic states of the system, when quinine and iron are inadmissible, "so powerful is it in oxidizing or decarbonizing the blood when liver and skin have failed in their offices." There is no tonic comparable to it, he says, in the sequelæ of the exanthemata, in cachexia of all kinds, and in all scrofulous diseases. Dr. Fountain,[4] of New York, in 1859, drew attention to its oxidizing properties, being capable of coming to the aid of the respiratory process; of stimulating secondarily the function of absorption; of relieving the cyanosis of heart disease; and of dyspnœa in cases of pneumonia typhoides, hydrothorax, and thoracic effusion of blood from intercostal wound, "where the chlorate of potash supplied the blood with oxygen sufficient to sustain life during the time respiration was seriously impeded." Osborne[5] declares the action of chlorate of potash to be chemico-physiological, depending first on its oxidizing property when in contact with morbific matter in the blood; secondly, on its remarkable physiological action on the muscular fibres of the vessels, causing them to contract and thus increase the blood pressure. He was himself singularly sensitive to its action. Five-grain doses caused the radial artery to contract to the calibre of a bell wire. Its action was similar on the muscular coat of the bowel. It was also recommended by Drs. Fountain, Turlé, and Harkin,[6] of Belfast—the latter always an enthusiastic believer in the virtues of the chlorate—in consumption and scrofula, but Austin Flint[7] declared that fourteen well observed cases failed to show it had any specific effect on developed phthisis. Dr. Harkin[8] also, following Hutchinson, urged its importance in purpura hemorrhagica. He suggested its use in all maladies dependent upon defective nutrition, excretion, aeration, and molecular metamorphosis. "It possesses the power of developing vital force in weakened constitutions, of retarding degeneration of tissues, and frequently of controlling the too rapid advance of senility due to climacteric conditions." He *believed* "a portion was carried off by the kidneys, another by diffusion into the liquor sanguinis, the textures, the blood globules and the white corpuscles; a third *may be supposed* to part with three equivalents of oxygen in the blood, leaving behind chloride of potassium, which may be detected in the urine as well as in the blood, of which it is an important element." By Seeligmüller[9]

---

[1] *Medical T.*, Nov., 1857, p. 476.
[2] *Guy's Hosp. Rep.*, Vol. VIII, 1851, p. 331.
[3] *Lancet*, 1859, p. 449, 29th Sept.
[4] *New York J. of Med.*, July, 1859.
[5] *Lancet*, 8th Oct., 1859.
[6] *Dub. Jour. Med. Sc.*, May, 1861.
[7] *American Quart. J. of Med.*, Oct., 1861.
[8] *Dublin Jour Med. Science*, 1880, p. 398.
[9] Stillé and Maisch, p. 1221.

Digitized by Google

and others and by Dr. Hüllmann,[1] more recently it has been extolled almost as a specific in diphtheria, but as strongly condemned by Isambert, Jacobi, of New York, and also by Von Becker, of Vienna, as not only useless, but even dangerous to young children, from its depressing effect. I have found it in this disease very efficacious, very soothing to the local condition and a great help to maintain or restore the general powers and state of the blood. It can only prove "dangerous" when given in too large doses, as unfortunately is the tendency on the continent.

Dr. Mead employed it successfully in chronic dysentery, "one of the most hopeless and intractable diseases." He injected twice or thrice a day chlor. pot., ʒ ss, glycerin ℥ ss, warm water, ℥ iij–℥ iv. It heals the ulcerations of the mucous membranes. At first the injection can only be retained half a minute. It has also been recommended, both internally and externally, in cancer, by a long list of authorities, from Milsom in 1837, Tedeschi in 1846, to Burow, of Königsberg, in 1873,[2] Vidal in 1875, and Entyboule, 1877.[3] The latter advocates a seven to ten per cent. solution to be applied locally, but Burow recommends the powdered salts or the crystals. The latter are more painful. Its great value as a sialogogue was early noted by Dr. Dethan[4] and by Headland,[5] who pointed out that it may reinforce the pancreatic digestion. For many years previous to 1856, Sir James Y. Simpson[6] employed chlorate of potash with marked success in cases of placental insufficiency threatening the life of the fœtus, as he expressed it, "from asphyxia," resulting either from the defective quality of the maternal blood or the reduced extent of healthy placental tissue. He noted also its undoubtedly marked influence in promoting the growth and development of the fœtus. Grimsdale, Bruce, Moir and many other competent observers have recognized its great value in this connection. It would be impossible to recite here all the various symptoms and maladies in which chlorate of potash has been successively employed, but the only reputation which it has retained and which is universally accepted, is its almost specific action in rapidly curing foul and unhealthy conditions of the buccal and pharyngeal mucous membrane by local application. Its effect in such cases is too much in evidence to be passed over, and, accordingly, to this humble therapeutic rôle is the chlorate of potash relegated in the orthodox works on materia medica.

As the reputation of chlorate of potash rose on one chemical suggestion, so it fell mainly on another. Tiedemann, Gmelin, Dumas and Majendie had shown that when administered, it could be readily detected in the blood, and it had also been recognized by various observers unchanged in the urine, but the analysis of Wöhler and of Stehberger, declaring that practically all the salt is eliminated by the kidneys, struck a serious blow to its reputation, which had been, unfortunately, too exclusively founded upon a chemical theory of its action, and which undoubted clinical proofs of its efficacy in a variety of morbid conditions were unable to withstand. This unfavorable reaction was further accelerated by the fatal consequences resulting in certain cases from too large doses of the salt, either accidentally or imprudently administered for the sake of experiment. In 1862, Dr. Fountain,[7] of Iowa, who had written enthusiastically of its virtues, to show his opinion of its innocuousness, took one ounce in a single dose, with disastrous effect. "Violent gastro-enteritis was produced; at first there was free diuresis, but suppression followed and death ensued in seven days." Fatal cases have occurred from a single dose of 600 grains; from doses of 300 grains on four successive days; from single doses of 240 and of 180 grains, which caused death in a young girl in

---

[1] *Deutsche Med. Wochensch.*, 14th Nov., 1884.

[2] *Berlin. Klin. Wochensch.*, No. 6., Feb. 10th, 1873.

[3] *Thèse*, Paris, 1877.

[4] *L'Union Médicale*, 4th June, 1867.

[5] "Action of Medicine," p. 343.

[6] "Obstetrical Works," Vol. II, p. 460.

[7] Bartholow's "Materia Medica and Therap.," p. 180.

Digitized by Google

fifteen hours.[1] Altogether, there are 47 fatal cases of chlorate of potash poisoning on record up to 1884. On the other hand, large doses of from ʒj to ʒiij thrice a day have been taken with impunity for considerable periods. Germain Sée has given ℥j in a single day with complete impunity. The fact is, we have, in chlorate of potash, to deal with a potent drug. With this, as with every active remedy, the dose and conditions of administration determine the effect. A small dose may be stimulating, tonic and recuperative, a large one depressing and destructive. No rule in therapeutics is more invariable, and no rule has to be kept so steadily in view when studying the effects of drugs on the system, both in health and disease. It has been well observed, that no medicine that has any real power in controlling or arresting diseased action can be innocently administered in healthy conditions. We must consider the tolerance of the patient, the state of the system, and the conditions of administration. Danger has unquestionably arisen and the salt has suffered grievously in repute from being regarded as simple and indifferent in its action. Dr. Jacobi [2] has referred to the strange circumstance, that as it declined in professional it increased in popular favor; more so in America than any other drug. As a local application, either in powder or crystals, its action is distinctly caustic. This, indeed, is the cause of the pharmacopœial and other chlorate lozenges in use being so objectionable and inefficient, from their containing too great a proportion of the salt. For years I have employed a lozenge prepared with sugar only, each containing two and a half grains in the same bulk. In this form the affected membrane may be kept continuously laved, by means of the saliva, with the chlorate in sufficiently effective solution, by keeping the lozenge between the cheek and gums, where with the pharmacopœial or Wyeth's preparations an eschar would be produced.

The symptoms of poisoning with the chlorate will depend on the amount exhibited. In large doses it acts as a violent irritant. Physiologically, small doses increase arterial tension, stimulate the heart and the respiratory function and redden the blood corpuscles. In poisonous doses these effects are altered in excess. The arterial tension gives way to depression and collapse, with fall of temperature, the circulation fails, from ultimate paralysis of the heart—Isambert, by injecting a strong solution into the veins found that the heart ceased to beat in diastole—the blood corpuscles are destroyed by the conversion of their hæmoglobin into methæmoglobin. The débris of these block up the renal tubules, producing hæmaturia, and subsequently the suppression ultimately observed in these cases. Where extremely large doses are given, the result may be brought about by the potash base acting directly as a protoplasmic poison on the brain and spinal cord, being the most highly organized tissues, as shown by Ringer and Murrell.

Some time ago I was very much struck with the account of Dr. George Henderson's[3] experience with chlorate of potash, recorded in his journey from Lahore to Yarkund, over the lofty passes of the Himalayas; and subsequently with the more detailed observations of Bellew,[4] in his very interesting narrative of his expedition through Kashmir to Kashgar, in 1874. To relieve the dyspnœa and general distress, called by the natives "Dam," caused by the extremely rarefied air on the higher passes, and which proved fatal to Sultan Sa'id Khan Shagi, of Kashgar, Dr. Henderson gave a strong solution of chlorate of potash to those suffering from the symptoms in question; more as a *placebo*, as he says, than from any belief in its efficacy. It had a good effect, but he professes not to know upon what principle it acted. Bellew's account of his experience is more

---

[1] Quoted by Dr. Boude, *New York Medical Record*, May 8th, 1880.

[2] *New York Medical Record*, March 15th, 1879.

[3] "Lahore to Yarkund," by Dr. G. Henderson, 1870, p. 57–58.

[4] "Kashmir and Kashgar," by H. W. Bellew, C. S. I. Surgeon Major, 1875.

Digitized by Google

detailed and extremely interesting. The following are his own words: "At Gyapthang, 15,150 feet, and still more, near Karacoram, 18,300 feet, the effects of the rarefied atmosphere on men and beasts were extreme. Several of the former tumbled off their ponies, from the giddiness produced, and some fainted. My own servant fell three times, and the hospital dresser was carried over insensible, but they recovered themselves on reaching the lower ground on the other side. Several ponies died. Even when seated quietly, writing, I found my pen every now and again jerked forward by an involuntary sudden gasp to fill the chest and raise the load pressing on it. And worse, just as I was going off to sleep, in hopes of forgetting the pain that racked my head and the nausea that well nigh floored me, I was started up by a sense of immediate suffocation. A few deep-cut, unsatisfying gasps, and a reeling giddiness, brought my head on the pillow again, to doze dreamily awhile, only to start up afresh and go through the same process again, and so on till the bugle bade me rise. The exertion of dressing—a luxury I henceforward carefully denied myself until we got down to a dressing level—well nigh finished me, and it was as much as I could do to mount my horse. Availing myself of Dr. G. Henderson's experience of his journey across this region in 1870, I had provided myself with a large supply of the salt he found so useful, and with very satisfactory results, as our further progress proved. I distributed little bottles of the chlorate of potash among the members of the embassy and such of the followers as needed it, and from my own experience can testify to its value in mitigating the distressing symptoms produced by a continued deprivation of the natural quantum of oxygen in the atmosphere. The large proportion of oxygen contained in the salt probably supplies to the blood what in these regions it fails to derive from the air, and thus restores through the stomach what the lungs lose. Whatever the explanation of its action, however, there is no doubt of its efficacy in relieving the dreadful nausea and headache produced by the circulation of an indifferently oxygenated blood, and no traveler ought to venture across these passes without a supply of this simple remedy in his pocket."

I was very much impressed with these very interesting statements. They were so entirely confirmatory of my own long acquaintance with the medicinal energy of this salt, extending from 1857, when, as assistant to the late distinguished Professor of Midwifery in Edinburgh, I had ample opportunity of observing its very potent effects in his practice, and from which time experience has only increased my confidence in its remarkable medicinal properties. I have steadily extended its employment to all cases in which the oxygenation of the blood is interrupted or defective from any cause, mechanical or constitutional—in fact, so impressed did I become with its undoubted power and evident therapeutic activity, that I felt reluctant to accept the catalytic explanation of its *modus operandi*, and indeed became rather skeptical as to the reliability of the original investigations of Wöhler and others, to which I have already made reference. Accordingly, with a view to test this question, I had, in association with my friend, Mr. Otto Wehner, F. C. S., three separate series of experiments made, and to eliminate as much as possible sources of fallacy, each observation made in duplicate and the mean taken, while a different analytical process was used in each series. The following is a résumé of the methods of analysis followed and of the results obtained:—

1. *Methods of Analysis.*—The old indigo test, most sensitive as a qualitative, being found both inconvenient and unsatisfactory in a quantitative examination of a fluid containing much organic matter, other modes had to be adopted.

The total amount of chlorine or of chlorides contained in urine is, as a rule, determined by evaporating a measured quantity of urine, incinerating the residue with the addition of a little pure potassium nitrate, dissolving the fused mass remaining in distilled water, acidulating with nitric acid, neutralizing the excess of acid by means of calcium carbonate, and titrating the chlorine by means of standard nitrate of silver solution. It is apparent that should chlorates be present in the urine, they would part

Digitized by Google

with their oxygen during the process of incineration and become converted into chlorides.

If urine be treated with a solution of potassium permanganate, until all the organic matter has been destroyed, the dioxide of manganese separated by filtration, and the excess of permanganate carefully removed by the addition of oxalic acid, the chlorine may be directly determined as above indicated. By the treatment with permanganate of potassium, chlorates are not altered, and hence that part of the chlorine which is present as chloride is only obtained. By subtracting the amount of the chlorine present as chloride from the total amount, that portion of chlorine which is present as chlorate is obtained. The amount of chlorides in normal urine is fixed and constant. As test experiments the amount of chlorine was determined in normal urine, after destruction of organic matter by permanganate. A known quantity of chlorate of potash was then added to a corresponding amount of urine, and the amount of chlorine again determined as above. The same figure being obtained showed that the chlorate does not decompose by treatment with permanganate. After incineration of a measured quantity of urine to which a weighted quantity of chlorate has been added, an amount of chlorine was found equal to that obtained in the previous experiment *plus* the exact quantity added as chlorate.

After these tests experiments, I ordered a patient to take, for six days, 120 grains of chlorate of potash, in four equal doses, collect the urine for twenty hours, and submit an ascertained proportion each day for examination.

1st exam.	120 grs.	excreted	57.02 grs.	47.5 per cent.
2d	120 "	"	122.56 "	102.1 "
3d	120 "	"	141.01 "	117.5 "
4th	120 "	"	101.77 "	84.8 "
5th	120 "	"	162.52 "	135.4 "
6th	?		12.81 "	10.6 "
7th	none		none	none
Total taken,	600 "	"	597.69 "	497.9 "
			Average,	99.6 "

It follows from these results that *all* the chlorate was excreted by the kidneys as *such*, without its having parted with any oxygen contained in it. The total quantity taken was practically found in the urine, the very slight difference being doubtless due partly to—

1. Error of experiment;
2. Impurities of the salt employed;
3. Unavoidable loss in administering and collecting secretions.

One interesting circumstance was that analysis reflected exactly, in the varying amounts of chlorate collected in each experiment, any irregularity in the times of the patient taking the dose. For instance, the great fluctuations in experiments 4 and 5 were ascertained to be due to the patient having forgotten to take the dose at the right time, and instead of being found in sample 4, it came out in sample 5, the latter containing the exact quantity deficient in 4. The patient was also instructed to take *six* daily quantities, and the analysis was carried out on that supposition, but five daily quantities only could be recovered, and it was subsequently ascertained that the patient only took the doses on five instead of six days.

In the second series of experiments the copper zinc couple described by Gladstone and Tribe was used by Mr. Wehner as a reducing agent, instead of the former method, because, on incineration a little chloride may be volatilized; and because some chlorate may escape perfect decomposition. This couple is prepared over 1 metre of well-crumpled, very thin zinc foil cut in shreds, a solution of 15 grammes of sulphate of

Digitized by Google

copper, the resulting black spongy mass being well washed with water. It reduces chlorate of potash in aqueous solution with the evolution of hydrogen with extreme rapidity, at a temperature near boiling point. The reduction takes place much slower when urine is the solvent of the salt.

In this second series of seven experiments Mr. Wehner himself, at 1.35 P.M., on the 12th of February, took 2 grammes of pure, dry chlorate, and separately collected and analyzed every batch of urine passed, swallowing the saliva all the time carefully. As a result of the sum of these observations it was found that of the 2 grammes of chlorate swallowed, 1.8764 gramme, or 93.82 per cent., had passed through the kidneys in 29 hours and 40 minutes. The seventh sample passed after this was found free from chlorate.

1st specimen	1.35 P.M.,	17.23 %	chlorate.	Rate	per	hour	excreted	11.49 %.
2d "	5.05 P.M.,	23.62 "	"	"	"	"	"	11.81 "
3d "	7.05 P.M.,	16.08 "	"	"	"	"	"	8.04 "
4th "	10.00 P.M.,	10.91 "	"	"	"	"	"	3.74 "
5th "	10.00 A.M.,	15.01 "	"	"	"	"	"	1.25 "
6th "	7.15 P.M.,	10.98 "	"	"	"	"	"	1.18 "
7th "	9.30 P.M.,	none		"	"	"		none.

To state it differently, of the dose there was excreted—

After	1 hour	and 30	minutes,	17.23	of the	chlorate	taken.
After	3 "	30	"	40.84	"	"	"
After	5 "	30	"	56.92	"	"	"
After	8 "	25	"	67.83	"	"	"
After	20 "	25	"	82.84	"	"	"
After	29 "	40	"	93.83	"	"	"

Total 98.83 per cent. recovered, or 29 grains out of 31 grains, in round numbers.

It will be noted that the rate of secretion is constant for the first period of 3½ hours, viz.: 11 per cent. per hour, and that during this time nearly one-half of the whole amount taken has passed through the kidneys, the rate of elimination then, for a time gradually, and toward the end of the period rather quickly, decreases.

In the third series of experiments Mr. Wehner took within two hours 8 grammes (about 123 grains) of pure chlorate of potash, and carefully collected all the urine passed for nearly three days—until a qualitative test for chlorate gave a negative result. The amount collected was nearly four litres. The different portions were mixed and made up with distilled water to 4000 cubic centimetres. From 40 cc. the chlorides were removed by a mode to be subsequently described, and the chlorate reduced with zinc and sulphuric acid; 0.0884 gramme AgCl were obtained. A second experiment also yielded 0.0884 gramme AgCl, corresponding to 0.07552 gramme $KClO_2$, or 7.552 in the total volume. 94.40 per cent. of the chlorate taken was, therefore, excreted by the kidneys unreduced. The remaining 6 per cent. was probably eliminated by other natural channels. It is improbable it was decomposed within the system, because if such reduction took place, it ought to have been proportionally much greater in the case of the small dose, than in the case where a large dose is taken.

The latter results were obtained by electrically reducing the chlorate to chloride. A large number of test experiments proved the method quite an original one, quite simple and exceedingly accurate.

Several interesting points developed themselves in the course of these experiments. The appetite became greatly improved—indeed voracious, even when the system was saturated with the largest amounts of the salt. The salivary secretion became intensely acid, as the dose of the salt was pushed beyond forty-five grs. per diem—the teeth even showing evidence of corrosion. The nature of this acid has not yet been determined. It is neither hydrochloric nor phosphoric. It may probably be sulpho-cyanic acid.

Digitized by Google

The specimens of urine, also, voided during the exhibition of the chlorate, remained for an indefinite time unchanged, as regards odor and appearance, although swarming with bacteria. This, so to speak, chemical antiseptic action of the chlorate is a most invaluable one, and has been largely made use of in appropriate cases, when the urine tends to decompose in the bladder under certain morbid conditions.[1] It is evident, however, in the light of our analysis, that it cannot act as an antiseptic by parting with any portion of its oxygen.

If we are to accept these results as conclusive and final, we are driven necessarily to the view that chlorate of potash acts by its mere presence in the blood or system generally, as a catalytic simply. In this respect resembling iodide of potash, of which Scharlar found fifty-three grammes in the urine, and two grammes in the saliva, sweat, tears, etc., out of a total amount of fifty-three grammes administered. There are many other medicinal substances, organic as well as inorganic, although very active, which do not change in the body—or, at least, go out of it in the same form in which they entered it. Chemists, however, are inclined more and more to regard with doubt and disfavor so-called contact actions, where bodies act by their presence merely without themselves undergoing change. Chlorate of potash cannot in itself, under ordinary conditions, by precipitation of bases, produce free acid; first, because it does not form any insoluble salts; and secondly, even if it did, it could not liberate acid, as it is a neutral salt itself, and in all its decompositions with other salts, would still remain neutral or produce neutral derivatives.

The accuracy of these and previous analyses were, however, challenged by Prof. Ludwig, of Vienna,[2] in 1881, and subsequently by Dr. I. Von Mering,[3] of Berlin, in 1884. Ludwig states that in a poisonous dose chlorate of potash acts like arsenic and phosphorus, and, the blood corpuscles being destroyed during the conversion, it is reduced by the blood to the condition of the chloride, for it could be detected neither in the blood, urine, nor contents of the stomach. Again, the chlorate can be decomposed in the kidneys, by feebly acid urine, into a base and an acid, and the chloric acid thus formed is energetically toxic. Professor Ludwig points out that the examples are increasingly numerous of stronger acids, under certain conditions, being separated from their bases by weaker acids, and therefore, it should not be surprising that the feebly acid urine should so act in this instance. These opinions were founded on a case of lithotripsy, in which Billroth had ordered, as usual in his clinique, a five per cent. solution to the extent of five grammes in twenty-four hours. The patient, aged 64, was found dead, and declared, by Ludwig, to have died with all the symptoms of chlorate of potash poisoning. It was ascertained that he had been given fifteen grammes by mistake. In the interesting discussion on this case in the Medical Society of Vienna, Ludwig[4] declared that the chlorate had been reduced in the economy, and had passed into the chloride of potassium, adding that it was no longer doubtful that the chlorate, under certain circumstances we are at present ignorant of, may become wholly, or in great part, reduced within the body. He referred to the opinion of Prof. Binz, who first pointed out that many organic substances (such as blood, fibrin, yeast, etc.,) when passing into a state of putrefaction, may decompose chlorate of potash. It is quite impossible, in the face of the elaborate and repeated analyses of Wehner, and other reliable chemists, to accept without reserve the inference sought to be drawn by so eminent an authority as Ludwig, that the whole of a poisonous dose of chlorate, amounting in this instance, to

[1] The power of the chlorate "to counteract putrescence of the fluids in scarlatina, typhus and cholera," was pointed out by Royle and Headland, "Mat. Med.," p. 96.

[2] *Jour. de Pharm. et de Chimie, Sept.*, 1881.

[3] "Das Chlorsaure Kali," Berlin, 1884. Dr. Thos. Stevenson, in London *Med. Rec.*, 1884, p. 518.

[4] *Allgem. Wien Med. Zeit.*, *Nos.* 44, 45.

Digitized by Google

fifteen grammes, could be entirely reduced in the *living* body. It is equally opposed to all chemical experience. I am inclined to seek an explanation in the theory that this large dose, more than half an ounce of the salt, acted directly as a caustic, destructively, on the tissues and blood vessels, thus supplying the putrefactive organic material, which according to Binz's experiments, is sufficient to decompose the chlorate, and that the complete reduction found by Ludwig, was due to post-mortem changes. Wehner [1] has suggested in this connection, that all the salt may have been separated by the kidneys, before the death of the patient, but his own experiments, showing how unvarying is the rate at which the chlorate is eliminated in the urine, render this explanation untenable.

In the same discussion Dr. Mezek stated that he had given sixty grammes spread over an average of fourteen days, in seventy cases of cystitis and cysto-pyelitis, simple and specific, and that neither he nor his colleague, Dr. Englisch, had observed any ill consequences. The acid reaction in the urine usually commenced on the second day; the triple phosphate disappeared from the sediment, which gradually lessened and became less viscid, while the irritability of the bladder correspondingly diminished.

Dr. Von Mering has carried out an elaborate experimental investigation into the action of chlorate of potash under different conditions. He has arrived at the conclusion that "this salt, under the influence of carbonic acid, and probably other acids, is decomposed in the system with the gradual liberation of chloric acid, which tends to reduce the alkalinity of the blood; and in this lies the key to the right understanding of the action of chlorate of potash." I confess my inability to reconcile this view with the considerations previously advanced. It is exceedingly improbable, within the body, that so very feeble an acid as carbonic acid could decompose chlorate of potash in an organic solution, liberating so strong an acid as the chloric. It is not impossible, of course, reasoning by analogy, for chloride of sodium is so reduced in the stomach into soda and hydrochloric acid, but in this instance there is a special provision and apparatus to accomplish the reduction. Chloric acid in solution gives off its oxygen much more readily than its potash salt does. If chloric acid were liberated it should certainly be expected to yield up its oxygen and become reduced, but Wehner's repeated experiments conclusively showed this not to be the case. The only other possible theory that can be advanced with a view to reconcile such experimental discrepancies is to suppose that the salt in its passage through the system is capable of gradual—it may be also partial—decomposition, and of the ultimate reconstruction of its constituent elements. The means of determining such a process are unfortunately not at present within our reach.

It is not, however, necessary, much less essential to belief in the efficacy of a remedy, that we should be able to follow and recognize the chemical processes within the organism by or through which its curative influence is attained. This most desirable consummation in the case of every drug, will doubtless come in due course with the progress of science, but we are not meantime to refrain from using them.

The limits to which this communication has already extended render it necessary for me now to describe, or rather to state briefly those marked conditions in which the exhibition of the chlorate is indicated, and has been found an efficient remedial agent when administered judiciously, with reference to dose and other pharmacological conditions.

The therapeutic properties may be stated generally as those suggested by its chemical constitution and affinities. As a salt exceptionally rich in oxygen, it has, without decomposition, the valuable property, *per se*, by its mere presence apparently, of oxygenating or aerating the blood, and so by restoring or exalting this vital character of the

[1] *The Lancet*, 11th March, 1882, p. 421.

Digitized by Google

circulating fluid, influencing to a corresponding degree the nutrition and functional activity of the various tissues and organs of the body. Beyond this it does not appear to have any specific action in any disease.

I have already referred to the experience of Simpson and others to its effect in preventing abortion when threatened from placental inadequacy. Its influence on the fœtus itself directly is no less remarkable. I have accumulated a mass of evidence in my own practice showing that its continuous exhibition during pregnancy—even when abortion or premature delivery has not been imminent—has an extraordinary effect in increasing the weight and development of the infant. In the case of a lady who has been under my care during eleven out of twelve confinements, the facts amount to a demonstration. Three out of her first six children were so feeble at birth that they barely survived the ordeal—the others being very small—the seventh child, a boy, was prematurely delivered at the eighth month, did not weigh quite five pounds, and was with the greatest difficulty reared. In this case the chlorate, which had been commenced at the beginning of the pregnancy, was suspended, by other advice, at the third month, when the patient passed temporarily from under my care. The effect of the suspension of the chlorate on the mother was almost equally disastrous. In five subsequent pregnancies the patient was placed, from the missing of the first period, on the salt, with the result that excellent health was enjoyed by the mother, that all the children were born at full time, and weighed from 11 to 11¾ pounds. They are in striking contrast, as regards size, to the child I have referred to as born at the eighth month. Many mothers, in my experience, have objected to take the chlorate in subsequent pregnancies, from their recognition of its effect on the size of their children in previous confinements, and so adding to their sufferings. The mother is usually conscious within two minutes of its ingestion, of the effect of the salt on the fœtus, from the increased activity and strength of its movements. The mother also experiences a feeling of exhilaration and *bien aise* at the same time, which often creates quite a craving for the chlorate, under the use of which appetite is greatly stimulated.

I have also for many years employed this salt in all cases of pulmonary insufficiency; in arrested phthisis, where there is excavation; in chronic pneumonic consolidation; in bronchitis where due aeration of the blood is impeded either from emphysema or thickening of the tubes; and in pleurisy with effusion, or empyema, where the lung is more or less compressed. I have also seen it help to tide over the crisis successfully in pneumothorax. When administered steadily and judiciously in such conditions, it has the effect of rapidly alleviating the symptoms of dyspnœa and cyanosis, and at the same time improving the general state and removing the accompanying feeling of distress.

In all cases of cardiac disability, either organic or functional, its action as a tonic and stimulant to the organ is very marked. It probably thus acts through the improved condition of the circulating fluid itself, which also exerts a potent influence on the nutrition of the tissues generally.

In all states of the system where the blood is impoverished either in amount or quality after hemorrhages; in convalescence from acute diseases, more especially where mucous surfaces have been involved; in anæmia; in chlorosis; and particularly in the peculiar deterioration of the blood which results from the various forms of malarious poisoning, the chlorate of potash is strongly indicated, either alone or in such combinations as the complexity of the morbid conditions present may demand. I have seen many cases, both of anæmia and chlorosis, that had resisted all the usual preparations of iron, yield rapidly to the tincture of the sesquichloride of iron in an effervescing solution of twenty-five grains of chlorate of potash thrice daily, after food—the base, no doubt, aiding greatly in producing the good result. Quinine, digitalis or nux vomica may be added, as the symptoms indicate.

Digitized by Google

Chlorate of potash should, if possible, be given in aerated solution. I order it in syphons, each containing ʒj or ʒij—marking the limits of daily doses for an adult. In this form it is more palatable and better tolerated by the stomach. It is very readily taken in milk, and also with wines or spirits, when such form part of the regimen. It should never be given in full doses except with or after food. The amount taken should be spread out over the day as much as possible. In moderate doses it is diuretic, partly, in all probability, from its stimulating effect on the heart, but as the kidneys are the main channel of its discharge from the system, it should be given with caution or withheld entirely where there is renal inadequacy and consequent accumulation of effete materials in the blood.

Its action as an antiseptic is peculiar and valuable. While it effectually retards chemical decomposition in organic fluids, such as the urine, even when loaded with pus, it does not prevent bacterial evolution. In all cases where there is suppuration, either simple or specific, of the genito-urinary tract; where the urine is phosphatic, purulent or offensive from any other cause, the chlorate, well diluted and administered continuously as a diet drink, either alone or in milk or barley water, will give most satisfactory results. I was much impressed with its effect in a case of prostatic abscess in a young gentleman resulting from a local inflammation caused by sitting, while heated, at a foot-ball match, on damp grass. It had passed into the chronic stage when I saw the case. There was a large proportion of offensive pus passed with each voidance of urine, and there was great pain and irritability of the bladder. The general health had suffered greatly from the long suppurative process and accompanying irritation. It had resisted all treatment, and up to the time I saw the patient, every variety of treatment had been tried, including washing out the bladder twice a day with various antiseptic solutions—latterly, Condy's fluid. I at once put the patient on a milk, farinaceous dietary, and gave chlorate of potash to the extent of ʒij per diem, at first alone and latterly with the tincture of the ferric sesquichloride. Relief began almost at once, and the cure ultimately complete within six weeks.

In chronic pharyngeal folliculitis or so-called clergyman's sore throat, I do not think its singular efficacy in combination with liquor arsenicalis is sufficiently recognized. Steadily persevered in and the arsenic pushed occasionally to its full effect, with the throat kept constantly day and night bathed in a solution of the chlorate in the form of the milder (grs. iiss) lozenge I have previously described, no treatment in my hands has been found so effectual in this troublesome and obstinate malady.

I need not again refer to the action of chlorate of potash as a purely local remedy in non-parasitic affections of the buccal and pharyngeal mucous membranes, it is so generally conceded.

If I have succeeded, even to a limited extent, in restoring to professional favor this once much-lauded remedy, the object with which this paper has been written will have been accomplished.

DISCUSSION.

Dr. H. A. Hare, of Philadelphia.—It should never be forgotten that chlorate of potash does not part with its oxygen at the temperature of the body; and even if it did do so, a sufficient quantity of oxygen could not be given off by the small quantity of the drug necessarily administered to do any good as an oxidizer in the body. Again, chlorate of potash should never be given internally in diphtheria, on account of its irritant action on the kidneys, which, in this disease, are already crowded with bacilli and greatly inflamed. As the object in this case is to locally affect the lesions in the pharynx, which is done by the elimination of the drug by the salivary glands, it is evident that the topical applications will do so as well, without danger of involving the kidneys. If chlorate of potash ever produces any changes in the organism,

Digitized by Google

it probably acts as do all potash salts, viz., as depressants, and not owing to its oxygen. Another objection to the use of chlorate of potash internally, particularly in diphtheria, is its depressing action on all the tissues, particularly the heart.

Dr. J. SOLIS-COHEN, of Philadelphia.—In diphtheria, chlorate of potash lozenges are very useful adjuvants to the treatment; there is nothing that gives as good results as chlorate of potash. It must not be pushed to the extent of injuring the kidneys, and as soon as there is evidence of such irritation, the dose must be reduced. In all inflammatory affections of the mouth, pharynx and air passages, the results are good. It is a fact that I previously have noted, that in diphtheria all the chlorine compounds have been found to be useful.

Dr. TRAILL GREEN.—The sodium salt is preferable to the potassium chlorate, as it is more soluble and less irritating. I first became acquainted with the salt in 1846, and generally give it in place of the other. Since the introduction of these salts the treatment of diphtheria has undergone a complete change; caustics are no longer used.

Dr. G. L. MAGRUDER, of Washington.—In dispensary service I have remarkably satisfactory results in the treatment of catarrhal conditions of the bowels, especially in young children during hot weather. In catarrhal states of the air passages, also, potassium chlorate is very useful.

Dr. F. E. STEWART.—In cases of fetid breath I have seen benefit from the chlorate of potash.

Dr. WM. MURRELL.—The chlorate of sodium has been largely used in London since the publication, some time ago, of Dr. Sainsbury's paper in the *Lancet* on "Chlorate of Potash."

Dr. FRANK WOODBURY.—It is not necessary that chlorate of potassium shall be decomposed in the blood in order to be therapeutically active; sodium chloride is an important article of food, but no one claims that it is unstable; on the contrary, after being disseminated all over the body, it is excreted in the same form in which it was administered. Too much attention should not be given to the mere analogies of chemical composition; oxalic acid and starch contain the same elements, but differ widely in their effects. The question of solubility, however, is of much importance. As the chlorate of potassium is not very soluble, it is liable to crystallize from the blood in the kidneys, and give rise to irritation or inflammation. As it is quite depressing in its effects, it should be given with caution to children; and a very colorable imitation of diphtheria can be obtained by administering the agent in relatively large doses to children with sore throat.

Dr. CARTER.—I use chlorate of potash very frequently in inflammation of the throat and nose with very good effect. I have been led to believe that the chlorate of potassium and all other chemical drugs, have an action, *per se*, as chemical compounds, and not as potassium or chlorine would have if uncombined.

Dr. POLLOCK, of Glasgow, Scotland.—All oxygen-bearing substances give up some oxygen in the body. Perhaps the marked benefit derived from the use of such agents is due to the fact that the oxygen that is given off is in a more than usually active condition. This may explain the discrepancy, to some extent, between the quantity of the drug used and the amount of actual benefit.

Digitized by Google

ON THE ACTION OF CERTAIN DRUGS ON THE CIRCULATION AND SECRETION OF THE KIDNEY.

DE L'ACTION DE CERTAINES DROGUES SUR LA CIRCULATION ET LA SÉCRETION DES REINS.

ÜBER DIE WIRKUNG GEWISSER ARZNEIMITTEL AUF DIE CIRCULATION UND SEKRETION DER NIERE.

BY C. D. F. PHILLIPS, M. D., F. R. S. E.

MR. PRESIDENT AND GENTLEMEN:—The paper I have the honor of presenting to you for this afternoon, is founded on experiments undertaken by me in conjunction with Mr. Rose Bradford, in the Pharmacological Laboratory of University College, London.

There is probably no single class of drugs that the physician requires to use more frequently than those known as diuretics, or supposed to have a diuretic action; at the same time these drugs are required for a large number of very different diseased conditions.

Further, the secretion of the kidney is so intimately connected with the vasomotor phenomena of the gland and of the body at large, or rather, with the *differences* existing at any given time between the local circulation in the kidney and the general circulation, that the action of any given drug or diseased condition on the renal secretion is an exceedingly complicated one. It would therefore obviously be useful to know the precise action of any given drug on the renal circulation, particularly as, when in doubt concerning the therapeutical value of a drug, we frequently turn to experimental pharmacology in order to throw light on the question.

With regard to diuretics, the evidence at present obtainable from pharmacology is by no means satisfactory, owing to a variety of circumstances into which we need not here enter, the principal one being, however, that either a very few observations have been made, or, what is worse, that these observations have not been conducted under precisely similar conditions as to animal, anæsthetic, etc. The majority of observers have contented themselves with simply investigating the flow of urine before and after the administration of a given drug; others have observed the blood pressure, and a very few have carried on simultaneous observations on both blood pressure and urinary flow. That this last method is much better than the first, becomes obvious when we remember how complicated a mechanism the secretion of urine is, and how largely, though not entirely, it is dependent on the vasomotor system. But even when the action of drugs on the kidney is investigated in this manner, and their action both on the general vascular system and on the amount of urine is determined, yet one very important factor is left out, viz., the effect of the drug on the *renal blood vessels.*

It may be said that the effect of the drug on these vessels can be deduced from the observed effects on the blood pressure and on the urinary flow; but that is not a scientific method of procedure, and further, it is, as will be seen below, a very fallacious one. It does not necessarily follow that a drug has the same action on all peripheral vessels, and it is quite conceivable that it may constrict those of one area and dilate those of another. This is particularly true of the kidney vessels, where the flow of urine is not so much dependent on the general blood pressure, as on the *difference* between the renal vessels and the vessels of the body at large. Thus an ideal diuretic would be one that dilated the blood vessels in the kidney, but constricted all others in the body.

Now, by the observations mentioned above, such an action as this could not well be demonstrated with any approach to scientific accuracy; so it occurred to us that it might be well to re-investigate drug action on the kidney from a rather different stand-

Digitized by Google

point to that usually adopted. It is well-known that Mosso and others have investigated the peripheral circulation by enclosing a portion of the body, such as the arm or leg, in a rigid vessel filled with fluid, and communicating with a delicate registering apparatus, so that any expansion of the limb would, by causing an outflow of fluid, produce a movement of the registering apparatus, which could be recorded graphically on the blackened surface of a revolving drum. Dr. Roy, of Cambridge, has extended this method to other organs, such as the heart, spleen, and kidney. In this method the kidney is enclosed in a metal box filled with warm oil, the box being of such construction that no oil is allowed to leak out, and at the same time the structures at the hilus of the organ, vessels, ureter and nerves, are not injuriously pressed on. This is effected by the box or oncometer being made in two similar halves, and each half having a piece of peritoneum (specially prepared) applied to it in such a manner that the space between the membrane and wall of the oncometer can be filled with oil. The two halves of the instrument are hinged together, and the kidney placed between the corresponding membranes of opposite halves. This membrane being very flexible, applies itself accurately to the kidney; so that, to use a simile, the kidney placed in the oncometer resembles, for instance, the heart enclosed in the pericardium full of fluid. The heart would represent the kidney, and the visceral layer of the pericardium the delicate membrane of the oncometer, the pericardial sac finding its equivalent in the oncometer, and the pericardial fluid in the oil filling the instrument.

It is obvious, then, that if the oil in the oncometer be placed in communication with a recording apparatus, any expansion of the kidney can be readily detected, and recorded graphically in the usual manner. The recording instrument is called by Dr. Roy the oncograph, to distinguish it from the oncometer. Its exceeding delicacy is shown by the fact that the tracing obtained in this manner from the kidney, say of the dog, is practically identical with a simultaneous tracing of the carotid blood pressure; the slight expansions of the kidney caused by each heart beat, as well as those caused by the respiratory undulations of the blood pressure, being perfectly recorded by this instrument.

If, now, in the same animal, we record simultaneously the general blood pressure, the expansion and contraction of the kidney, and the excretion of every drop of urine, we can determine the action of any given drug much more completely than before.

Our method of experimentation was shortly as follows: The animal (cat or dog) having been anæsthetised with chloroform, or with chloroform and morphia, a cannula was placed in the usual manner in the carotid. The kidney was then exposed, carefully separated from surrounding structures, and placed in the oncometer, this having previously been filled to the required extent with warm oil. A cannula was then placed in the ureter, and each drop of urine falling from it was made to break a circuit through an electro-magnetic arrangement, so as to make a mark on the recording surface for each drop, Before commencing the actual experiment the animal was curarized, and artificial respiration maintained, as any movements on the part of the animal might seriously damage the oncometer. Finally a cannula was placed in the external jugular vein, for injection of the drugs experimented with.

It is essential for success to handle the kidney as little as possible, as otherwise the flow of urine is much lessened; but incredible as it may seem, if due care be taken, the kidney will go on secreting (as pointed out by Dr. Roy) for many hours perfectly regularly, although placed under the abnormal circumstances of being enclosed in a box of warm oil.

In an experiment performed as just described, it will be found that the blood pressure curve and the kidney curve remain perfectly constant for several hours. The flow of urine is also very constant in its rate, although this varies considerably in different animals, owing to a variety of circumstances; but in the same animal, the rate of flow

Digitized by Google

does not sensibly alter unless some drug is injected, or the amount of chloroform administered is altered. This latter factor was maintained constant by blowing the air from the artificial respiration apparatus through a bottle containing chloroform.

We will now, having described our method, pass on to the consideration of the action of the various drugs experimented upon by us, beginning with citrate of caffein, as this is deservedly one of the most popular of diuretics. When a solution of citrate of caffein is injected into the external jugular vein, in doses of half to one grain of the salt, the following changes are seen to occur: The blood pressure is at first lowered, and afterward slightly raised: the rise, however, is not so great as the fall, and is generally of shorter duration. Both effects are, with these small doses, slight and transitory in nature. The fall is due to a diminution in the force of the cardiac beats, and also, perhaps, to the heart becoming contracted, so that less blood is pumped out during each systole; but whether this be so or not, the amplitude of the cardiac beats is markedly diminished. This period of *diminished* force is followed by one during which the heart is slowed in rhythm, and the individual beats *increased* markedly in force, and then the blood-pressure returns to the normal, and frequently rises slightly above it. This period of inhibition often persists for a minute or two, or longer if larger doses are used. Occasionally, and for a short time only, there is a period of marked acceleration; but this is not always observed, and is of the nature of an after-effect, not being so characteristic of caffein as the strong slow cardiac beat mentioned above.

The effects of caffein on the kidney, however, are much more marked. Immediately after the injection the kidney contracts, and this contraction may last for two or even three minutes, whereas the fall of the general blood pressure lasts only some twenty or thirty seconds, This marked contraction of the kidney (frequently too large to be completely registered), which is due to a constriction of the renal vessels, is followed by a large expansion: that is to say, the kidney not only rapidly returns to its former bulk, but expands considerably beyond, and to a much greater extent than the previous contraction. This expansion of the kidney is not only much greater in amount than the previous contraction, but also lasts a much longer time; a single dose of caffein citrate, *e. g.*, one grain, producing an expansion frequently lasting twenty or thirty minutes.

It will thus be seen that the effects of this drug on the circulation of the kidney, and on the general blood pressure, are by no means similar; since the peripheral action on the kidney would, *per se*, tend to cause an initial heightening of the blood pressure, instead of the observed fall. It might be urged that the contraction of the kidney was really due to the fall of blood pressure; but that this is not the case is shown by the fact that the two phenomena do not occur simultaneously, and do not, as mentioned above, persist for the same length of time, the kidney contraction lasting some minutes, the fall of blood pressure some seconds only. Further, if very small doses of caffein be used, the contraction of the kidney will follow, but not the fall of general blood pressure. This fall is undoubtedly a cardiac effect, as can readily be seen by inspection of the curves.

The effect on the urinary flow is also very marked. During the period of contraction of the kidney the flow of urine is either greatly diminished, or may even be totally arrested. During the subsequent expansion the rate of flow is frequently trebled, and this effect persists as long as the expansion. Thus, the effect of caffein citrate is a twofold one: during the first stage of its action there is a fall of general blood pressure, and great constriction of the renal vessels; during the second stage the blood pressure returns to its normal height, and the kidney undergoes great expansion; but this latter effect has a longer latent period, and persists longer than the former. Both the constriction of the renal vessels and the fall of blood pressure would cause less blood to

Digitized by Google

enter the kidney; and so it is not surprising that during this stage the actual secretion of the kidney is frequently temporarily arrested, to be succeeded by a marked increase during the second stage of the action of this drug. There are many drugs that will produce constriction of the kidney vessels, but as far as we have seen, caffein is the only one that will actually arrest the flow of urine.

As another example of a drug that will produce expansion of the kidney and marked diuresis, we may cite ulexin, a new alkaloid that has recently been obtained from the seeds of the common gorse, Ulex Europæus. Although the principal action of this drug is on the respiratory system, it is also a powerful diuretic; but it has the objection that, at any rate in animals, the diuretic action is only maintained with doses that would either kill through the respiration, or would produce violent convulsions. The action of ulexin on the kidney is as powerful, or even more so, than that of caffein; but it is much more transitory in its action. A dose of one-sixth of a grain injected into the external jugular vein will produce great heightening of the general blood pressure, accompanied by a contraction of short duration, followed by a very large expansion, during which the rate of the urinary flow may be quadrupled.

There is, however, this great difference between the two drugs, that whereas the caffein effect may last for half, or even three-quarters of an hour, the diuretic action of ulexin passes off in about ten minutes from the moment of its injection. There is another point of difference between caffein and ulexin, which is of some interest. They both cause constriction of the kidney, followed by expansion, although the effects produced by the latter do not last so long as those produced by caffein; but if the dose be frequently repeated, the following differences come out.

With caffein, if several doses of one grain be injected rapidly, one after another, into the external jugular vein, we soon arrive at a point when each injection only produces contraction of the kidney—there being no subsequent expansion and diuresis; in other words, the caffein ceases to have any diuretic action.

With ulexin it is different. Repeated doses, or single large doses, produce only a moderate degree of expansion, instead of the much larger double effect produced by small initial doses. Hence, although both drugs produce, in small doses, a double effect, the second portion of which—*i. e.*, the expansion—is not only the larger, but also the most important; practically, *excess* of caffein produces only the first phase, and *excess* of ulexin only the second phase of this double effect. With regard to caffein, this may be a point of some importance, since at this stage of its action it not only ceases to be a diuretic, but its action rather tends to arrest what flow there is.

Among other substances causing expansion of the kidney, we may cite dextrose. The injection of one cubic centimetre of a strong solution—twenty per cent. or fifty per cent.—produces a continuous expansion, remarkable for its extreme slowness and persistence. This expansion, although very persistent in its duration, is not great in amount, being much less than that produced by either caffein or ulexin.

Many other substances produce expansion of the kidney vessels, such as urea (determined by Dr. Roy), acetate and chloride of sodium, etc.; and it is very probable, although we have not yet determined this point, that all the constituents of the urine have a similar action, causing dilatation of the renal vessels to a varying extent in different cases.

The drugs investigated by us that produce only constriction of the kidney form a much more numerous group, including several that are usually considered good diuretics, and prominent among these is digitalin, which is further interesting from the fact that it is so much used as a diuretic clinically.

Digitalin, in doses of $\frac{1}{40}$ grain, produces marked contraction of the kidney. The peculiarity of this contraction is its extreme persistence, lasting for as much as half an hour, and sometimes, if large or repeated doses are used, the kidney does not

Digitized by Google

return to its previous volume throughout the duration of an experiment. It is needless to discuss here the effect of the drug on the blood pressure and on the heart, as they are both so well known; but observers are rather divided in opinion as to the action of digitalin on the kidney, some maintaining that such action is purely indirect —*i. e.*, due to its effects on the heart, etc.; others, that the drug is a true diuretic, and that the observed increase in the urine during its administration is not solely due to the improvement of the general circulation. It is not easy to decide between these two views, but it is interesting to observe that although, like caffein, it produces contraction of the renal vessels, yet, contrary to results with caffein, during this contraction the flow of urine is not diminished, but is generally slightly increased. This is a point to which we will return later; but it appears to us that the probable explanation is that, although both drugs produce contraction of the kidney, the one raises the general blood pressure while the other depresses it.

Spartein, a drug that has attracted considerable attention of late, has an action very similar to that of digitalin on the kidney vessels and on the general blood pressure; this being slightly raised, the cardiac beats increased markedly in force and diminished in frequency, and the kidney vessels constricted. The action of the two drugs on the flow of the urine is, however, very different. Thus, with spartein, in one case the rate of secretion prior to the injection was one drop in ten seconds; during the contraction of the kidney vessels caused by the drug, the rate fell to one drop in twenty-eight seconds, and after the kidney contraction had passed off the rate only rose to one drop in eighteen seconds. Hence it can scarcely be said that spartein is a diuretic, since its action on the renal vessels is accompanied by a marked diminution in the rate of the urinary secretion. On the other hand, spartein, like digitalin, strophanthin, etc., increases notably the force of the cardiac beats, although markedly slowing the rate, so that it is quite possible that in disease it will increase the urinary flow, not from any action on the kidney (since, as shown above, this kidney action is in the opposite direction), but simply because it improves the general circulation. It seems to us incorrect to class such drugs as spartein with diuretics, for although in disease an increase in the amount of urine is observed under their action, this is not from any specific local effect, but is rather from the improved condition of the systemic circulation. This is still more the case with regard to the kidney, since the action of many of these drugs on the healthy kidney is rather of a nature to diminish than to increase the amount of fluid excreted.

Further light is thrown on this question by the action of strophanthin, and also by the action of the glucoside apocynëin, obtainable from the Apocynum cannabinum (Canadian hemp). Strophanthin, as is well known from the work of Professor Fraser, acts mainly, if not entirely, on the heart muscle—that is to say, as far as its action on the general circulation is concerned; the heightening of arterial pressure produced by it being practically entirely due to the increase in the force of the cardiac beats. Thus, in this respect, it differs considerably from digitalin and spartein, where the cardiac action is accompanied by a constriction of the muscular coat of the peripheral vessels.

When the action of strophanthin on the kidney is investigated, it is found that the drug only causes a slight temporary contraction of the organ, which soon returns to nearly, though not quite, its previous volume. Thus, whereas digitalin will produce a contraction of the kidney, lasting half an hour, strophanthin, on the other hand, produces a much slighter contraction, lasting only twenty or thirty seconds. Further, it does not produce any marked increase in the rate of the urinary flow, althoug, *a priori* it might have been expected to do so.

We have made many observations on the Apocynum cannabinum—a drug that has,

Digitized by Google

we believe, a considerable reputation among some observers as a diuretic in certain conditions. The glucoside apocynëin was obtained by Mr. Gerrard for us from the dried plant, and this we have found to be a very powerful drug, resembling in many points strophanthin. The injection of $\frac{1}{24}$ grain of the glucoside (not quite free from adventitious matter) causes a contraction of the kidney very like that produced by a fortieth of a grain of strophanthin, lasting about fifteen to twenty seconds, after which the kidney trace returns to its normal level. The amplitude of the cardiac beats is greatly increased, the rhythm much slowed, and the blood pressure slightly raised. In many cases the rhythm is even more slowed than by strophanthin; for instance, in one dog, the rate was reduced from 100 per minute to forty per minute, after about one-sixth of a grain had been injected, in four doses of $\frac{1}{24}$ grain each, at intervals of fifteen minutes. In these respects this drug resembles strophanthin, as also in its not increasing the flow of urine; thus, in one case the rate of flow prior to the injection was one drop in twenty seconds, and subsequently to it, remained constant at the rate of one drop in twenty-two seconds, notwithstanding the fact that the drug had greatly increased the force of the cardiac beats.

Turpentine and adonidin produce marked contraction of the kidney, the effects of the former being very marked and persistent, and that of the latter resembling the contraction produced by digitalin, except that it is not of such long duration. Finally, barium chloride produces great contraction of the kidney, as well as great rise of blood pressure. The effects produced by this salt also resemble, to a certain extent, those produced by digitalin, but the rise of blood pressure after barium is much greater than that caused by digitalis.

In summing up the results obtained by us, we see that, on the whole, it is more common for drugs reputed diuretics to produce a contraction of the renal vessels than an expansion. Further, that the expansion is either slight—*e. g.*, as by acetate of soda, or, if large—*e. g.*, as by citrate of caffein, it is only produced by small and initial doses, and that if the dose be too frequently repeated no expansion is obtained. Ulexin possibly may be useful as a diuretic in small doses, but its powerful action on the respiratory mechanism is a great drawback. Thus, in the case mentioned above, one-sixth of a grain was used, but half this dose would completely arrest the respirations. In all cases this renal expansion is accompanied by a copious flow of urine (more persistent in the case of caffein than in the case of ulexin).

Then if we consider the drugs that produce contraction of the kidney, such as digitalin, spartein, adonidin, etc., it is obvious that they cannot all be bracketed together, since although they all produce a similar effect on the general blood pressure, and on the renal circulation, yet it is only the first that has any very obvious diuretic action. Again, we might expect such drugs as strophanthin and apocynëin to have a diuretic action, if anything, greater than that of digitalin, but experimentally it is found that such is not the case.

From all these considerations, we see further evidence of the truth, that the flow of urine is not so much dependent on the blood pressure as on the rate of flow of the blood in the renal vessels. With regard to this point, it is necessary to remember, that although such drugs as strophanthin produce a great increase in the *force* of the cardiac beats, yet these are very much slowed, so that it is quite possible that although the heart's action is stronger, yet the total amount of blood sent through any given organ, such as the kidney, in a given time, may remain the same. Whereas such a drug as digitalis, producing, as it does, a rise of blood pressure and a contraction of the kidney vessels, may cause an increased quantity of blood to pass through the renal vessels. On this view one could find the explanation of digitalin being a diuretic, and strophanthin not being one.

Digitized by Google

Inasmuch, however, as spartein has not so marked diuretic action, we must also assume that digitalin must have some peripheral action on the secretory apparatus of the kidney.

Our results may be tabulated briefly as follows, in three divisions:—

(A) DRUGS THAT FIRST CONTRACT, AND AFTERWARD DILATE THE KIDNEY.

1. *Caffein*—in small doses—induces in the stage of contraction, a fall of blood pressure—in that of expansion, a slight rise: during the former, the flow of urine *may* be arrested; during the latter it is always increased, such increase depending on dilatation of the renal vessels.

(The possible *arrest* of secretion during the first stage is special to caffein, and may be induced by large or repeated doses.)

2 *Ulexin*—One-sixth grain greatly raises blood pressure during the first stage (that of contraction): in the second, expansion is much greater in degree but shorter in duration than under caffein, and is accompanied by brief but marked increase in urinary flow: the effective dose is limited by its toxic action on respiratory centres. Practically, *excess* of caffein induces only the first stage—*excess* of ulexin only the second.

(B) SUBSTANCES THAT DILATE THE KIDNEY, BUT TO LESS EXTENT AND MORE SLOWLY THAN CAFFEIN AND ULEXIN,

Are dextrose, urea, sodium, chloride and acetate, and probably all constituents of the urine.

(C) DRUGS THAT CONTRACT THE KIDNEY WITHOUT SUBSEQUENT EXPANSION.

1. Digitalin, with *increased* secretion of urine (probably resulting from general heightened blood pressure).

2. Spartein, with *diminished* secretion (in health at least).

3. Strophanthin causes slight temporary contraction, with no marked increase of secretion.

4. Apocynëin; similar temporary contraction, and no definite increase of secretion.

5. Turpentine; 6, adonidin; and 7, barium chloride give similar results.

In conclusion, it seems to us that the plethysmographic method of experimentation is a valuable one for determining the exact action of drugs on the circulation, and one that deserves more attention than it has hitherto attracted.

DISCUSSION.

Dr. RALPH STOCKMAN.—Have you tested the action of nitrites upon the kidneys?

Dr. PHILLIPS.—No.

Dr. STEWART.—Was the blood pressure measured with a manometer?

Dr. PHILLIPS.—Yes.

Dr. MURRELL.—The subject is such a technical one that it is no wonder that there is some reluctance to discuss it without previous preparation. The subject of the action of diuretics is a very interesting one, and it is a field that needs to be investigated. We can make a man perspire very promptly, but we often find difficulty in getting the kidneys to work. I cannot see, exactly, why the instrument used is better than Marcy's tympanum for the purpose. It would be advantageous to use patients who have lost the anterior wall of their bladder, in order to study the action of diuretics, and to check the results of studies upon animals.

Dr. PHILLIPS.—I consider the oncometer as much more convenient to use and more readily seen. It is one of the simplest things possible when once in action,

Digitized by Google

and works by entirely different mechanism from Marcy's instrument. The true diuretic is one which directly causes expansion of the kidney by increasing the blood supply of the organ. We do not find this to be the case with digitalis, which acts upon the heart and forces more blood through the kidney within a given time, thus increasing the proportion of water in the urine. It is not a true diuretic.

Dr. WOODBURY.—Having been impressed by the interesting character of the experiments which have been reported, and realizing the importance of the subject, I would ask if the chemical analysis of the urine was systematically conducted in connection with the physical examination? It seems that this is very necessary in collecting the data for a comparative estimate of the value of diuretics. Water forms the bulk of the urinary secretion in mammals, and may vary in quantity within rather large limits, without indicating anything more serious than the state of supplemental activity of the skin. In fact, it may be influenced by meteorological conditions. In a case of diabetes mellitus, seen some years ago while Resident Physician to the Pennsylvania Hospital, there were sudden fluctuations in the quantity of urine voided, which for a time seemed unaccountable, until it was noticed, by comparison with the weather record, that the exacerbations preceded a snow or rain storm, and that they gradually passed off when the storm actually appeared.

Water is only incidental to the urinary secretion; snakes' urine is solid, birds' urine is nearly so. The characteristic and essential ingredients are the nitrogen-containing salts, such as urea, the urates, creatin, etc. A true diurectic, therefore, would be one that influenced the separation of such excrementitious waste from the blood by the parenchyma of the kidney. The testimony with regard to the influence of digitalis upon the excretion of urea is conflicting; at times and under some circumstances it is claimed that it increases the discharge of urea, and may, therefore, be considered as a true diuretic. Have such determinations been made in the present series of experiments?

Dr. PHILLIPS.—These experiments have been under way for three years and a half, but they are not yet concluded; the chemical analyses have been made in some cases, but not yet in sufficient number to report. It was found that all diuretics increase the amount of circulation in the kidney. This investigation is only as yet in its infancy, but it is believed that in the course of time the results from this line of work will give more information of the action of diuretics and their relative value.

Digitized by Google

THIRD DAY.

The President in the chair.

Dr. JOHN E. BRACKETT, of Washington, D. C., read a paper on—

CASCARA SAGRADA (RHAMNUS PURSHIANA).

BY JOHN ELY BRACKETT, M. D.

RHAMNUS PURSHIANUS.

Synonym.—Cascara Sagrada, cascara (bark), sagrada (sacred), is the common Spanish name, and means simply sacred bark.

Part Employed.—The bark.

Natural Order.—Rhamnaceæ.

Habitat.—California and Oregon.

Properties.—Laxative, cathartic and intestinal tonic.

In its botanical origin, this plant belongs to the natural order of Rhamnaceæ and the tribe Rhamneæ, and is indigenous to the Pacific slope of North America, noticeably, to the States of California and Oregon. It is a small tree, and its name was given in honor of the Prussian botanist, Frederick Pursch, who, in 1814, gave it a fixed place in botany.

The plant rarely exceeds twenty feet in height, averaging from ten to fifteen feet, and measuring through the trunk from six to nine inches. The leaves are elliptical, from three to five inches in length, by one and a half to two inches in their greatest width. The flowers are small and white, and appear after the leaves have matured. The fruit is a plain, round, black, berry about a quarter of an inch in diameter, and contains three seeds. Both the tree and the fruit, it will be seen, are much larger than other species of the Rhamnus group.

In its *microscopical structure*, the outer bark consists of dark-brown, withered cells, then several rows of cells filled with a dark-red coloring matter, and in newly-gathered bark, a couple of rows of cells containing chlorophyll. The middle bark is made of parenchymatous cells, which are filled with small starch grains. In a transverse section may be seen several groups of cubical crystals, and in a longitudinal section groups of very thick-walled yellow cells. The inner bark consists mainly of yellow medullary rays, separated by bast parenchyma, through which are scattered numerous yellow bast fibres. In longitudinal sections these same fibres are frequently surrounded by cubical crystals.

In its *chemical composition*, there is 1st a brown resin of strong, bitter taste, contained mostly in the middle and inner layers of the bark; this is sparingly soluble in water, freely soluble in alcohol and dilute alcohol, scarcely soluble at all in absolute ether, but quite soluble in chloroform. Caustic alkali solutions dissolve it with a beau-

Digitized by Google

tiful purple red color, which may be precipitated by acids. Animal charcoal will remove it from alcohol solutions.

2d. There is a red resin which is nearly tasteless and which is soluble in water, freely soluble in alcohol and dilute alcohol, only sparingly soluble in ether and chloroform, quite soluble in caustic alkali solutions, to which it imparts a rich brown color, and may be precipitated by acids. This resin is not removed from alcohol solution by animal charcoal, and is found in the outer layer of the bark.

3d. There is a light yellow resin, or natural body, tasteless, insoluble in water, soluble in hot alcohol and in chloroform. In concentrated solution it deposits granules of a pale orange color. It gives negative results with the general tests for alkaloids.

4th. From an absolute alcohol solution a crystallizable body is obtained, occurring in white, double pyramids and in some other forms of the dimetric system. At a temperature a little above the water bath, the crystals first melt and then sublime, the sublimate being partly crystalline. This crystalline substance is not noticeably soluble in chloroform or ether, is slowly soluble in absolute alcohol and slightly so in dilute alcohol. The acids do not dissolve it, neither is it colored by any of the ordinary tests, and test paper gives neutral results. The general tests for alkaloids, in alcohol solutions, give negative results.

5th. Tannic acid is found, of the variety which gives a brownish-green precipitate with the salts of iron.

6th. Oxalic Acid.

7th. Malic Acid.

7th. A yellow-colored fat oil.

9th. A volatile oil not very abundant, having the odor of the bark.

10th. Wax.

11th. A considerable quantity of starch.

Within the past few years the chemistry of the Rhamnaceæ has considerably increased in interest. The European buckthorn or Rhamnus Frangula was reported in 1876, by both Liebermann and Waldstein, to contain emodin, a well determined constituent of Rhubarb, allied to chrysophane, and chemically a derivative of anthracene. These same investigators further find it nearly certain that frangula is capable of ready change into emodin by the glucosic fermentation. We know that Rhamnus Frangula is both purgative and emetic when first gathered, and may retain these double properties for some considerable time; but after two years storing only the purgative property remains. Emodin is a constituent of rhubarb. The glucosic fermentation which changes frangulin into emodin doubtless gives a chemical explanation of the medicinal powers of the bark. The Rhamnus Purshiana, though not identical with the Rhamnus Frangula, yet show certain chemical reactions which are quite similar. The different species of the Rhamnaceæ doubtless contain bodies closely allied, but the medicinal qualities seem quite distinct; these qualities may be determined only by physiological investigation and therapeutic trial.

In its therapeutic application, Rhamnus Purshiana belongs to the tonic astringent and resin-bearing purgative class of evacuants. The taste is nauseous, bitter, and with rather a disagreeable odor; produces in the stomach a sense of warmth, with, perhaps, in some cases, flatulence and griping pains, these latter conditions not altogether common, and may be prevented by combination with aromatics. In very small doses I have known it to excite the appetite and promote digestion, the tannic acid it contains acting as an astringent, diminishing peristalsis. In sufficient doses a laxative; if the dose be increased a purgative effect will follow its administration, producing stools of an almost invariable brownish yellow, shading off into a light orange. From this unvarying color we may reasonably assume that the drug possesses the power of increasing the flow of bile, and therefore may be placed among the cholagogue medicines,

Digitized by Google

although investigators thus far do not fully concur in this. That the intestinal secretions are promoted, the glandular appendages of the mucous membrane stimulated, and the tone of the muscular coat raised, there can be no question; as all resin-bearing purgatives produce these effects, and in the case of the medicine under consideration, is doubtless due to one or more of the already enumerated resins.

As a gentle laxative I have found this an excellent medicine, and when given in small doses, seems to stimulate the intestinal movement without noticeably augmenting its secretions, other than a natural increase of the proper secretion of the glands, and not merely an osmosis of fluid from the vessels. In larger doses the effects following its administration are decidedly purgative, as is evinced by the hastened peristaltic action, increased secretion, and loose and watery stools.

It is in chronic or habitual constipation, however, that the medicine proved most useful in my hands; and for overcoming this most obstinate and troublesome disorder, to my mind, few medicines equal it. The fluid extract is the most eligible, as well as the most reliable preparation of the drug. Of this, I usually give from five to fifteen minims night and morning; always commencing with the minimum dose, and gradually increasing until the stools are softened, when the dose may slowly be diminished; strongly impressing upon the mind the necessity for perseverance in the use of the medicine, even though a period of months may be requisite in order to produce a cure.

I may be permitted to strengthen my own conclusions by quoting from the following reports: Dr. Russell, Professor of Materia Medica, Yale Medical College, says: "I do not think that its effects are diminished by constant use. I believe that in doses of from fifteen to thirty drops (fluid extract) given three times daily, it is an admirable remedy for habitual constipation. The stools produced by moderate doses of it are so copious, soft and unirritating, that it must be well suited to cases afflicted by constipation with hemorrhoids, fissure of the anus, etc. It is a remedy of great value, and should occupy a prominent position in our Materia Medica."

In the *British Medical Journal* of April 11th, 1885, is published an analysis of thirty-three cases of habitual constipation treated by Rhamnus Purshiana, being a paper read before the British Medical Association by Dr. Reid, of London. The conclusions arrived at were as follows: "1st. Cascara Sagrada, or Rhamnus Purshiana, was a most useful remedy, both regarding its immediate effects and after results in obstinate and chronic cases of constipation. 2d. It was better to prescribe it in continuous small doses, rather than in occasional large ones. 3d. Cases were met with in which, even in large doses, at any rate in the form of the fluid extract, the drug had not been of service. 4th. No rule could be laid down by which one could ascertain previously whether the drug would suit or not; but when pain was produced, in all probability it was owing to too large a dose being given. 5th. It was of great service when other aperients failed."

Prof. H. Senator, in the *Berliner Klinische Wochenschrift*, January 5th, 1885, says: "Its action is prompt and harmless, even after a protracted use. It is scarcely sufficiently bitter to call for a flavoring vehicle. Its proper place in the pharmacopœia is between rhubarb and senna, but it has the advantage of acting in smaller doses than either of them."

It is, perhaps, needless for me to say that chronic or habitual constipation is produced by a great variety of causes. The diet may be largely at fault; there may be a deficiency or a faulty composition of the intestinal juices; the glands which pour out their secretion into the intestines may be impaired; there may be some nervous influence, or some mechanical pressure, or possibly, the presence of poisonous bodies, such as lead, for example, any one of which may impair the bowel to such an extent as to interfere with its requisite propelling power. Absence of or insufficient physical exercise; impoverished blood, anæmia, and general nervous debility; absorbing mental

Digitized by Google

occupation; a neglect to respond to the calls of nature; excessive urination; profuse perspiration, or other discharges of the fluids of the body in undue amount; atonic dyspepsia, or various organic diseases of the stomach and bowels. Now, to say that any one medicine is going to relieve constipation due to all these several causes, without in the least attempting the removal of the cause, is simply absurd. A proper attention must, as a matter of course, be directed to the cause, and its removal insisted upon whenever possible, or when that is impossible, to be alleviated with other remedies in so far as we may be able.

The pharmaceutical preparations are—

1. An extract; Extractum Rhamni Purshianæ, is a solid or inspissated extract, and represents in three grains of the extract ten grains of the drug. The dose of this as a laxative is one to three grains; as a purgative three to eight grains; and is best given in the form of a pill or in capsules.

2. A powdered extract is prepared of the same strength as the solid or inspissated extract. It is nothing more than the solid extract evaporated, at a low temperature, to dryness; is then powdered, and the quantity of moisture lost by evaporation is replaced by its weight of powdered bark. The dose is the same as the solid extract, and may be given either in the powder itself, or in pill or capsule.

3. Pills, or Pilulæ Rhamni Purshianæ; each pill contains two grains of the solid extract, with sufficient dry excipient to obtain a pill mass of proper consistency. About seven grains of the true bark are represented in each pill.

4. Capsules, or Capsulæ Rhamni Purshianæ, contain each three grains of the solid extract evaporated down to dryness, and mixed with sufficient castor oil to make ten minims. The castor oil has been added for the purpose of preventing the powdered extract from absorbing moisture from the capsule hull and from caking together. The inspissated extract cannot be encapsuled, because it contains moisture which would attack the hull of the capsule and cause it to collapse or to leak. Only oily substances are encapsuled in soluble elastic capsules, or are used as a vehicle in which to incorporate medicinal substances.

5. A fluid extract, Extractum Rhamni Purshianæ Fluidum, represents in one cubic centimeter of the extract one gram of the drug. It has an average of from twenty to twenty-two per cent. of extractive, and contains in a hundred parts thirty-three and a third parts alcohol. The dose, as a laxative, is from three to fifteen minims, two or three times a day; as a purgative, from twenty to forty-five minims.

6. Elixir Rhamni Purshianæ, or as commonly known, Cascara Cordial, is prepared by Parke, Davis and Co., of Detroit, Michigan, and is an agreeable and pleasant combination of cascara and berberis aquifolium barks, together with solvents and aromatics. This preparation I have used to some extent and have usually found it quite reliable and always palatable. The dose is from half a teaspoonful to a teaspoonful, two or three times a day.

For the microscopical structure of the bark, and its chemical composition, I am largely indebted to Dr. Prescott, Professor of Organic and applied Chemistry in the University of Michigan.

DISCUSSION.

Dr. Murrell.—These preparations are familiar to us in England, where this bark is officinal. I do not think that there is any special virtue in it over Buckthorn. It used to be given as a cathartic to dogs.

Dr. Phillips.—I have used it in treating constipation, and consider that it has great and many advantages over senna and rhubarb. It differs from senna in having a more powerful cholagogue effect; and, secondly, it causes less watery secretion to be poured out into the small intestine, and is less griping. It is effective and reliable.

Digitized by Google

It differs from rhubarb in being more stimulating to the hepatic cells, and not so astringent, and, therefore, less liable to produce secondary constipation. From my experience, I am much pleased with it.

Dr. WOODBURY.—In cases of hemorrhoids with constipation, I have noticed that with the correction of the torpid condition of the intestinal walls, the hemorrhoids gave no further trouble. This is a great boon to women who frequently suffer in this way.

Dr. RALPH STOCKMAN read a paper—

ON THE PHARMACOLOGY OF SOME BODIES DERIVED FROM MORPHIA.

SUR LA PHARMACOLOGIE DE QUELQUES CORPS DÉRIVÉS DE LA MORPHINE.

ÜBER DIE PHARMAKOLOGIE EINIGER DERIVATE DES MORPHINS.

BY D. B. DOTT, F.R.S.E., AND RALPH STOCKMAN, M.D.

The relationship between chemical constitution and physiological action must always be a subject of deep interest and importance to the pharmacologist. In this short paper I propose to lay before you, in a very brief manner, the results of an investigation which has been made into the action of a few of the substitution products of morphia. Owing to the very large number of such bodies which it is possible to produce, the present communication partakes more of the nature of a preliminary note than of a completed investigation.

In 1854, How, by acting on morphia with iodide of ethyl, obtained a body which he named hydriodate of ethylmorphia

$$\left(C_{17}\overset{H_{18}}{C_2H_5}NO_3HI\right),$$

and which he regarded as a substitution product, the ethyl being supposed to replace one atom of hydrogen in morphia. He also formed iodide of methylmorphia, and both these bodies he took to be substitution products. In the light of our present chemical knowledge we regard them as addition, and not as substitution, compounds.

In 1869 Crum, Brown and Fraser published a research on the physiological action of these substances, in which they showed that the original action of the alkaloid is quite lost, and a curara-like action substituted in its place. From the nomenclature which these observers have adopted they make it clear that they regard such bodies as addition products—that, in fact, the iodide of methyl is simply added to the morphia molecule and does not replace a hydrogen atom in it. Notwithstanding this, we find that, in all text-books and reference books which treat of the subject, the bodies which these investigators used are universally regarded as substitution products, and it is at present generally held that the substitution of a hydrogen atom in an alkaloid by such a body as methyl causes a curara-like action, no matter what the original action of the pure alkaloid may be.

It appears to us, indeed, that morphia methyliodide and similar bodies are not to be regarded as morphine compounds, but rather as salts of a new base. That such bodies frequently give the same color reactions as the alkaloids themselves proves nothing more than that both contain some common nucleus. They are, therefore, entirely different from the bodies formed by replacing one or more hydrogen atoms in an alkaloid by radicals such as methyl (CH_3), ethyl (C_2H_5) or acetyl (C_2H_3O).

The formula of morphia is $C_{17}H_{19}NO_3$, or, as it is more frequently written now,

Digitized by Google

$C_{17}H_{17}NO(OH)_2$, containing, therefore, two molecules of hydroxyl. It has been repeatedly shown that it is the hydroxyl hydrogen atoms which are most easily replaced by different radicals.

With regard to the action of morphia, it may be regarded as causing symptoms readily divisible into two stages. In frogs, when 0.030–0.050 grm. are given, there is produced a condition of narcosis which is soon followed by greatly increased reflex excitability.

In mammalia the same symptoms may be observed with appropriate doses, but the narcotic effects are much more pronounced than the tetanic.

The first body which we shall consider is methylmorphia (codeine) $C_{17}H_{18}(CH_3)$-NO_3. It has been known for many years that codeine extracted from opium is to be regarded chemically as methylmorphia, but, in spite of several attempts to prepare it artificially from morphia, success was only obtained by Grimaux, in 1881. By acting on morphinate of sodium with methylchloride he obtained a body which he showed to be chemically identical with codeine. We prepared methylmorphia in practically the same manner as Grimaux, and found that its action is similar to that of natural codeine obtained directly from opium. Thus, frogs, with a small dose, such as 0.005–0.025 grm. given hypodermically in solution, show symptoms of narcosis, passing, after a varying but always comparatively short time, into a condition of greatly increased reflex excitability. With larger doses the narcotic period was much shortened, and tetanus supervened in a few minutes. The central nervous system only is affected.

In rabbits 0.01–0.02 grm. caused well-marked but not very deep narcosis. On increasing the dose the reflexes were found to become greatly exaggerated, while with a further increase tetanus was speedily induced, and death, in convulsions. In dogs, while narcosis was readily enough induced by small doses, the accompanying disagreeable symptoms which are so often seen with morphia—salivation, vomiting and diarrhœa—are much more marked than in the case of the latter alkaloid.

Ethylmorphia, $C_{17}H_{18}(C_2H_5)NO_3$. The action of this body has been briefly described by Bochefontaine (*Compt. Rend.*, 1881). Owing, however, to his having used too large doses, he has failed to observe that, like codeine, it produces a preliminary narcotic effect. He describes its action as purely tetanic. Our observations show, however, that its action on animals is similar in every respect to that of methylmorphia, and so far as physiological effect is concerned it is immaterial whether methyl or ethyl replace the hydrogen atom in morphine.

For the sake of brevity we omit the details of our experiments at present.

As regards the preparation of this body, the method was analogous to that used for the preparation of methylmorphia, and we ascertained by analysis that it was pure. In our experiments we used the hydrochlorate, a crystalline salt.

Acetylmorphia $C_{17}H_{18}(C_2H_3O)NO_3$. This body was first prepared by Wright, and we prepared in the same manner as he has done, satisfying ourselves as to the purity of the body which we obtained. The introduction of æthyl into morphia alters its action very slightly qualitatively, but quantitatively both the narcotic and tetanizing actions are induced by smaller doses than is the case with morphia. Thus, with 0.010–0.015 grm. frogs exhibit perfectly the two stages, a similar dose of morphia being quite insufficient to produce characteristic effects, beyond slight drowsiness. With 0.020 grm. tetanus supervenes in a few minutes, and this always proves a fatal dose. In rabbits 0.001 grm. is sufficient to produce slight lethargy, lasting for two or three hours. With such a dose the rate of respiration falls from about 120 per minute to 12–36 per minute, the heart remaining unaffected. Larger doses deepen and prolong the narcosis. 0.10 grm. causes very great increase in the reflexes, while a slightly larger dose produces convulsions and death. In dogs, as with methylmorphia, a good deal of distress is seen along with the narcosis.

Digitized by Google

Diacetylmorphia, $C_{17}H_{17}(C_2H_3O)_2NO_3$, was first prepared and described by Wright. In it two atoms of hydroxyl hydrogen are replaced by two molecules of acetyl. An account of its action would simply be a repetition of what we have said about acetylmorphia. In this case, therefore, the action remains the same whether one or both hydrogen atoms are replaced.

Dimethylmorphia $C_{17}H_{17}(C_2H_3)_2NO_3$. This base was prepared by warming together in alcohol equivalent quantities of morphia, soda, and methyliodide; then on cooling additional equivalents of soda and methyliodide were introduced and the solution again digested on the water bath. The moisture was evaporated to get rid of the alcohol, and the alkaloid extracted with chloroform. The hydrochlorate was the salt used for experiment.

The preparation, purification and analysis of these substitution compounds presents peculiar difficulties, as one has to deal not only with ordinary impurities, but with the risk of the presence of isomers. For instance, methylmorphia methochloride, dimethylmorphia hydrochloride and methylmorphineëthine hydrochloride, have all the same molecular weight. It is impossible, therefore, to distinguish one from the other by ultimate analysis or by forming platinum or silver compounds. We took, in this case, special pains to ensure the purity of our salt, but for the present we are hardly in a position to state definitely which hydrogen atom in morphia is replaced by the second molecule of methyl introduced—whether it is one of the hydroxyl hydrogens, or one of the hydrogen atoms in the morphine nucleus. We have named it at present dimethylmorphia. Its action differs so materially from that of diacetylmorphia, that it arouses the suspicion very thoroughly that the same atoms of hydrogen are not replaced in both cases.

With this body, the symptoms which characterize the morphia action are profoundly altered, so much so that points of similarity are difficult to find. In frogs, five milligrams caused slight depression, which soon passed off, the animal passing again into its normal condition. When 0.010–0.025 grm. were given, the animal showed symptoms of extreme depression, lost its power of jumping, and ceased to respond to external stimuli. The motor nerves remain unaffected, but the brain and spinal cord are much depressed. The most prominent effect, however, was poisoning of the muscles. At the point of injection the muscles passed into a state of rigor mortis almost at once, and this condition gradually extends all over the body, death resulting from poisoning of the whole muscular system. The action of this body, however, is not comparable to that of digitalin, but rather to that of saponin. The heart is slowed and depressed, and does not pass through the characteristic stages of the digitalis heart.

In rabbits, no narcosis was produced even with large doses. In one case, 0.05 grm. caused only slight depression and slowing of the respiration.

In another case, 0.5 grm. caused great depression and death in eighteen minutes. The cause of death was failure of respiration, due to poisoning of the thoracic muscles, the substance having been injected hypodermically over the thoracic region.

In conclusion, I must express my regrets that we are able to report on so few of these bodies. We have now a large number prepared, but owing to the great difficulty of obtaining them quite pure, we have not yet made experiments as regards their action. It will be inferred from our description that none of these bodies are to be preferred in practice to morphia itself. They are all (with the exception of dimethylmorphia) more poisonous than morphia, and possess in a much higher degree the power of causing disagreeable symptoms. As regards dimethylmorphia there seem to be no indications for its use clinically.

Digitized by Google

DISCUSSION.

Dr. HULL.—Is there any assignable cause to explain why the acetyl morphine is so much more active than the methyl compound?

Dr. STOCKMAN.—The present investigation may throw some light upon this point hereafter; but at present I am unable to answer this question.

Dr. WILLIAM MURRELL.—There are only two members of this group that I have had any experience with, apocodeine and apomorphine. The latter was discovered by Malthison, and its action was investigated by Dr. Lee, of London, who recommended it as the safest and best agent for an emetic. It did not come into use as a therapeutic agent in England until about five years ago; since then it has been extensively employed, and was introduced into the British Pharmacopœia at the last revision. A special two per cent. solution was recommended for hypodermic use. The preparation is a very good one for this purpose, but the directions for the extemporaneous preparation of the solution, given in the British Pharmacopœia, to have it freshly prepared, are simply puerile. It gets darker in color on standing, but the physical change does not interfere with its physiological effects. I have kept a solution on hand for months, and have been unable to detect any difference in its activity; it acts the same in identical doses as the fresh solution does. I have had the same preparation in my poison antidote-bag for four years, and nearly every case of opium poisoning gets some of it, and it acts very satisfactory yet.

The other agent I have used is apocodeine, but I have not been satisfied with it. It is much slower than apomorphine. I therefore went back to the apomorphine. I do not regard them as identical in their action, although they are in preparation. Apocodeine is commonly made from morphine, and apomorphine from ethylmorphine or codeine. There is much difference of opinion as to the value of codeine as a therapeutic agent. On the Continent codeine is much used in diabetes. I am not satisfied that it possesses advantages over morphine in this disease.

Dr. STOCKMAN.—Codeine is given in doses as high as three grains in diabetes, and in smaller doses in cough mixtures. Morphine is usually given in smaller doses. Theoretically, the conditions should be reversed: codeine is more poisonous, and, therefore, should be given in smaller doses. It is my opinion that none of these substitutes for morphine should be used. In former times morphine was given in diabetes, now it is the fashion to give codeine. Morphine is much better in cough mixtures, and from the pharmacology of these compounds, I would say that morphine is better in every way than its derivatives.

Dr. BRACKETT.—I found that, clinically, the codeine is better accepted by the stomach than morphine. I was surprised to hear that codeine is more violent in its action; and especially, to learn that it produces vomiting. My own preference is for the hydrochlorate of codeine for all the purposes to which this drug has been applied.

Digitized by Google

THE ANTISEPTIC ACTION OF CALOMEL IN LARGE DOSES.

L'ACTION ANTISEPTIQUE DU CALOMEL À GRANDES DOSES.

DIE ANTISEPTISCHE WIRKUNG DES KALOMEL IN GROSSEN GABEN.

BY GEORGE S. HULL, M. D.,
Chambersburg, Pa.

The gradual return to the use of large doses of calomel in the treatment of diseases of the alimentary canal which are peculiar to the hot weather of summer and mostly due to fermentation, seems to warrant a study into the special action of this favorite salt of mercury in these disorders.

Years ago heroic doses of calomel were lauded in the treatment of cholera, dysentery and like affections, but the dread of these large doses led to a reaction in favor of small and frequently repeated ones; and to such an extent has this been carried, that many physicians of the present day make use of granules of from one hundredth to one-tenth of a grain, and think a ten-grain dose a relic of barbarism.

Lately, however, the great advance in the study of bacteriology, and the consequent search for germicides among medicines, has led to a renewed interest in the salts of mercury, among which stands the most powerful germicide yet discovered, the corrosive chloride, the merits and uses of which are so well known.

In this examination of the mercurials, the mild chloride has not been neglected, and as a result we are beginning to hear again of doses of calomel ranging from a scruple to a drachm, being given in summer diseases of the intestinal canal; and the claim is that it cures them by virtue of its antiseptic action.

The object of this paper is to inquire into the so-called antiseptic action of large doses of calomel—the considerable and successful use of it in such doses in an epidemic of dysentery having aroused a personal interest in the matter.

Does calomel act as an antiseptic *per se*, or by virtue of its power, in disease, of causing a large flow of bile, which fluid exerts the antiseptic action? I shall not consume time in a discussion of the manner in which calomel brings about its peculiar effects upon the system, but only glance at several theories as bearing upon the question of its antiseptic action. Almost forty years ago, Sir Henry Holland said, in a lecture, that any remarks "on the principle or method of using calomel must be trite and altogether needless;" and to-day there is but little ground for anything novel in this direction, as we can boast of but little additional knowledge of the precise way in which calomel acts upon the liver in disease—I say disease, as it is even claimed that calomel diminishes the flow of bile in health.

The most likely theories of the action of calomel are those of Mailhe and Jeannel. The former claimed that calomel is converted into corrosive sublimate by the chlorides in the stomach, and, as such, absorbed and made active. Against this we have the fact that calomel, to be acted upon by the chlorides in the gastric juice, must have a much higher temperature than the stomach affords, and much more time than is occupied in digestion to form even minute quantities of corrosive sublimate. Also, the action of calomel upon the system is quite different from that of the corrosive chloride, resembling more the action of blue mass. And yet when chlorides are abundant in the stomach, as when salt meats are freely used, or when brackish water is drunk, as is sometimes the case on the sea coast, there is, undoubtedly, increased danger of salivation. The theory advanced by Jeannel, and spoken of favorably by some authorities, seems to be the most plausible. The mercurous chloride, in the presence of an alkaline carbonate, is decomposed and mercurous oxide formed. This gray oxide is somewhat soluble in water, but if a fatty oil be present, the solubility is very much increased.

Digitized by Google

Now, as the juices of the intestine are alkaline, and as fatty matters are always present, it is supposed that the calomel, after passing through the stomach unchanged, is decomposed and dissolved to a sufficient extent by the intestinal juice. However, as Prof. H. C. Wood says: "The varying constitution of the alimentary juices and the complex chemical relations of calomel, would indicate that its solution in the alimentary canal is accomplished in more ways than one." The excess of chlorides may cause a slight portion of the calomel to be converted into corrosive sublimate, and the presence of sulphureted hydrogen may lead to the formation of a soluble sulphide. Certain it is that it is not judicious to administer large doses of calomel where salt foods are consumed largely, and may it not be probable that the reason calomel acts so promptly, and often violently, when the contents of the bowels are in a fermented state and charged with sulphureted hydrogen, is that the mercurous chloride reacts with the hydrogen sulphide, forming the soluble mercurous sulphide, which is readily absorbed? I have noticed that calomel is apt to act very promptly and effectively when there were eructations of this offensive gas, but have always reasoned that this state of affairs showed a badly engorged state of the liver, and that the calomel found a large amount of bile to be liberated, and hence the free purgation. May not the very presence of the sulphureted hydrogen be the reason why the mercurial is so promptly absorbed and its effects so quickly manifested?

Then we have the additional and important fact that calomel is soluble in the bile, pointed out by no less an authority than Headland; and this may assist in accounting for the reason why calomel purges most powerfully when the dose is repeated at short intervals; the first dose releases some of the bile, and the following ones, being the more readily dissolved, owing to the presence of the bile, are quickly absorbed, thus keeping up and intensifying the purgative action. It is the practice of many physicians, in order to increase the purgative effect, to associate a fair amount of sodium bicarbonate with calomel, and here we have the alkaline carbonate ready to reduce the mercurous chloride and permit of its being dissolved, even in the stomach, providing its contents are not too acid, and contain fatty matter. The familiar and useful compound cathartic pill of our Pharmacopœia contains, with the calomel, alkaline matter, in the form of soap; so do other cathartic mercurial pills. Before proceeding further and attempting to show in just what manner calomel may act as an antiseptic, it may be well to introduce some clinical evidence in support of this claim. Accordingly, I will relate briefly several cases, chosen from a number in a recent epidemic of dysentery at Chambersburg, Pa.; and also will supplement them with extracts from reported cases of this and allied diseases, in which calomel was given in large doses.

During the summer of 1886, dysentery became epidemic in Chambersburg and vicinity, the total number of cases being estimated at about 800. After but indifferent success with the usual modes of treatment, I became very much impressed with the treatment by calomel in large doses. The very first case in which it was given was such a surprise to me that I determined to give it a faithful trial thereafter, and have done so. The case just mentioned was briefly as follows: Henry H., a lad of eighteen years, had been ill for several days before being seen; his dysenteric symptoms were unusually severe, and in spite of the usual treatment (castor oil, followed by opiates and astringents by mouth and rectum,) he grew worse. On the seventh night he had between twenty and thirty passages of bloody, gelatinous matter, the tormina being very severe and uncontrollable. In the morning he was in a decided typhoid condition; pulse 130, temperature 104°, tongue dry, hard and brown, limbs drawn up in bed, body already very much emaciated, face sunken, intellect dull. In this emergency I determined to give the calomel treatment a test, so emptied the contents of the vial in my pocket case (containing nearly a drachm) upon his tongue and gave him frequent small amounts of water until it had completely disappeared. In about six hours I returned

Digitized by Google

and found the vessel about half full of an olive-green fluid, about the consistence of thick paint. His tormina had left him and he was disposed to sleep; his bowels were quiet and there seemed a marked change for the better in every respect. He made a good recovery after this, without the use of either opiates or astringents.

In another case, a lady of thirty-five years, who had been ill for several weeks before I saw her, and was delirious, and as I thought, near death, a similar dose was given. A large amount of the olive-green fluid was discharged from her, after which the blood disappeared from the passages and signs of betterment set in, although the irritated condition of the bowels demanded the use of opiates for a few days.

In quite a number of cases seen very early in the disease, and in which twenty grains of calomel were given at once, the symptoms were very much modified, and in some cases the disease terminated abruptly.

After some experience with this mode of treatment I became confident of a recovery when calomel brought away the characteristic bilious discharge, and very rarely did it fail to do so; never, to my recollection, in adults. In several cases where there was a tendency to relapse, the dose was repeated, with good results.

Numerous similar cases could be quoted from the experience of others, but I will only refer to one; that of Dr. James Johnson, who, in his day, strongly advocated the use of these large doses. His description of his own symptoms, showing the relief he obtained from scruple doses of calomel, is indeed graphic. He contracted dysentery while hunting on the banks of the Ganges. He was taking, under advice, small, repeated doses of calomel, with the hope that ptyalism would alleviate his sufferings. Growing worse and fearing a fatal termination, he took, on his own responsibility, one scruple of calomel, and fancying it rather lulled the tormina, he, after a number of hours, repeated the dose. He then fell asleep, and on awaking was so free from pain that he feared it was a dream. To quote his own language, he says: "My skin was covered with a warm moisture, and I lay for some considerable time without moving a voluntary muscle, doubtful whether my feelings and senses did not deceive me. I now felt an uneasiness in my bowels and a call to stool. Alas, thought I, my miseries are not yet over! I wrapped myself up, to prevent a chill, and was most agreeably surprised to find that, with little or no griping, I passed a copious, feculent, bilious stool, succeeded by such agreeable sensations—acquisition of strength and elevation of spirits—that I ejaculated aloud the most sincere and heartfelt tribute of gratitude to Heaven for my deliverance."

To add still more weight to the prognostic value of the appearance of bile in the stools of dysenteric patients, I might quote from Dujardin Beaumetz, who in one of his lectures says: "You may regard your patient as cured when once bile appears anew in the stools."

In looking over the modes of action of the drugs which are the most successful in the treatment of dysentery, it is noticeable that they all either aim at releasing the bile or at depleting the portal circulation by causing a flow of serum from the numerous veins composing it. Of the former, calomel and ipecac are examples; of the latter, the salines.

The rationale of the calomel treatment in dysentery, where we have a loaded portal circulation, brought about by some specific poison, and followed by discharges of mucus from the irritated bowel, which irritation increases until blood tinges the stools and finally tissue breaks down and ulceration results—the rationale of this treatment seems to be that it unloads promptly this congested circulation, by removing the obstruction; *i. e.*, by relieving the engorged liver of its bile; and the reason for giving a large dose in preference to small repeated doses, would seem to be as follows: So much of the large dose reaches the alkaline small intestine, that after sufficient of it is dissolved to excite the flow of bile, there still remains a quantity of it to be dissolved by the bile, and this

Digitized by Google

solution, which has increased antiseptic powers, flowing down the canal, after having led to a depletion of the engorged portal circulation and thus given relief to the congested mucous membrane, has a twofold action: First, by virtue of its purgative action it sweeps from the bowels offending matter, and in the second place, and where it excells other cholagogues and purgatives, it directly (by virtue of its antiseptic properties, due, in a large measure, to the mercury salt in solution,) destroys the infectious matter, *i. e.*, acts as a germicide.

In the ordinary summer diarrhœas, where fermentation plays a part, this happy action is manifested to a great advantage; even in cholera I can well believe it is capable of doing good, and possibly, when given early, is able to cut short, or at least to mitigate the disease, by virtue of this antiseptic action.

We have on record various and opposed opinions concerning its use in cholera, as it has been given in the smallest doses, and in such enormous quantities that one pound has been administered in a single case. In this rapid and formidable disease there is not the same latitude for giving it as in dysentery, and unless its action is manifested in time to destroy and eliminate from the intestinal canal the bacillus of this disease, we cannot place much reliance upon the administration of it, as the absorbed poison is hardly likely to be influenced by any remedial property calomel possesses.

Dr. Vandeveer, in his report of the Franklin Street Cholera Hospital, in the epidemic of 1854, states that he gave thirty-five to forty grains of calomel in the first stages of confirmed cholera, which, if rejected by the stomach, was repeated every five minutes until sufficient was retained. Even in the stage of collapse he gave as much as sixty grains in the same manner.

With reference to purgation being brought about by its use, he says: "I have never met a case of cholera treated as above stated where hypercatharsis ensued; on the contrary, it has frequently occurred that patients, both in hospital and private practice, have been seized with violent vomiting, purging and cramps, which had, from their own statement, been kept up, every ten minutes, for one, two, or three hours, and, upon taking sixty grains of calomel have not vomited for six, ten, or twelve hours after; and in two or three instances, in the hospital, after waiting twelve hours, mild enemata were resorted to in order to open the bowels." His success was such, that even one-third of those cases described as in "profound collapse" when admitted were saved. Dr. Ayre gave in three days 580 grains, without producing any soreness of the gums, and his patient recovered. Dr. Sternberg says that calomel given with a view to the formation of corrosive sublimate in the stomach and its germicidal action upon the bacillus, is a very unscientific procedure, because of the uncertainty of the formation of the corrosive sublimate, and if any should be accidentally formed, the smallness of the amount would be of little avail. This may be readily granted, as there is, under any circumstances, but small likelihood of the formation of a sufficient amount of corrosive sublimate in the stomach to disinfect the whole intestinal canal without previously doing much, and perhaps greater, damage by virtue of its own poisonous action. Now, if calomel, after some decomposition, is dissolved in the bile, as stated by Headland, may not the early use of large doses of this tasteless powder, which is far more likely than any other cholagogue to remain in a sensitive stomach, may not the administration of it before collapse has set in, and while the liver can respond to its action, cause a flow of bile which will dissolve some of the excess, and passing through the intestine act as a germicide by virtue of the mercurial salt held in solution? I put this as a question which has constantly come up before my mind, and it seems to me an answer in the affirmative could be reasonably hoped for. Certainly, but little objection can be urged against the use of these large doses, when it has been the experience of all who have used them that the danger of salivation is almost nothing. And as regards the poisonous action

Digitized by Google

of calomel, it does not seem to be actively poisonous in any single doses. And besides its probable germicidal action, it possesses, as claimed for it by several writers, a decidedly sedative action, frequently bringing on sleep and even quieting pain. This sedative effect, which I have seen decidedly manifested in some cases, is spoken highly of by Vandeveer; also in the case of Dr. Johnson, as related above, he emphasizes this result of its action; and in an excellent paper by Dr. T. D. Lente, in the *New York Medical Journal*, on the "Sedative Action of Calomel," he speaks enthusiastically of its action in this respect, even in such diseases as croup and pneumonia. To sum up the matter in a few words, if we can both promote the flow of bile and increase its antiseptic properties by large doses of calomel, should we not give this matter our careful consideration, and see how it will stand in the light of clinical evidence.

DISCUSSION.

Dr. JULIUS GNEZDA, of Berlin.—Has the lecturer used large doses of calomel in hepatitis, or commencing cirrhosis of the liver?

Dr. HULL.—I have never used large doses in such cases. I might add to what I have said, that in none of the cases treated did there follow abscess of the liver or any of the other troublesome and serious sequelæ of dysentery. This was probably averted by the prompt action of the calomel treatment.

Dr. GREEN.—Large doses of calomel are not given as frequently by physicians as formerly; and it is a question if the most of it does not pass through without producing any effect whatever. I prefer to give fractional doses.

Dr. BRACKETT.—Large doses of calomel in the early stage of typhoid fever are used in Germany. From personal trial, I was not satisfied with the doses even as large as ten grains, and have gone back to small doses.

Dr. UPSHUR.—I began many years ago to use calomel in large doses, but the tendency of late years has been to give small doses. When I desire action upon the liver I give one-fourth or one-fifth of a grain of calomel every hour or two, and after four or five doses I obtain copious bilious stools. The practice in Richmond to-day is averse to giving large doses in dystentery, and we get just as good results from other purgatives. The benefit from the calomel is not due to antiseptic action, but it relieves by removing irritating contents from the bowels. I believe that if the lecturer had given teaspoonful doses of sulphate of magnesia, and then followed up with small doses of morphine, he would have had equally good results.

Dr. W. M. RAMSEY.—Being from the same locality as the gentleman who read the paper, I am exceedingly anxious to hear this question of large doses of calomel in dysentery discussed by the Section. I have never used over ten or twelve grains at a dose. If the experience of those present confirms that of the reader of the paper, I am willing to test the treatment. Within a few days I have had a gentleman prescribe for a patient of mine, in consultation, one-twentieth of a grain of calomel, and obtain from this small quantity bilious stools within twenty-four hours. You see the great difference in the dose, which gives rise to my dilemma which to use.

Dr. A. M. DUNCAN, of Ohio.—I have for four years past been in the habit of using calomel in dysentery, in from one-half to one-tenth of a grain doses, with good results. I often associate with it, in small doses, ipecacuanha, or use the latter alone; both remedies to be freely repeated as the case requires.

Dr. J. GNEZDA.—Calomel is not used in typhoid fever in the clinic of Prof. Leyden, in Berlin; but it has been tried with more or less success in cases of hepatitis

Digitized by Google

during the last two years. This treatment was recommended by Prof. Sacharin, of Moscow, Russia.

Dr. HULL.—In my paper I reported the results of individual experience in the treatment of dysentery with large doses of calomel. Some of those who complimented me by discussing the paper must have misunderstood my position. I did not say it was the only treatment, nor the best treatment of this disorder; nor did I say that it acted as an antiseptic only in such cases. The title of my paper is the "So-called Antiseptic Action of Calomel," and I should have been pleased, indeed, had the discussion been centred upon this point, or even upon the action and merit of large doses of calomel in the treatment of dysentery.

Digitized by Google

FOURTH DAY.

A paper by Dr. GEORGE ARMSTRONG ATKINSON, of Edinburgh, Scotland, in the absence of the author, was read by Dr. RALPH STOCKMAN, entitled—

THE PHARMACOLOGY OF THE NITRITES AND OF NITROGLYCERINE.

LA PHARMACOLOGIE DES NITRITES ET DE LA NITROGLYCÉRINE.

DIE PHARMAKOLOGIE DER NITRITE UND DES NITRO-GLYCERINS.

BY G. ARMSTRONG ATKINSON, M.D.,

Of Edinburgh, Scotland.

The pharmacology of the nitrites and of nitroglycerine is most conveniently discussed by considering, in the first place, the Pharmacology of the Nitrites, and then the Pharmacology of Nitroglycerine.

THE PHARMACOLOGY OF THE NITRITES.

The essential basis of pharmacological action in this group of medicinal agents is nitrous acid, an acid which is remarkably unstable at ordinary temperatures, especially in the presence of water. By making a watery solution of about 1 in 3000 there is not very rapid deterioration, and such a freshly prepared solution may be used for a couple of days or so. The solution, however, has no advantage over a simple metallic nitrite solution, as of nitrite of sodium, and the experiments carried out with it on myself, on rabbits and frogs, showed its action to be identical with that of a solution of nitrite of sodium, in so far as the action of an acid can be considered identical with one of its salts. Our knowledge of the actions of the nitrite group has chiefly been derived from the effects produced by nitrite of amyl. Since here the base (amyl) has a decided action of its own, it is advisable, in order to ascertain the action of nitrous acid, in combination to form a salt, to select such a nitrite as nitrite of sodium, the base sodium possessing in its combinations no well marked pharmacological activity.

THE PHARMACOLOGY OF NITRITE OF SODIUM.

The literature pertaining to the action of this nitrite is not great. Gamgee, Lauder Brunton, Hay, Leech and others have all recorded their experience of a like similarity of action between this salt and amyl nitrite. Barth (Toxikologische Untersuch. über Chilisalpeter., Bonn, 1879) pointed out its highly poisonous qualities; Binz (*Archiv f. Exper. Path. und Pharmak.*, XIII, 183) gave some experiments as to its general action on dogs, rabbits and frogs, showing that it produced death from a general paralysis, especially of the muscular system, no convulsions preceding the fatal issue. Reichert (*American Journal of Medical Sciences*, vol. 80, p. 158) states he found its action, so far as he investigated it, identical with that of nitrite of potassium, with which nitrite he made a long series of experiments, which will afterward be referred to. The nitrite of

Digitized by Google

sodium used in the experiments I am about to detail was recrystallized by myself, from commercial specimens of the salt, and contained nitrous acid equal to from 99.2–99.4 per cent. of actual nitrite.

General Action on Frogs.—Almost all the experiments were made on winter and summer specimens of the Rana Temporaria; those made on the Rana Esculenta were merely check experiments and showed no difference. The general action on frogs is well seen in animals to which minimum lethal doses of the nitrite have been administered. For frogs of 25 to 30 grams .15 grain (.00972 gram) is a fatal dose, subcutaneously injected. Such a frog with such a dose, in about two minutes after injection, without any preliminary stage of excitement, leaps less readily and its reflex movements are less vigorous. In ten minutes after injection it jumps feebly and unwillingly, its cutaneous vessels are dilated, their contents are darkened, and respiration is slower. After fifteen minutes the animal sits with its thorax only slightly raised from the table, its respirations are decidedly slowed, the pupils are as large as before the experiment commenced; the lower eyelids are slightly closed. On pinching the legs a feeble reflex movement is produced and the animal crawls away with a kind of walking movement. It cannot jump and cannot turn off its back. In one hour, having gradually become feebler, without any twitchings or convulsive movements, respiration has ceased. The exposed ventricle of the heart is beating slowly, generally about 18 per minute, the auricles usually about twice as fast. The blood is markedly chocolate colored. The strongest stimulation of the exposed sciatics by a du Bois-Reymond induction coil with one Daniel's cell produces only feeble movements of the toes. All skeletal muscles inexcitable except those on inferior aspect of thighs and of legs, which feebly respond to strongest current. Cord on strongest stimulation produces no movement. Ventricle beats for one and a half hours more, auricles for three hours, the heart stopping in full diastole and being quite inexcitable. Tissues and blood (on dialysis) give nitrite reactions, and blood shows the spectroscopic appearances of methæmoglobin. Post-mortem rigidity comes on early; the pupils in it are contracted to pin points and the subcutaneous tissues become somewhat œdematous. Similar doses to larger frogs produced severe symptoms in about 20 to 30 minutes, the symptoms being similar to the above but not proceeding to death. The frog gradually recovers and in two or three hours is almost well. Smaller doses produce slighter but similar effects. With larger doses the various stages occur more rapidly but without any difference in phenomena. A frog weighing 22 grams received subcutaneously three grains of the nitrite. The animal rapidly became profoundly affected as described with the other frogs. In five minutes respiration ceased; in 15 the ventricle and in 18 the auricles stopped beating. The nitrite was ascertained to be readily absorbed from the stomach or the skin of the frog. Strong solutions painted over the legs rapidly killed.

In Rabbits of about three pounds in weight, three grains was found to be a fatal dose. Larger rabbits recovered from this quantity. The phenomena are closely allied to those in the frog. A white rabbit weighing four pounds four ounces received subcutaneously four grains of nitrite of sodium. Animal almost immediately became very prostrate, lying down on its belly with its limbs extended; respirations rapid and shallow, blood vessels dilated and blood of a chocolate color. Symptoms of muscular paralysis rapidly progressed, and in half an hour the animal was very feeble and moribund. It now cried once or twice. Respiration, which had become slow, ceased; the rabbit made slight movements of a dyspnœic character with its forepaws and died.

Immediate post-mortem: Venous system engorged; blood deeply chocolate colored; right ventricle in full diastole; left in moderate systole; auricles occasionally feebly beating; sciatics and muscles inexcitable; all viscera congested. Intestines contracted somewhat less strongly to strong current than in normal rabbit. Dialysed blood gave nitrite reactions. Urine contained trace of nitrite but no sugar and no albumen. In

Digitized by Google

rabbits more slowly poisoned gastric ecchymoses were present and there was a tendency to looseness of the bowel, but no diarrhœa was ever observed. Larger doses than the above produced death very rapidly; from smaller doses recovery after development of the severe symptoms was comparatively rapid.

In Dogs the phenomena are much as in rabbits, except that vomiting usually occurred shortly after exhibition of the poison, even subcutaneously. Large dogs, as of about fifty pounds, required about twenty grains, this dose being fatal in a little more than one hour. Post-mortem rigidity, both in dogs and rabbits, was early in its appearance. In dogs, nitrite reactions are very usually obtained early in the urine; in rabbits, not always, and only when present in very minute traces.

In Man I am unaware of any fatal cases; in myself, after taking on an empty stomach eight grains of nitrite of sodium, in a few minutes I experienced a great tendency to faintness with considerable acceleration of pulse, and great lowering of arterial tension. No sickness occurred, but considerable eructation of oxides of nitrogen. No visible flushing of any part of the body was detectable.

Action on the Individual Systems.—Action on the blood and on the circulatory system. The account given of the general action indicates an important action on the blood and circulatory system. Nitrite of sodium very readily produces methæmoglobin, and in dogs to which a dose of about four grains of nitrite of sodium has been given, methæmoglobin is found for thirty to thirty-six hours. The red blood corpuscles are unaltered, the white are readily paralyzed—by a one per cent. solution in four or five minutes—by a one per mille in fifteen to twenty minutes. The excised heart of the frog is killed by being placed in normal saline containing nitrite of sodium in eight to nine minutes, if a one per cent. solution be used, in about three hours if a one per mille—no preliminary stage of increased rapidity of beat occurs, the heart gradually beating more and more slowly, and finally stopping in full diastole. Directly applied to the heart in situ death was caused by strong solutions, as twenty per cent., in a few minutes; by weak, as one per cent., in an hour or so. Subcutaneously injected the same phenomena occurred, no preliminary increase in rate being present. To ascertain the effect on the heart when the vascular dilatation is not present as a disturbing influence, Williams' well known heart apparatus was employed; with one in 1000 death of the heart in diastole occurred in 10 or 11 minutes without any preliminary rise; with one in 10,000, a slight rise for four or five minutes was observed, followed by a slow fall to death, which occurred in 40 to 60 minutes; with one in 20,000 the rise lasted 10 to 15 minutes, and death did not occur for some hours. The slight rise in blood pressure seemed due to a slight quickening of the heart's action, rather than to an increase in the strength of the individual beat. From experiments with many variations of dose it was ascertained that no stimulating effect on the vagus terminations in the heart, and no paralyzing effect either was induced. Both arteries, veins and capillaries were widened by the drug, as ascertained by a micrometer. The solution of nitrite in normal saline was run through the vessels of a pithed frog, the pressure of the inflowing fluid being maintained at a constant level by means of reservoirs fitted with Mariotte's flasks. Dilatation occurred with all strengths from one in 10,000 to one in 200,000. In mammals great vascular dilatation and great acceleration of the heart beat occur. The ophthalmoscopic appearances are those of slight arterial and venous dilatation with small doses; with large doses this dilatation was rapidly followed by diminution in size. Blood pressure experiments on rabbits showed a steady fall; with no dose was there any preliminary rise in blood pressure ever observed. The heart beat was markedly accelerated. The vagus terminations in the heart are unaffected. When the animal is fully under the nitrite, section of the vagi is still followed by some rise in blood pressure, although the rise is very much less than in the normal animal. Section of the depressor nerves was followed by a fall in blood pressure, probably due to loss of sensory impulses passing from the heart to the

Digitized by Google

accelerating centres in the medulla. When the free carotid was clamped, with the blood pressure very low, the rise was only small. The acceleration in heart beat seems, therefore, to be due to depression of the medullary inhibitory centre, secondary to the fall in blood pressure, which is almost entirely due to vascular dilatation, the heart muscle being only affected in advanced poisoning.

On Respiration the effects are to cause increased rapidity followed by slowing and finally by stoppage before the heart ceases. The nitrite, by producing methæmoglobin, and by lowering the blood pressure, causes the increase. The paralysis of respiration is due to the effect of the nitrite on the muscular system chiefly, but also in part to the effect on the medullary centre.

The temperature falls, a slight rise occurs at the beginning, with the thermometer in the rectum, due to vascular dilatation.

Striped muscle is rapidly paralyzed, non-striped more slowly; in the rabbit non-striped, as of the intestine, is only paralyzed by a one per thousand solution in four hours; by a one per hundred in about half an hour.

Motor and Sensory nerves are almost, if not quite, unaffected, even with lethal doses, as to their peripheral terminations. Their trunks are also probably unaffected, unless the nitrite be directly applied. The *brain* and *cord* are both paralyzed without any previous excitation, unless saturated or very strong solutions be directly applied. Certainly in the uninjured animal no such stimulation occurs.

On the urine small doses slightly increase the flow; large always diminish it here if the arterial tension be previously raised by means of digitalis. The urea and uric acid are almost unaffected. Sugar appears in the urine of rabbits after some hours, if the animal be kept very decidedly under the influence of the drug, but rapidly disappears if the administration of the drug be stopped. The nitrite is largely destroyed in the system, being partly, however, excreted as nitrate, partly, probably, as urea. A portion of it is excreted as nitrite.

THE PHARMACOLOGY OF THE OTHER NITRITES

is briefly dismissed.

Nitrite of Potassium is stated by Reichert (op. cit.) to produce restlessness and excitement in frogs, followed by depression and incoördination of voluntary movements, occasionally clonic convulsions or a tetanoid condition being present: Motor and sensory nerves are mentioned as being depressed, and the blood pressure is stated as being primarily increased and then diminished. According to this observer nitrite of sodium would markedly differ from nitrite of potassium in action, and I made, therefore, a prolonged series of observations with the potassium salt. It was found to be practically absolutely identical in action.

Nitrite of Ethyl was used mixed with an equal volume of absolute alcohol. This drug acted much as the sodium salt, but much more rapidly, death after subcutaneous injection occurring in a few minutes, and by inhalation in one or two. The blood is rendered markedly chocolate colored, from methæmoglobin production; the muscles and sciatics are very little affected; slight dyspnœic convulsions occur, and death is apparently due largely to medullary paralysis.

Nitrite of Amyl has been so largely written upon that it requires but little to be said of it. Death with it is partly due to the muscle paralysis, especially if the drug be subcutaneously exhibited, as then very slow absorption takes place, death not usually occurring for many hours. The fatal issue is principally, however, produced by similar causes to those acting with nitrite of ethyl.

THE PHARMACOLOGY OF NITROGLYCERINE.

This body, a trinitrate of glyceryl, as shown by Hay, breaks up in the system with the production of nitrites, hence, therapeutically, it has a nitrite-like action when given

Digitized by Google

to man and mammals by the stomach, subcutaneously or by intra-peritoneal injection; occasionally, with very large doses to rabbits by the stomach, as five or six grams, tetanic convulsions occur. Watery solutions, usually of the strength of one per mille, when subcutaneously injected into frogs, produce a series of phenomena quite different from nitrite of sodium. A frog weighing 20 grams received subcutaneously 30 minims of a 1 per 1000 watery solution of nitroglycerine. In one minute it began to walk slowly about and refused to jump unless strongly irritated. In two minutes it was unwilling to jump at all, and the movements were stiff, with a tendency for the animal to rotate on its haunches. Reflex movements much diminished in three minutes from time of injection. Four minutes after injection it gave a sharp cry, and after a few seconds of very irregular movements pronounced tetanus occurred. The tetanus soon passed off, but was readily induced by stimulating skin until following morning. Animal feeble following day, and did not fully recover until third day after injection.

If smaller doses be given, clonic but not tetanic movements occur. If larger, as 100 minims of the 1 per 1000 solution, the animal dies from the exhaustion produced by the tetanus. After death the post-mortem appearances are like those of nitrite poisoning to some extent, and the blood, on dialysis, gives nitrite reactions.

Portions of the brain were successively removed, and the clonic movements were localized in the medulla, the tonic in the spinal cord. The effects on the other systems of the body were similar to those of nitrite of sodium, being, indeed, due to the production of this salt.

Administered to mammalia by the stomach, death occurred, due to nitrite poisoning, but if intravenously injected, tetanus, preceded by clonic movements invariably, was very readily induced by the injection of a few cc. of a one per mille watery solution into the jugular vein of rabbits. The cause of the non-appearance of tetanus when given by the stomach is the slowness of absorption of sufficient quantity to induce convulsions, as the nitroglycerine is not very soluble in water (1 in 760); and, further, the absorbed ether is rapidly partially decomposed into nitrite. The excretion of nitroglycerine is by the urine, from which, after large medicinal doses, it can easily be extracted by means of ether.

It is obvious, therefore, that in nitroglycerine we have a twofold action, that of nitroglycerine and that of the resulting nitrite. Nitroglycerine, sui generis, acts as a convulsive agent, the nitrite which results from its decomposition as a paralyzing.

(Fuller details of this paper will appear in the *Journal of Anatomy and Physiology* for January, 1888.)

DISCUSSION.

Dr. William Murrell, of London.—During the last ten years I have devoted much attention to the investigation of the physiological action of the nitrites, and especially nitroglycerine. As a result of this work, nitroglycerine has been introduced as a remedy in angina pectoris. Its action is quite different from that of the nitrite of amyl in this affection. It has been employed with advantage not only in angina pectoris, but also in pseudo-angina, cardiac asthma, and similar morbid states. The tabellæ of the British Pharmacopœia are not a good form of administration, and a simple one per cent. alcoholic solution is to be preferred. Originally, I gave this by itself, or with a little water; but now I always combine with it spirits of chloroform, tincture of capsicum or peppermint, to promote rapid absorption. I have seen many cases of angina pectoris practically cured by this method of treatment, although it was absolutely necessary for the patient to continue the use of the drug for months or even years. When the anginal attacks are slight, nitrite of amyl answers admirably, but when they are more severe, it is necessary to resort to nitroglycerine to control them. With regard to the nitrites, I had an unfortunate experi-

Digitized by Google

ence. Dr. Law recommended nitrite of sodium in twenty-grain doses in epilepsy and angina pectoris. Upon adopting this recommendation, I had unfavorable results; and it subsequently turned out that Dr. Law had been using an impure drug, while I had used the pure salt. I subsequently concluded that the field was so much better occupied by the nitrite of amyl and nitroglycerine that I did not need the nitrite of sodium. In one case I noted that it acted very decidedly as a diuretic.

Dr. UPSHUR.—What is the dose of nitroglycerine?

Dr. MURRELL.—I cannot lay down any rule, but one-hundredth of a grain is the usual dose. Some persons can take very little without producing headache; others can take fifteen minims of a one per cent. solution, and I have known a man who could take one minim without bad result. Patients can keep on taking the drug for a long time. It should be carried in the pocket ready for use if an attack threatens, or the patient might die before the remedy could be obtained to relieve him.

Dr. UPSHUR.—Is it equally useful in other forms of neuralgic attacks, especially such as the reflex neuroses attending uterine disorders? At the climacteric period, women suffer from various forms of nervous disorders, especially sub-mammary pain, and headache located just to the left of the parietal suture, and when it becomes more violent, radiating over the entire head. This accompanies uterine disease. For this neurosis he had tried almost everything, but had not yet used nitroglycerine, on account of uncertainty as to dosage.

In the cases of puerperal septicæmia, nitrite of amyl was given with marked success. The patient was in a state of collapse; skin cool and bathed in perspiration; the only evidence of life was a fluttering action of the heart. As the patient was dying of heart failure, it occurred to him to use the nitrite of amyl to sustain the circulation. Within three minutes from the time she began the inhalation of the amyl the skin became dry and the pulse returned to the wrist, and she opened her eyes. I watched her for two hours, and then left the agent in the hands of an intelligent nurse, directing that it was to be resorted to when there was evidence of the failure of the pulse. The patient took the remedy, at intervals, for forty-eight hours, when it was gradually withdrawn, and alcoholic stimulants substituted. My friend, Dr. Ashton, of Port Royal, has used the amyl in a similar case, with the happiest results.

Dr. H. G. BEYER, U. S. Navy.—Is the dilatation of the blood vessels from nitroglycerine a general or local effect? Is the diuresis which follows due to dilatation of the blood vessels or to a direct action upon the kidneys?

Dr. MURRELL.—I have used nitroglycerine in the following kinds of cases: angina pectoris, pseudo-angina pectoris, cardiac asthma, some kinds of headache, and in some forms of neuralgia in cases of Bright's disease. It has also been recommended in sea-sickness, in asthma, both cardiac and spasmodic, and surgical shock. As to the dose, much depends upon the form in which it is administered. The best form is the centesimal alcoholic solution. With regard to the effect, I consider that it is a general and not a local one.

Dr. WADE.—The pearls of nitroglycerine are convenient for use. I consider it the best heart stimulant known for sudden emergencies. A very convenient way to carry it is as a ten per cent. solution in alcohol, which can also be used for inhalation.

Dr. PHILLIPS.—In regard to the dose of nitroglycerine, in cases of angina pectoris, I have found it very variable. In difficult cases I give one minim of the one per cent. solution every half hour, for four days. Some patients take twenty-five

Digitized by Google

and even thirty minims, in the form of the tabellæ of the Pharmacopœia, each of which contains one minim. In other patients I found it impossible for them to tolerate more than three or four minims. With regard to the nitrite of sodium, I found it very valuable in dyspnœa from bronchitis; the effect is more prolonged than from the other nitrites, and it is better taken.

Dr. BRACKETT.—I have listened with interest to the remarks with regard to the use of nitrites in epilepsy. In one case of my own, the action of the nitrite of amyl was very favorable. It is necessary, in order to use it, that there should be some warning of the attack. My patient, who was a printer, on the appearance of the symptoms indicating the approach of a convulsion, crushed a nitrite of amyl pearl, and by inhaling it, warded off the attack. The tendency to convulsion passed away, and he was able to go on with his work. The effect did not seem to be curative at all, except that it was abortive, and lessened the number of convulsions. If it only aborts the attacks it is a remedy of great value. The great point is to keep the patient supplied with the drug for prompt use.

Dr. WOODBURY.—I would like to know the hypodermic dose of the one per cent. solution. I have seen it stated that it was superior to brandy for heart failure, and could be administered in doses of from five to fifteen minims.

Dr. MURRELL.—The pearls are too expensive, and the same objection might be urged against the tabellæ. The best way is to keep the solution in a small bottle. The capsules are mere chemists' curiosities. I have not had much experience with the hypodermic use of nitroglycerine, which is slower than by the mouth, and I prefer to give it by the mouth.

Dr. WADE.—I have used the nitrites, on theoretical grounds, in localized suppuration of the brain, in cerebral embolism and thrombosis, in order to prevent the tendency to defective nutrition of the brain. The effect was very decided in several cases, and was shown by great improvement in intelligence.

Dr. PHILLIPS.—A dose of twenty minims hypodermically is too large, as it would represent from forty-five to sixty by the mouth. I have given five minims of the one per cent. solution, and it brought on headache and dizziness, although it relieved the neuralgia for which it was taken.

ÜBER DAS GIFTSEKRET DER NAJA TRIPUDIANS, GÜNTH. (COBRA DE CAPELLO).

ON THE VENOMOUS SECRETION OF NAJA TRIPUDIANS, GÜNTH. (COBRA DE CAPELLO).

SUR LA SÉCRÉTION EMPOISONNÉE DU NAJA TRIPUDIANS.

VON J. GNEZDA, M. D.

Der Umstand, dass die giftigen Sekrete der Schlangen in ihren fortdauernden Eingriffen in die organische Lebewelt, ihrem exceptionellen physiologischen Herkommen und in ihrer chemischen Natur, sowie als Venenum selbst wenig gekannt sind, befürwortete die Untersuchung eines der verbreitetsten derselben. Die Gelegenheit zu einer solchen wurde durch Prof. E. DuBois-Reymond in Berlin geboten, welcher ein von

Digitized by Google

dem Leiter der Deutschen Cholera-Commission, Prof. R. Koch, aus Indien gebrachtes Quantum von reinem Gifte der Naja tripudians dem Verfasser zur Verfügung stellte.

Zur Schilderung des Giftes nach verschiedenen Seiten hin übergehend, sei bemerkt, dass mit derselben nur Thatsachen und Resultate von mit diesem Giftpräparate vorgenommenen Experimenten registrirt werden sollen. Aus diesem Grunde kann auf die, dieser vorangegangenen Arbeiten von Brainard, Vulpian, Fayrer, Sedler, Gautier, Aronsohn, u. s. w., hier nicht eingegangen werden.

Das zu den Reaktionen und Experimenten benützte Präparat zeichnete sich durch möglichste Reinheit aus, indem bei der von Prof. Koch mitgetheilten Gewinnungsweise des Giftes keinerlei chemische Umwandlung damit vorgegangen sein konnte. Man liess nämlich die gereizten Brillenschlangen in Muschel- oder Schneckenschalen, deren Mündungen mit Pergament verschlossen waren, beissen. Das flüssig eingedrungene Sekret erstarrte bald an der Luft und bildete nun eine Substanz, welche sich in unregelmässigen amorphen, kleinen Splittern präsentirte, etwa wie grobkörnig gestossenes Glas. Es besitzt strohgelbe Farbe und bricht das Licht einfach. Wenn gelöst (3%), ist es stark adhärent am Glase. Die wässerige Lösung sieht schleimig und bläulich opalescent aus, reagirt neutral und ergiebt, geprüft auf das chemische Verhalten:

Das Cobragift ist leicht löslich in Wasser, unlöslich in Alcohol, Aether und Eisessig.

Mit Salzsäure giebt das Gift einen weissen Niederschlag, welcher unter dem Microscop amorph erscheint, in Ueberschuss von Salzsäure sich nicht löst, wohl aber in Wasser. Trägt man in eine salzsaure Lösung des Giftes Steinsalz ein, so bildet sich um den Steinsalz-Crystall herum ein weisser Niederschlag. Mit Salpetersäure giebt die Giftlösung einen reichlichen weissen Niedershlag, welcher bei starkem Erhitzen und Ueberschuss von Salpetersäure gelb wird. In verdünnter Lösung verschwindet der Niederschlag beim Erhitzen.

Sättigt man die neutrale Lösung mit Steinsalz, so entsteht ein weisser Niederschlag. Mit Natron- oder mit Kalilauge, sowie doppelkohlensaurem Natron entstehen keine Fällungen.

Die Lösung trübt sich beim Sieden; setzt man Essigsäure dazu, so bildet sich kein Niederschlag, auch nicht beim Kochen mit Kalisulphat, sondern eine blosse Trübung. Die Giftlösung verliert durch Kochen ihre giftige Wirkung. (Versuch an Kaninchen.)

Mit neutralem Bleiacetat entsteht eine schwache Trübung, mit basischem ein schwacher Niederschlag. Mit Platinchlorid giebt das Cobragift einen starken Niederschlag, mit ammoniacal. Silbernitrat keinen. Quecksilberchlorid giebt einen weissen Niederschlag. Ferner tritt eine starke Fällung ein mit Quecksilbernitrat, mit Jodkalium, Jodquecksilber.

Essigsäure und Ferrocyan-Kalium, sowie Phosphorwolframsäure geben Niederschlag.

Wird eine geringe Quantität des Giftes mit Millons Reagens erhitzt, so entsteht eine rothe Färbung.

Mit Natronlauge und Kupfersulphat giebt das Gift in der Kälte die Biuretreaction.

Die mit dem Cobragifte vorgenommenen *Experimente** ergaben:

Subcutan oder subperitoneal injicirt, tödtet es Amphibien, Reptilien, Fische, Vögel und Säugethiere. Letztere gehen bei gleicher Dosis und unter Berücksichtigung des Körpergewichtes am ehesten zu Grunde, während Tauben relativ immun sind. Das Verhältniss gestaltet sich derart, das bei der gleichen Giftmenge (0,003) ein Kilogramm Frosch binnen 5½ Stunden stirbt, ein Kilogramm Hecht binnen 2 Stunden, ein Kilogramm Katze binnen 10¾ und dieselbe Menge Taube binnen 26 Stunden. Am schnellsten gingen Kaninchen zu Grunde, nämlich binnen einer halben Stunde.

* Verwendet wurde eine 3% wässerige Lösung.

Digitized by Google

Der Igel (Erinaceus europ. L.) wird vom Cobragifte ebenso gut getödtet, als andere Säugethiere.

Wird das Cobragift durch den Oesophagus in den Magen von Fröschen gebracht, so tödtet es dieselben. Ebenso Sperlinge. Das Cobragift wird von der Schleimhaut der Conjunctiva und Vagina resorbirt. Eine Blasenbildung wurde nicht beobachtet.

Was die Wirkung des Najagiftes auf einzelne Gewebssysteme und Organe betrifft, so konnte man beobachten an Nerven:

Die Bewegungslosigkeit der vergifteten Thiere rührt nicht von einer directen Lähmung der motorischen Nerven her. Man konnte an todten Kaninchen und Fischen dieselben mit Erfolg reizen. Bei Parallelversuchen mit normalen Fröschen, sowie einseitig unterbundenen Nervmuskelpräparaten, zeigte sich eine verminderte Reizbarkeit des vergifteten Nerven im Vergleiche zum intakten. Das elektrotonische Verhalten zeigte eine Abschwächung des myopolaren Kat- und Anelektrotonus.

Eine erhöhte Reflexerregbarkeit wurde ausnahmslos bei allen vergifteten Thieren beobachtet.

Bei Kaninchen, Meerschweinchen und Katzen zeigte sich: Gesteigerte Puls- und Athemfrequenz, Erweiterung der Pupillen, Aufrichten der Ohren, Salivationen, Brechen, Zwangsbewegungen, Zwangsstellungen und Coordinationsstörungen, Convulsionen, bis zum Zusammenfall. Bei Tauben gesellt sich dazu das Unvermögen, zu fliegen, trotzdem zweckmässige Bewegungen dazu stattfinden. Auf optische und acustische Reize reagiren erwähnte Säugethiere gut. Enthirnt man Frösche, so zeigen sich keine abweichenden Vergiftungsphänomene in Bezug auf Dauer oder Bewegungserscheinungen; vergiftet man jedoch das Rückenmark eines decapitirten Frosches, so stellt sich Strychnintetanus ein. Post mortem weist das Gehirn der Säuger stark injicirte Gefässe der Pia auf.

Vorherige doppelte Vagusdurchschneidung bei Kaninchen, mit darauffolgender Giftinjection erschien indifferent in Bezug auf Athmung und Herzschlag.

Die Muskeln der vergifteten Thiere als System zeigten keine Abnormitäten. Wurde Gift direct in einen Muskel (M. gastrocnemius) injicirt, so erhielt man beim Frosche etwas verminderte Reizbarkeit auf directe elektrische Reize. Muskeln unmittelbar verendeter Kaninchen reagiren physikalisch und chemisch gleich denen normaler. Der Rigor mortis tritt bald post mortem ein und ist sehr stark.

Die verschiedenen Drüsen betreffend, so secerniren die Frösche (früher abgetrocknet) ihr Hautsekret nach der Vergiftung sehr reichlich. Vögel und Säugethiere bekommen Salivationen und Regurgitationen.

Bei der Section erscheint die Leber schwammig, leer und leicht zerreissbar, die Milz zusammengeschrumpft, das Pancreas als in voller Thätigkeit gewesen.

Weder das Herz der Kalt- noch der Warmblüter scheint direct beeinflusst zu werden. Parallelversuche an ausgeschnittenen Froschherzen in physiolog. Kochsalzlösung ergaben häufige Contractionen des vergifteten unmittelbar nach der Injection von Cobragift. Diese vermehrte Frequenz weicht bald einer constanten Abnahme. Vagisection ändert bei Kaninchen an der Frequenz wenig.

Die *Respirationen* nehmen gleich den Herzstössen an Zahl erst zu und dann constant ab. Künstliche Athmung bei Erzeugung von Apnöe bewirkte in drei Fällen bei Kaninchen verlängerte Vergiftungsdauer; die Thiere gingen nicht wie sonst in einer halben, sondern in 1½ bis 2 Stunden zu Grunde. Die Lungensäcke der vergifteten Frösche erscheinen collabirt, hyperämisch and bläulich; die Lungen der Säugethiere hellroth mit hämorrhagischen Herden.

Eine Wirkung des Cobragiftes auf das Blut, resp. einer oder mehrerer seiner Bestandtheile, konnte mit dem Holm-Green'schen Apparate an den Capillaren der Froschlunge beobachtet werden. Die geradlinig verlaufende Capillare nimmt wellenförmige Contouren an, nachdem vorher durch die V. hypogastrica das Cobragift injicirt wurde.

Digitized by Google

Die rothen Blutkörperchen werden deformirt, indem sie Stechapfelform annehmen. Der Blutdruck (gemessen an Kaninchen) wird durch das Cobragift, wenn auch in geringem Maasse, doch constant etwas erhöht. Blut von Fröschen und Kaninchen direct mit Cobragift versetzt, nimmt eine schmutzig-rothbraune Färbung an. Das Blutspectrum wird durch das Cobragift verändert. An der injicirten Stelle zeigt sich ein missfarbiger, dunkelrother Fleck, um den sich ein heller Hof bildet; die Gefässe um dieselbe in einer weiten Ausdehnung sehr stark injicirt.

Die Harnblase der vergifteten Säugethiere war stets stark gefüllt; am Harn nichts Auffälliges zu bemerken.

Die Körpertemperatur eines winterschlafenden Igels, dem 0,03 Cobragift injicirt wurden, stieg binnen fünf Stunden um 3° C. bei constanter Zimmertemperatur. Eine leichte Erhöhung der Temperatur ist bei Säugethieren in der Agonie wahrnehmbar und hält bei Katzen noch post mortem eine halbe Stunde an.

Die Frage nach einem Antidot des Cobragiftes betreffend, so wurde davon ausgegangen, erst die Wirkungsweise des Giftes kennen zu lernen, um dann weitere Versuche zu unternehmen. Die Beantwortung der Frage, ob das Agens des Giftes ein Ferment, ein Alkaloid oder eine Cyanverbindung, würde es in Verbindung mit den Ergebnissen des Experimentes erst ermöglichen, einen Erfolg für die Therapie zu erwarten. Denn die durch eine Speculation herbeigeführten Versuche zeigten, dass den wirkungslosen Substanzen anderer Autoren noch zuzuzählen seien: Borax, Salicylsäure und Curare.

Eine Beschreibung der Experimente und extense Darstellung des Gegenstandes soll an anderer Stelle erfolgen.

DISCUSSION.

Dr. Phillips.—The method of collecting the poison was ingenious, and the report of its physical and physiological qualities is very interesting.

Dr. Lewin.—Ich möchte gern den Vortragenden fragen, ob er das Blut spectroscopisch untersucht hat. Es würde dies vielleicht gerade in Rücksicht auf die berichteten microscopischen Veränderungen der rothen Blutkörperchen zu einem Resultat geführt haben. Denn es scheint ein gewisser Zusammenhang zwischen diesen beiden Punkten zu herrschen, wie es sich zum Beispiel nach der Vergiftung mit chlorsaurem Kali und anderen Giften zeigt. In Bezug auf die Gegengifte des Cobragiftes sei hervorzuheben, dass alle uns bekannten chemischen Antidote nur wirken, wenn sie an der Bissstelle mit allen Theilen des Giftes in Berührung gekommen sind. Es ist sicher, dass solche Stoffe, wie das von Lacerda empfohlene Kalium permanganicum, in grosser Menge vorhanden sind. Doch sei auf den in neuerer Zeit aus Indien berichteten Punkt hinzuweisen, dass Schlangenbeschwörer in manchen vollkommen verzweifelten Fällen von Cobrabiss durch Inoculation eines unbekannten Stoffes complete Heilung erzielt haben. Es ist nicht von der Hand zu weisen, dass ein solcher chemisch wirkender, wahrscheinlich aber nur symptomatisch wirkender Stoff vorhanden ist.

Dr. J. Gnezda.—Die Gestaltveränderung der rothen Blutkörperchen in Stechapfelform ist als gebräuchlicher Terminus hiermit definirt. Die spectroscopische Veränderung des Blutes kann nicht eher als von einem bestimmten Körper herrührend bezeichnet werden, bevor dieser Körper dargestellt ist.

Dr. Lewin.—Die von dem Herrn Vortragenden gesehenen Streifen sind interessant; aber ähnliche nach der Linie D liegende drei (3) Streifen habe ich bisher nicht gesehen. Ich mache nochmals besonders auf die Wichtigkeit des Zusammenhanges zwischen Veränderungen der rothen Blutkörperchen und chemischen Verän-

Digitized by Google

derung des Blutes aufmerksam und verweise in dieser Beziehung auf die Untersuchungen von mir und C. Posner über das Methämoglobin und Hämatin.

Dr. F. Woodbury.—The results of the investigations of the lecturer were in the same line as those of Mitchell and Reichert upon the poison of the rattlesnake. It will be remembered that these observers reported the finding of two proteid toxic substances in the rattlesnake virus. The incident recorded in the life of Buckland gives a good idea of the effect of the cobra poison upon man, and shows that ammonia and brandy will save life provided that only a small amount of the poison has entered the circulation.

Dr. Traill Green.—The question of the treatment of snake poison has interested me for many years. In one instance, in which a rat died instantly when struck by the snake, I found that the fang had penetrated the jugular vein. The toxic effect may be prevented if the virus does not enter the circulation.

Dr. L. Lewin, of Berlin, read a paper—

ÜBER DIE MAXIMALDOSEN DER MEDICAMENTE.

ON THE MAXIMAL DOSES OF MEDICINES.

SUR LES DOSES MAXIMUM DE MÉDECINES.

VON DR. L. LEWIN.

Von den vielen Schwierigkeiten, mit denen die Pharmakotherapie zu kämpfen hat, ist nicht die geringste die Dosirung der Medicamente. Es kann wohl der Satz ausgesprochen werden, dass jede übliche Dosirung einer Arzneisubstanz unzuverlässig ist und immer nur einen Versuch darstellt. Die Gründe hierfür sind einleuchtend genug; von den wesentlichen führe ich nur an:

I. Die Verschiedenheit
   a) der Menschen untereinander;
   b) des einzelnen Individuums zu verschiedenen Zeiten;
   c) in der Intensität der Erkrankung der Menschen an ein und demselben Leiden;
   d) der Krankheiten unter sich, gegen die das gleiche Arzneimittel gebraucht wird.

II. Die Schwankungen in der Wirksamkeit eines grossen Theiles unserer Arzneistoffe.

Von dem wesentlichen Einflusse dieser und anderer Factoren ist man von jeher überzeugt gewesen. Dieselben lassen sich jedoch im einzelnen Falle wenig oder gar nicht in Berücksichtigung ziehen, weil der grössere Theil derselben der menschlichen Erkenntniss wahrscheinlich für immer verschlossen bleiben wird. Man suchte deshalb bis in unsere Zeit hinein *die Dosirung der nicht, oder nur erst in sehr grossen Mengen heroisch wirkenden Heilmittel* so zu gestalten, dass man gewisse, meist durch Empirie gewonnene Durchschnittsdosen als normale ansieht, und bei dem Gebrauche derselben auch die Individualität, soweit es angeht, berücksichtigt. Nur in einigen Pharmakopöen, wie z. B. der britischen und spanischen, sind diese, gewissermaassen traditionellen Arzneiangaben freilich nur zur Information der Aerzte und ohne bindende autoritative Kraft angegeben worden.

Ausser diesen Durchschnittsdosen besitzen aber die Pharmakopöen der meisten

Digitized by Google

Staaten gesetzmässige Feststellungen der einzelnen und der täglichen Gabe, oder wie die schwedische und norwegische Pharmakopöe nur für die Einzeldosis, von solchen Stoffen, welche eine Wirkung schon *in relativ kleiner Menge eintreten und beim Steigern der Dosen leicht Giftwirkungen erscheinen lassen.* Die Nothwendigkeit solcher maximalen Dosirung, die selbstverständlich nicht das freie Ermessen einer Ueberschreitung ausschliesst, leuchtet ein, wenn man bedenkt, dass gerade die unzweckmässige Verabfolgung derartiger Substanzen leicht zu schwerer Schädigung der Gesundheit führen kann. Dem Arzte wird dadurch, nicht als absolute Vorschrift, sondern zur warnenden Belehrung, eine approximative Grenze für sein Verschreiben angegeben. Es wäre sehr zu wünschen, dass die Staaten, welche eine solche officielle Begrenzung von Arzneidosen bis jetzt nicht besitzen, dieselben einführten. Es würden dann z. B. die Anklagen beim Eintreten einer perversen Arzneiwirkung, denen der Arzt am leichtesten da ausgesetzt ist, wo keine derartigen Bestimmungen vorhanden sind, nicht vorkommen können.

Zwei Gruppen von Präparaten fallen in den Bereich dieser Maximaldosen.

1) Pflanzen, rohe Pflanzenprodukte und pharmaceutische Darstellungen aus denselben.
2) Chemisch einheitliche, wohlcharakterisirte Substanzen, wie Metalloide, Metallsalze und Kohlenstoffverbindungen.

Die erstgenannten Stoffe sind an sich ihrem Wirkungswerthe nach fast insgesammt inconstant, und soweit die galenischen Präparate hierbei in Frage kommen, weisen sie ausserdem noch bedeutende Verschiedenheiten in ihrer Wirkung auf, weil eine internationale Uebereinstimmung in der Darstellung sich bisher nicht hat ermöglichen lassen. Die Arzneimittel der zweiten Gruppe sind dagegen überall gleichwerthig herstellbar und äussern demnach ceteris paribus die gleichen Wirkungen. Die Grenzdosen, welche von diesen Stoffen nicht ohne triftigen Grund überschritten werden sollen, wurden durch Versuche an Gesunden und Kranken, durch Vergiftungen von Menschen und das Thierexperiment gewonnen. Trotzdem also die Basis hierfür eine ausreichend breite ist, sind doch die entsprechenden Angaben in den verschiedenen Pharmakopöen bei vielen Stoffen sehr verschieden. Diese Differenz ist hinsichtlich der ersten Gruppe verständlich, aber unbegreiflich, soweit die zweite in Frage kommt.

Dieser Punkt bildet den Anlass zu der vorliegenden Auseinandersetzung und den sich daran knüpfenden Vorschlägen. Wäre es möglich, alle starkwirkenden galenischen Präparate international in gleicher Weise darzustellen, so wäre damit auch die Möglichkeit einer allgemeinen gleichen maximalen Dosirung aller hierhergehörigen Stoffe gegeben. Ich halte dies aber bis auf weiter für unerreichbar. Dagegen lässt sich sehr wohl die zweite Gruppe und ein Theil der ersten der mit maximaler Dosis zu versehenden Stoffe in dieser Beziehung äqualisiren. Die Wichtigkeit einer derartigen Ordnung liegt auf der Hand. Der jetzt so rege internationale Verkehr, die Thatsache, dass Recepte des einen Landes vielfach in einem anderen dargestellt werden und dadurch schwere Unzuträglichkeiten entstehen können, erfordert dringend, dass wenigstens das in dieser Beziehung Erreichbare erstrebt wird.

Die bisher bestehende Ungleichheit in der Dosirung und der Auswahl der bezeichneten Substanzen sind zum Theil ausserordentlich beträchtlich. So wird Codein in der *Pharmakopöa Hungarica* (1871) zu 0.03 grm. maximal begrenzt, in der *Pharmakopöa Swedica* (Ed. VII) zu 0.25 grm. Die *Pharmakopöa Neerlandica* versieht die verdünnten Mineralsäuren mit einer solchen Grenzzahl, während sie Guttæ davon frei lässt. Ausserdem aber vermag ich auf Grund meiner toxicologischen Studien mit Bestimmtheit auszusprechen, dass manche dieser officiellen Angaben einer wissenschaftlichen Basis entbehren und schon aus diesem Grunde einer Revision bedürfen, andere für den Arzt bequemer eingerichtet werden können, ohne dass dadurch wesentlich an dem Erfolge der Dosen etwas geändert wird.

Digitized by Google

Ich habe deswegen mit Berücksichtigung meiner eigenen Forschungen und der meisten Pharmakopöen, welche maximale Dosen anführen, einen Entwurf der Stoffe verfasst, welche zu der von mir skizzirten, zweiten Gruppe gehören, und sie mit den Grenzzahlen versehen, die ich als empfehlenswerth ansehe. Jeder Pharmakopöe, auch denen, die nicht maximale Dosen enthalten, sollte eine solche Tabelle im allgemeinen Interesse beigefügt werden, um den Arzt in den Stand zu setzen, auch stark wirkende einheitliche Präparate, die in seiner Heimath nicht officinell sind, in richtiger Menge verschreiben zu können.

Es wäre zu wünschen, dass von dem internationalen Congresse aus der Anstoss gegeben wird, diese Materie in dem hier betonten Sinne international and bald zu regeln.

ENTWURF ZU EINER INTERNATIONALEN MAXIMALEN DOSIRUNG GLEICHMÄSSIG DARSTELLBARER HEILMITTEL.

	Pro dosi.	Pro die.		Pro dosi.	Pro die.
Acidum arsenicosum	0.005	0.02	Hyoscyaminum crist.	0.01	0.04
Acidum carbolicum	0.2	0.6	Iodoformium	0.2	0.6
Acidum oxalicum	0.4	1.0	Iodol	0.2	0.6
Aconitinum crist. Duquesnel	0.001	0.003	Iodum	0.1	0.4
Apomorphinum hydrochloric.	0.01	0.04	Kalium bichromicum	0.03	0.1
Argentum nitricum	0.05	0.2	Kalium chloricum	2.0	10.0
Arsenicum jodatum	0.01	0.04	Kalium cyanatum	0.03	0.1
Atropinum sulfuricum	0.002	0.005	Kalium osmicum	0.03	0.1
Auro-natrium chloratum	0.05	0.2	Kalium sulfuratum	0.5	2.0
Barium chloratum	0.4	1.0	Kreosotum	0.2	0.6
Bromum	0.05	0.2	Liquor kalii arsenicosi (1% Ac.		
Brucinum	0.1	0.4	arsenicosum)	0.5	2.0
Caffeinum	0.4	1.0	Morphinum	0.03	0.1
Chininum sulfuricum	2.0	4.0	Morphinum sulfuric. u. hydro-		
Chloralum hydratum	2.0	4.0	chl.	0.03	0.1
Cocainum hydrochloric	0.2	0.6	Narceinum	0.1	0.4
Codeinum	0.1	0.4	Narcotinum	0.5	2.0
Colchicinum crist.	0.005	0.02	Natrium arsenicicum	0.005	0.02
Coniinum	0.002	0.005	Nicotinum	0.002	0.005
Coniinum hydrobromatum	0.05	0.2	Nitroglycerinum	0.002	0.005
Cuprum sulfuricum	1.0 pro emetico.		Oleum crotonis	0.05	0.2
Delphininum	0.01	0.04	Opium (10% Morphin)	0.2	0.6
Digitalinum crist. Nativelle	0.001	0.003	Papaverinum	0.4	1.0
Digitalinum crist. Homolle	0.002	0.005	Phosphorus	0.005	0.02
Duboisinum sulfuric	0.002	0.005	Physostigminum salycilic.	0.002	0.005
Ferrum arsenicicum	0.01	0.04	Pikrotoxinum	0.01	0.04
Homatropinum hydrobromic.	0.01	0.04	Pilocarpinum hydrochlor.	0.03	0.1
Hydrargyrum bichloratum	0.03	0.1	Plumbum aceticum	0.2	0.6
Hydrargyrum bijodatum	0.03	0.1	Santoninum	0.2	0.6
Hydrargyrum chloratum	1.0	2.0	Sparteinum sulfuricum	0.2	0.6
Hydrargyrum cyanatum	0.03	0.1	Stibium sulfurat. aurant.	0.2	0.6
Hydrargyrum jodatum	0.1	0.4	Strychninum	0.01	0.04
Hydrargyrum nitric. oxydul.	0.03	0.1	Strychninum nitricum	0.01	0.04
Hydrargyrum oxydatum	0.03	0.1	Tartarus stibiatus	0.2	0.6
Hydrargyrum oxydul. nigr.	0.2	0.6	Veratrinum	0.01	0.04
Hydrargyrum sulfuric. basic.	0.4 pro emetico refr. dosi.		Zincum chloratum	0.03	0.1
Hyoscinum hydrojodic. u. hydro-			Zincum cyanatum	0.03	0.1
brom.	0.002	0.005	Zincum sulfuricum	1.0 pro emetico.	

Digitized by Google

ENTWURF ZU EINER INTERNATIONALEN MAXIMALEN DOSIRUNG GLEICHMÄSSIG DARSTELLBARER HEILMITTEL.

Pro dosi.	Pro die.		Pro dosi.	Pro die.	
0.001	0.003	Aconitinum crist. Digitalinum nativelle.			Argentum nitricum. Auro-natrium chloratum.
		Atropinum sulfuric.	0.05	0.2	Bromum.
		Coniinum			Coniinum hydrobromatum.
		Digitalinum Homolle & Quevenne.			Oleum crotonis.
		Duboisinum sulfuricum.			Brucinum.
0.002	0.005	Hyoscinum hydrojodicum u. hydrobromic.	0.1	0.4	Cadeinum. Hydrargyrum jodatum.
		Nicotinum.			Iodum.
		Nitroglycerinum.			Narceïnum.
		Physostigminum salicylicum.			Acidum carbolicum.
		Acidum arsenicosum.			Cocainum hydrochloricum.
0.005	0.02	Colchicinum.			Hydrargyrum oxydulatum nigrum.
		Natrium arsenicicum.			Iodoformium.
		Phosphorus.			Iodolum.
		Apomorphinum hydrochloric.	0.2	0.6	Kreosotum.
		Arsenicum jodatum.			Opium.
		Delphininum.			Plumbum aceticum.
		Ferrum arsenicicum.			Santoninum.
0.01	0.04	Homatropinum hydrobrom.			Sparteïnum sulfuricum.
		Hyoscyaminum crist.			Stibium sulfurat. aurantiacum.
		Pikrotoxinum.			Tartarus stibiatus.
		Strychninum.	0.4	...	Hydrargyrum sulfuric. basic.
		Strychninum nitricum.			Acidum oxalicum.
		Veratrinum.	0.4	1.0	Barium chloratum.
		Hydrargyrum bichloratum.			Caffeinum.
		Hydrargyrum bijodatum.			Papaverinum.
		Hydrargyrum cyanatum.			Liquor kalii arsenicosi.
		Hydrargyrum nitric. oxydulatum.	0.5	2.0	Kalium sulfuratum.
		Hydrargyrum oxydatum.			Narcotinum.
0.03	0.1	Kalium bichromicum.	1.0	...	Cuprum sulfuricum.
		Kalium cyanatum.			Zincum sulfuricum.
		Kalium osmicum.	1.0	2.0	Hydrargyrum chloratum.
		Morphinum et ejus salia.	2.0	4.0	Chininum sulfuricum.
		Pilocarpinum hydrochloric.			Chloralum hydratum.
		Zincum chloratum.	2.0	10.0	Kalium chloricum.
		Zincum cyanatum.			

DISCUSSION.

Dr. F. Woodbury.—It is known that the doses of medicines must vary according to circumstances. Patients recovering from mania a potû may take an ounce of chloral hydrate without any ill effect; others are dangerously affected by ten grains. Moreover, patients may take a certain quantity for a dose for a long time and regard it as a safe dose, and yet on some occasion this same dose will rapidly bring on fatal coma. I have observed that fatal cases occur after eating a meal. Is it possible that chloroform might be liberated, when chloral is given during digestion, so that it comes in contact with the bile, which is a highly alkaline fluid? In some patients even the smallest quantity of chloral is fatal.

Dr. H. G. Beyer, U. S. Navy.—The remarks just made remind me of a paper by Dr. Mayer, which appeared in the *Archiv für Pathologische Pharmacologie*, and in which the author tried to prove experimentally that the action of chloral hydrate, as well as that of trichloracetic acid, and, in fact, all other halogen derivatives of organic compounds, produce their effects by the chlorine which they contain, which, indeed, is said to be set free whenever these compounds are brought into an acid medium. The cells of the cerebrum are supposed to react slightly acid during intellectual activity. When trichloracetate of sodium is injected into the blood the chemical is decomposed, chlorine set free, and sleep is produced, or intellectual torpor, in the same manner as that is done by chloral hydrate.

Digitized by Google

Dr. Stockman.—It is not probable that the small amount of chloroform liberated by decomposition of an ordinary dose of chloral would be more injurious than the chloral hydrate, which is itself a direct muscle poison. It is known to all as a depressing agent to the heart, and ether is, therefore, much safer as an anæsthetic. On the other hand, there is a good deal of dissatisfaction felt with chloral on account of this depressing action.

The following paper was read:—

THE EMMENAGOGUE ACTION OF THE MANGANESE PREPARATIONS.

L'ACTION EMMÉNAGOGUE DES PRÉPARATIONS DU MANGANÈSE.

DIE EMMENAGOGE WIRKUNG DER MANGANPRÄPARATE.

BY JOHN N. UPSHUR, M.D.

Richmond, Va.

Empiricism in medicine, we desire to avoid, so far as may be, and in this modern age the ever increasing tendency to exactness makes it obligatory upon us as physicians to administer remedies intelligently and not by a system of guessing. When Murrell and Ringer suggested the Manganese Preparations as a remedy in the treatment of amenorrhœa, an important advance was made in the medication of this affection. But it remains for them or others to systematize and arrange, and determine when properly to exhibit the remedy, and when it should be withheld. Careful observations were necessary in order that useful deductions might be drawn and definite rules of administration formulated.

This was the more needful because of the multiple causes which may give rise to menstrual disorders, and hence the importance of carefully scanning not only all forms of amenorrhœa, but of dysmenorrhœa also.

Furthermore, it is important that we should investigate the remedy as to its physiological action, that we may fully comprehend the manner in which the beneficial results accrue, or the cause of failure when no benefit is derived. It is thus, and thus alone, that we can intelligently arrive at sound conclusions with regard to this or any other remedy.

And first let us interrogate this agent as to its physiological action on the system, see to what beneficial results are to be ascribed, and why, if there should be, any choice in the particular preparation selected for exhibition. The virtue of the remedy, of course, is found in the manganese base.

PHYSIOLOGICAL ACTION.

The only definite knowledge we have of the action of the manganese preparations is in the local action of the Permanganate of Potash when used as a deodorizer and disinfectant, its value being based upon the readiness with which it parts with its oxygen. This, as well as other manganese preparations, is easily decomposed in the presence of any organic matter, giving up its oxygen freely. Bruce says of its action internally, that nothing definite is known (Materia Medica, p. 83). The readiness of its decomposition tends to prove that it enters the blood in some *nascent* form, and possibly may exert its influence on the uterus and appendages through the stimulant influence of the oxygen on the red blood globules, and by diminishing the lymph

Digitized by Google

constituent of the blood, thereby diminishing its plasticity, and thus promoting the increased fluidity of the vascular fluid, it antagonizes inflammatory action, so far as the endometrium is concerned. Or the stimulant influence of the blood nutrition, by virtue of an increased amount of oxygen contained, so influences the innervation of the ovaries and tubes as to effect a healthy, or at least, improved molimen.

In considering these agents the fact that manganese is found in combination with iron in its mineral state, is likewise contained, though in small proportion, in the blood and a few other tissues, that it is sometimes exhibited in anæmic or cachectic blood states as a substitute for iron, gives us at least a hint as to the modus of its action in the treatment of menstrual disorders.

Biddle tells us that "in small doses it improves the appetite, promotes digestion, and the body gains weight;" likewise, that "in large doses cardiac action is depressed and the blood pressure lowered" (Materia Medica, 10th Ed., p. 139). Permanganate of potash, like others of the potash salts, is regarded as an alterative; here we have a pointer, in another direction, of the mode of action in menstrual disorders.

Bartholow says it enters the blood as an albuminate, ... that in large doses it produces effects analogous to the salts of zinc—progressive wasting and feebleness, staggering gait and paraplegia (Materia Medica, 5th Ed., pp. 136–7). Here we have suggestions of action, definite and pronounced, upon the spinal cord or its membranes, through the vascular supply.

Brunton tells us that manganese salts have the power of interference with the transverse conduction power of the cord (Materia Medica, p. 640), but he does not even give us a suggestion of *how* this is effected. May it not be that reflex action, thus modified by smaller doses of these salts, the resultant action on the vasomotor nerves supplying the uterine blood vessels may permit dilatation of the uterine arterioles, and the consequence is an increased and easier flow of blood at the time of the period. There is no evidence, so far as I know, to show, or even suggest, that the agent under discussion possesses any ecbolic power, therefore its action must of necessity be in a different direction from stimulation to uterine contraction.

Having thus considered the action of the drug, it follows naturally that the morbid conditions for the relief of which it is exhibited must be carefully scanned. Every physician is familiar with the fact of how varied may be the causes of amenorrhœa. The age of the patient is primarily a factor. Has she arrived at the age of puberty? If so, has she ever menstruated at all; if not, why not? Does the cause lie in arrest of development or malformation of the organs of generation? Or is it located in the general system somewhere? If the girl has once had her menses established, what is the cause of the arrest? Is it physiological, as pregnancy, or pathological, from some diseased condition susceptible of removal? If she menstruates, what then are the conditions of the flow, scanty, painful, or irregular? Why these factors present? An answer to these questions gives us the various classes of cases to which the manganese preparations are now being daily administered.

CHOICE OF PREPARATION AND MODE OF ADMINISTRATION.

The two preparations usually administered are the permanganate of potash and oxide of manganese, and both, I believe, are best given in pill form. When an agent is so readily decomposed in the presence of any organic matter, its pharmacy must, of necessity, be difficult. Permanganate of potash is most easily made into pill with fuller's earth and administered in capsule. It is given in the dose of half a grain to two grains three times a day, after meals, beginning a week before the expected return of the period. The objection to its use being that it is so badly tolerated by the stomach that sometimes and in some subjects it is rejected almost as soon as swallowed, bringing with it the entire contents of the stomach, and, if this be not the case,

Digitized by Google

it causes such an uncomfortable burning sensation in the stomach as to arouse great opposition on the part of the patient to its administration. So great, indeed, is this aversion to it, that some patients who suffer most acutely at the time of the monthly period declare that they prefer to endure the pain, rather than tolerate the disagreeable effects of the remedy. This is the chief objection. I have not noted that it produces other disagreeable effects, except, perhaps, slight diarrhœa in a few subjects. To get the full effect of the remedy, it must be given before three successive periods.

The oxide of manganese should be given in the form of gelatine-covered pills. It has the advantage of being far less disagreeable to the stomach than the permanganate of potash. It should be administered in one or two-grain doses, after meals. My habit is to give it for a month, sometimes two months, with a slight interval, beginning immediately after a period and continuously on through the succeeding one. If benefit accrue from its exhibition it is demonstrated by a more comfortable flow, indicated by increase in quantity, improvement in color, and abolition or diminution largely of pain. These results, if the drug is to do good, may be expected the first month.

Finally, in what cases should the agent be exhibited and when withheld? In amenorrhœa due to an impoverished and cachectic condition of the blood, oxide of manganese, given in combination with some preparation of iron, will undoubtedly be of benefit. In that condition in which the amenorrhœa is due to defective vascular or nervous supply to the generative organs, benefit may be hoped for. When we have pain due to *functional cause*, as when no obstruction exists, but there is or has been some previously diseased condition which has left the endometrium in a state of abnormal congestion. Cold may have been contracted during a previous period, or there may have been an inflamed condition of the uterine mucous lining, due to an obstruction which has been removed, such as a cervix previously stenosed or flexed. In those cases in which we see vicarious menstruation. In amenorrhœa due to plethora and obesity. Here I would recommend larger doses, as it is claimed, as part of its physiological action, that it has the power of producing waste and lowering blood pressure. When existing in patients accustomed to self-indulgent and luxuriant habits. This class of patients are especially prone to menstrual disorders. *In fine, when the menstrual derangement is due to functional and not mechanical or obstructive cause.* Especially am I inclined to the opinion that the agent under consideration is of decided benefit in membranous dysmenorrhœa. Though I have seen a number of cases benefited, I have not seen a sufficient number to speak positively. Take a single case, by way of illustration. Mrs. T. C. W. suffered as a girl from anteflexion and consequent dysmenorrhœa. After the cervix had been slit posteriorly by her physician, and the endometritis, from which she coincidently suffered, had been cured, she married, soon became pregnant, and I delivered her at full term, after a short and unusually easy labor. She developed puerperal septicæmia, and had a protracted and dangerous illness. In a few months after her recovery she began to suffer again at her periods, the suffering increasing, and she soon developed well-marked membranous dysmenorrhœa, which, despite all treatment, and marked improvement of the uterine disease, persisted, and each month a tough, fibrous membrane was cast off from the uterine cavity. The first month after taking the manganese oxide there was a change in the constitution of the membrane; it was thinner and more delicate; she had amelioration in all of the trying symptoms she had so long endnred. The second period after only a thin pedicle was thrown off, and by the third it had entirely disappeared. After the remedy was stopped the membrane was again seen, but never in as aggravated a form as before. This same patient also complained of great lassitude and debility after a protracted use of the remedy.

These preparations are contraindicated in those conditions of the system in which the amenorrhœa is a conservatism of nature, serious organic disease of some

Digitized by Google

vital organ being the cause. Also in another class of cases, in *whom they produce the most intense headache.* These patients are not only forced to discontinue it in a few days, but if they have the fortitude to persist, my experience has not gone to prove that the menstrual derangement was in any degree corrected by it. In other patients, it seems to cause suffering by the intense nervousness which seems to follow its administration, and though they may derive some benefit, it is not to be confidently looked for. Analysis of the cause back of this will show the existence of ovarian irritation, hypertrophy or inflammation, and that they have long been the subjects of serious uterine disease.

Thus, I have endeavored, in some measure, to formulate the conclusions of a large use of this remedy; how imperfectly I have done so none can be better aware than myself. I trust, however, that what I have said may be, in some measure at least, a stimulus to others to closer and more accurate investigation.

FORMULA FOR OFFICIAL DILUTE HYDROBROMIC ACID OF THE UNITED STATES PHARMACOPŒIA.

FORMULE POUR L'ACIDE HYDROBROMIQUE DÉLAYÉ OFFICIELEMENT DE LA PHARMACOPÉE DES ETATS UNIS.

FORMEL FÜR DIE OFFICIELLE VERDÜNNTE BROMWASSERSTOFFSÄURE DER PHARMAKOPÖE DER VEREINIGTEN STAATEN.

BY DE WITT CLINTON WADE, M. D.,

Holly, Michigan.

In 1874 I produced hydrobromic acid by the decomposition of bromide of potassium with two equivalents of tartaric acid, and immediately commenced the study of its therapeutic properties. My first paper appeared in the *Peninsular Medical Journal* (Detroit), in February, 1875. Previous to these dates this acid had not been made by this process, and had never been used or written upon as a medicine. It is now in general use in all countries, and the therapeutic and pharmacological literature regarding it has been many times enriched in all languages. My original formula, which was designed to make each fluid drachm of the finished product represent ten grains of bromine, was published thousands of times, and I have reason to believe is in general use to-day. The last revision of the United States Pharmacopœia made dilute hydrobromic acid official, and its strength equaling ten per cent. of acid. Since this revision I have seen no formula published for the production of the official acid by the decomposition of bromide of potassium with tartaric acid. As the requirements of the Pharmacopœia give an acid representing only about sixty per cent. of the amount of bromine contained in my formula, and as I am quite sure, from a personal examination of the subject, that there is a great lack of uniformity in the strength of the acid being dispensed in this country, it would appear none too early to present a formula for the production of the official acid that is at once the simplest to adopt, and as has been shown by years of application, is free from objectionable features. The formula is as follows:—

Digitized by Google

Take of—

Bromide of potassium	four (4) avoirdupois ounces.
Tartaric acid	five (5) avoirdupois ounces.
Water	seven (7) fluid ounces.

Dissolve the salt in the water, and add the acid. When thoroughly mixed set aside in the cold, for the precipitation of the resulting bitartrate of potassium. Decant the supernatant fluid and dilute it with sixteen fluid ounces of water. The result is a ten per cent. solution, by weight, of hydrobromic acid.

Digitized by Google

FIFTH DAY.

The President in the Chair.

The following paper was read by title :—

COLLINSONIA CANADENSIS.

BY JOHN V. SHOEMAKER, A.M., M.D.,

Of Philadelphia, Pa.

Collinsonia Canadensis, commonly known as stone root or knob root, is one of the most valuable of indigenous American medicinal plants. It is widely distributed, being found in richly wooded soils, from April to October, in all sections of the United States.

It possesses a rank aromatic odor, and is hot and somewhat pungent to the taste. Its principal medicinal constituent appears to be a volatile oil, which is driven off by boiling or drying.

All parts of the plant may be used in medicine, but the root is the most powerful, and the portion usually employed. As it yields its virtues to water and alcohol it may be administered in the form of a powder or as the tincture, the fluid extract or the infusion. The dose of the powdered root varies from ten to sixty grains, that of the tincture from twenty drops to two drachms, the fluid extract from fifteen minims to a drachm, the infusion from one to four ounces.

The physiological action of Collinsonia has not been accurately studied. Small doses do not appear to produce any effect upon healthy persons, except a sensation of warmth in the stomach and bowels ; large doses produce diaphoresis and nausea, followed in some cases by repeated but painless vomiting.

Collinsonia is highly esteemed in many sections of the country as a domestic remedy for gravel and other urinary affections. Clinical observation indicates that its power is not limited, by any means, to this class of disorders.

It appears to be especially valuable as a sedative and antispasmodic. It is also possessed of considerable astringent and tonic properties. The popular belief in its efficacy in promoting the expulsion of urinary calculi is well founded. It relaxes the spasm of the ureters, or of the urethra, and by increasing the flow of urine, and by lessening the sensitiveness of the genito-urinary membrane, facilitates the expulsion of small concretions. When they are of large size, collinsonia is powerless to either dissolve or expel them, but it will alleviate the sufferings of the patient by diminishing the irritability of the bladder and urinary canal until complete relief is afforded by surgical methods.

Acute cystitis can be more quickly relieved by collinsonia combined with aconite and morphia, than by the administration of any other remedial agents. In two cases of this painful affection treated by me during the last four months in this manner, the patients were free from pain and fever on the fourth day, and discharged, cured, one

Digitized by Google

on the eighth and the other on the eleventh day. Cases previously treated without collinsonia invariably lingered from two to three weeks, and suffered for six or eight days from pain and vesical tenesmus.

I have not had an opportunity to try this remedy in chronic cystitis, but reasoning from analogy, it should be similarly efficacious in that tedious and troublesome affection.

Incontinence of urine in children due to spasmodic contraction of the bladder can be permanently relieved by the administration of one drachm of the tincture of collinsonia after supper and at bedtime, for several nights in succession.

Nervous individuals who may or may not have suffered from gonorrhœa not unfrequently complain that one or two minutes after having urinated and readjusted their clothing several drops of urine will involuntarily ooze out, staining their clothing and producing an unpleasant odor, which they imagine every one around them can perceive. This trivial but annoying affection is due to hyperæsthesia of the prostatic urethra or of the neck of the bladder, and can be effectually removed by the persistent use of the fluid extract of collinsonia, in twenty-drop doses, four times a day. In chronic gonorrhœa, when copaiba, cubebs and oil of sandalwood have failed to arrest the discharge, or have been rejected by the patient's stomach, drachm doses of the fluid extract of collinsonia, given every four hours, will not unfrequently effect a cure. Leucorrhœa and prostatorrhœa may be relieved or cured in the same manner.

Constipation, hemorrhoids, rectal neuralgia, and vague pelvic or abdominal symptoms are due, more frequently than is generally supposed, to spasm of the sphincter ani. Dr. J. M. Matthews, of Louisville, Kentucky, has shown that prompt relief can be obtained in such cases from division or forcible dilation of the refractory muscle. My experience, though limited to three cases, leads me to believe that the persevering employment, every night, of suppositories containing from forty to ninety grains of the powdered collinsonia root will, in many cases, render any operative procedure unnecessary.

The first patient was a woman, forty-five years old, who suffered for years from darting pains in the rectum, especially severe just before defecation. She had visited numerous celebrated springs, and consulted several regular and irregular physicians, without obtaining marked relief. When she placed herself under my care, I instituted a general tonic, anti-neuralgic treatment, combined with suitable laxatives. Her general health improved almost immediately, but the local disorder remained stubborn. Finally, suspecting the existence of an anal fissure or ulcer, I made an examination of the rectum, but discovered nothing abnormal but the vise-like grasp in which the finger or the speculum was held by the sphincter muscle. Suppositories composed of opium, opium and belladonna, opium and quinine, were then ordered, in succession, and successively failed to more than temporarily relieve. I then decided to either divulse or incise the sphincter, but being desirous to ascertain the relative merits of the operations, I ordered her, as a placebo, four suppositories, each containing thirty grains of powdered collinsonia, with instructions to use one every night, as usual. To my surprise and gratification the patient appeared at my office two days afterward, and informed me that "the last cartridges which I had ordered for her had given her so much ease, that she used two a day instead of one, and wanted more of the same kind, as she was convinced that I had found 'the right cure at last.'" I questioned her closely, and found that her relief was genuine and not assumed, and that it began within an hour after using the first suppository. I then ordered eight more, containing forty grains each, instead of thirty, and directed her to use one night and morning. She returned on the sixth day and told me that after having used five she felt so well that she did not think it necessary to finish the rest. The improvement was complete and permanent. During the year and a half that has elapsed since then, she has consulted me twice for other troubles, but the rectal pain has never reappeared.

Digitized by Google

The second case was a woman, thirty-five years old, and was similar to the preceding one, and was cured in a week by collinsonia suppositories.

The third case was that of a young man nineteen years old, who suffered from violent headache every second day. Inquiry revealed the fact that the headache was almost invariably accompanied by a feeling of weight in the abdomen and a desire to defecate, which could only be accomplished by much straining and repeated efforts, extending sometimes over a period of twenty to forty minutes. At the expiration of that time a large evacuation of the bowels usually occurred, followed by the immediate disappearance of the sense of weight in the abdomen, and the gradual cessation of the headache. The patient had taken all the varieties of pills in the market without any appreciable benefit, except when they left his stools thin and watery.

As defecation was not preceded or accompanied by pain, and usually relieved the other symptoms, I concluded that there was no ulceration or fissure present, and that the trouble was due solely to spasmodic stricture of the sphincter ani. I ordered him four suppositories, containing one drachm each of powdered collinsonia, one to be used each night at bedtime. He returned in four days as bad as ever, and inclined to doubt the value of "those things." I encouraged him to try again, and ordered six more, each containing seventy-five grains. I did not see him again for two weeks, when he came back and stated that when he had used four he felt so good that he threw the other two away, but that the trouble had returned and was now worse than ever. I then ordered eight suppositories, with ninety grains of collinsonia in each, and directed him to use one every second night, regardless of how well he might be. He obeyed these instructions faithfully, and during the five months which have since passed by he has not had any symptom of his former troubles.

It is more than probable that vaginismus or spasm of the sphincter vagina can be readily and safely relieved without resorting to the ludicrous or painful methods narrated in the text-books, by the continued employment of vaginal suppositories of collinsonia; opium, belladonna, chloral, conium, hyoscyamus, or stramonium may be added if advisable.

The antispasmodic properties of collinsonia render it of value in flatulent colic, infantile colic and biliary colic. It is especially serviceable in the latter affection, if given in the form of warm infusions, so as to thoroughly relax the biliary passages and facilitate the onward movement of the irritative calculi.

The most available preparation in colic is the tincture, the dose of which ranges from ten drops to half a drachm for children, and from one to two drachms, frequently repeated, for adults. Collinsonia alone will be found quickly curative in many cases of colic, when unaccompanied by rise of temperature. It will not relieve lead colic, or the pain of peritonitis or entero-colitis.

Collinsonia has proved curative, in my hands, in two cases of gastralgia, in which morphia, cannabis indica, belladonna and various other remedies only gave temporary relief. I also obtained markedly beneficial results from its use in five cases of dysmenorrhœa. Each patient was directed to take half a drachm of the fluid extract three times a day, for a week before the appearance of the menses, and two drachms of the tincture every four hours during their continuance. In the first and fifth cases the pain, though previously severe, was not noticeable; in the remaining cases it was so much lessened that the patients pursued their daily avocations as usual, one as a seamstress, one as a saleswoman, and the other as a telegraph operator, instead of being compelled to stay in bed for two or more days.

Ordinary colds and mild attacks of lumbago can be quickly broken up by taking a cupful of a hot infusion of collinsonia at bedtime. Spasmodic croup can be immediately relieved by the same means.

Collinsonia is an effective remedy in relaxation of the uvula, chronic pharyngitis, and

Digitized by Google

hoarseness dependent upon a lack of toncity of the vocal cords. The fluid extract may be given in these affections, in half drachm doses four times daily, and employed also as a gargle when diluted with four times its volume of water. It is also of value in gastro-intestinal catarrh and the catarrhal gastritis of beer and alcohol drinkers. It lessens the desire for liquor, restrains the secretion of mucus, and restores the normal tone of the alimentary canal, and reinvigorates the depressed nervous system.

Collinsonia is equal, if not superior, to cimicifuga in the treatment of chorea, and may be substituted for arsenic, with advantage, in many cases of that disease occurring in infancy and early childhood. In three cases recently treated with collinsonia, all traces of the disease disappeared in from two to four weeks. It is but fair to state that the same remedy failed to make any impression on two previous cases.

Collinsonia will be found palliative, if not curative, in whooping-cough, and may safely be given in that affection without the dread of disordering the stomach or producing any other unpleasant symptoms. It may also be given with confidence in nervous cough and the irritative cough of pharyngeal catarrh.

Collinsonia is also of value, in moderate doses, as a mild but certain general tonic, increasing the appetite, promoting digestion, and gently stimulating all the organs of excretion. It may be given with decided benefit in anæmia, chlorosis, incipient phthisis, and convalescence from the various eruptive fevers.

Externally it constitutes an excellent application to contused and incised wounds. Indolent ulcers may be stimulated to healthy action by an ointment consisting of one drachm of powdered collinsonia and one ounce of fresh lard.

Ascarides may be effectually destroyed by rectal injection, composed of the fluid extract diluted with four parts of water.

The following paper was read by title:—

COLD WATER AND ICE AS REMEDIES IN INFLAMMATORY AFFECTIONS.

EAU FROIDE ET GLACÉE COMME REMÈDES DANS LES AFFECTIONS INFLAMMATOIRES.

KALTES WASSER UND EIS ALS HEILMITTEL BEI ENTZÜNDLICHEN AFFEKTIONEN.

BY HIRAM CORSON, M.D.,

Of Conshohocken, Pa.

Sixty-three years ago an incident occurred—the death of a beloved niece, suffering with measles and tortured with hot teas, while begging for water, which filled my young heart with sorrow, and turned my attention to the value of cold water, not only as a drink, but as a remedy in disease. From that day to this, as the years have rolled on, I have carefully and anxiously observed the effects of cold water and ice in all the diseases here noticed, as they have presented themselves.

In entering on the practice of medicine, in 1828, in defiance, of the traditions handed down to us through ages, that cold water as drink in measles was injurious, and that hot teas and warm rooms were efficient to promote the eruption, I rejected these traditions and the practice founded on them, from a belief that the instincts of the sufferer and the highly inflammatory nature of the malady demanded the use of cold water as a beverage, to cool the heated system, and that cold air, and, when needed, sponging the

Digitized by Google

surface of the heated body with cool or cold water, would be means of great value. So, though I knew not of a single physician who gave cold water as drink in measles, or allowed the patient to be in a cool room, I began its treatment by forbidding all warm teas and *urging* the use of cold water as drink. My directions were, "give the children as much cold water as they can drink; keep the room cool; if they become very hot, restless and uncomfortable, sponge the surface with cool or cold water, until they are relieved." How grateful this was to the little sufferers, no one can so well appreciate as those who, having been tortured with hot teas, have been relieved from their use and allowed plenty of cold water. But little medicine was given, and now, in fifty-nine years, not one of my hundreds of cases thus treated has been lost by death. For a history of these cases see a paper by me, in *Philadelphia Medical and Surgical Reporter*, May 16th, 1872.

Even yet patients in many places fall victims to the hot treatment. Three years since a fine, strong boy of 12 years was brought, dead, from Philadelphia, thus treated; by positive direction of the physician forbidden cold water, and kept in a hot room. And this very year, August 27th, a lady from Philadelphia who had taken boarding for herself and child with a family near to me, where measles prevailed, told me she had asked her doctor what she should do if her child should get them; he replied, "keep it warm." So it seems not inappropriate to present the subject here. For many years this cooling treatment was strongly opposed by physicians in my region. Long ago I brought it before our County Medical Society, from time to time, until finally the members became divested of their fears and joined me in its use and its praise; and here let me quote what was said by our American Dr. John Bell, in his work on "Baths and the Watery Regimen," published in 1850, in Philadelphia. "In the continued use of cold water internally, by drinks and lavements, and externally by bathing or by topical applications, the physician has a means of diminishing and removing inflammations, both of the internal organs and of the external surface and the joints, as also fevers, hemorrhages and all diseases associated with much vascular action and nervous excitement." This declaration is so in accord with my own half a century's experience, that it gives me pleasure to say that it strengthened my determination to carry this principle of treatment to the relief of inflammations wherever found in the human system.

SCARLET FEVER

Is another disease in which the cold or cooling treatment is indispensable, if the comfort and safety of the patient are worthy of consideration. In 1830 I first encountered the scarlet fever, and at once pursued the same treatment as I had in measles, by cold drinks and, in severe cases with much eruption and great heat, by cool sponging. In 1833 I began to use ice in the mouth and ice water as drink, while physicians near to me continued the use of hot teas, gargles of cayenne pepper infusion, poultices to the outside of the neck, sometimes fat bacon, etc. The only difference in our treatment was that I used cold drink and cool sponging, they used irritant gargles, hot drinks, and kept the patients in heated rooms. The poultices to the outside of the neck were used by us all until 1844, when, disgusted with the use of poultices, so often followed by destructive suppuration of the glands under the jaw and discharges from the ears, and believing that I could hold in check the inflammation of the submaxillary glands by cold applications, I began the use of ice externally to the swelled glands of the neck. In that year, 1844, I was called to a boy of eight months, sick two days. There were great enlargements of the glands on both sides of the neck, hot skin, frequent pulse, slight discharge from the nose, the tonsils inflamed, drink came back through the nose, and the child could take but a single draw of the breast without dropping the nipple, because of the obstruction in the nostrils impeding respiration when the mouth closed. I had never saved a child so young, thus affected, by the poultice treatment,

Digitized by Google

and prevailed on the mother to let me use ice. So with small pieces of ice in gauze slipped into its mouth and lumps of ice lapped in bits of muslin and held continuously over the swelled glands, I anxiously watched the case. I had never known the ice to be thus used. In a few hours improvement was shown in greater ability to swallow; the swelling of the glands, heat of skin and frequency of pulse steadily diminished, and in two days the child nursed well and was out of danger. Do I hear you say that you can scarcely realize that the relief of this merely *local affection* could have so speedy and decided an effect on the general system? No, you did not say it; my dull ears misled me. You doubtless believe, with me, that there is never a fever in the human system which is not based upon (caused by) a local inflammation. This is not a new idea.* In 1832 the distinguished Dr. Jackson, of Northumberland, Pa., when the life of his daughter, in the fourth day of her illness in scarlet fever, seemed trembling in the balance of life and death, ventured to give her, instead of hot teas and stimulants, some cold water and small pieces of ice, wrote as follows: "My anxiety became intense, for I was possessed of the opinion that on the speedy improvement of the *local disease* depended the fate of my child. Cold water she desired above all things." He gave it. Hear him again: "She drank the coldest ice water and held ice in her mouth, and now for the first time I was satisfied with my prescription; the good effects were immediate, surprising, incredible, almost divine! Within a few hours the pulse was reduced from 160 to 120, the circumscribed crimson disappeared from her cheeks, the extremities became warm as the fauces became cooled, the whole countenance was changed, the typhous distress left it and something of the vivacity of common fever supervened." Afterwards he wrote: "I have had ample opportunity to test this remedy. In more than forty cases I have found it highly useful, and *without it I am certain more than one-half would have been lost.*" This from its internal use alone.

My praise of its results in my little patient's case, where I used it both internally and externally, should not be considered excessive. For more than fifty years I have used the ice and ice water internally to allay inflammation of the tonsils and pharynx; and for more than forty years, ice externally over the affected glands, in every severe case which has come to me, and with a success pleasant to remember. In 1857 a severe epidemic of scarlet fever occurred in the upper part of this county and the lower part of Berks. In every direction many cases were lost by those who held to the warm treatment. Some reported losses from "congestion of the brain," others from "effusion;" one "lost six from congestion, and believes no treatment has any power to save such cases." I had that winter more than a hundred, in which there was great tendency to disease of the brain; and this brings me to speak of another mode of applying cold, namely, by *affusion* of cold water on the head. To this I resorted in all cases where congestion or delirium was feared or present, and in this way, with ice and ice water in the mouth and for drink, and ice externally over the glands, and by affusion of it on the head when needed; not one case was lost to me that winter. The cases were severe and the treatment carried on heroically. It was no child's play. Boasting is it? Looks greatly like it; but a number of sharp, intelligent physicians practiced near to me; our Society met every two months; every year, sometimes at every meeting, my ice treatment and its success were reported to men not of my faith; had they known a single death in my practice they would have reported it, as they were desired by me to do. Have I lost cases since? Certainly; several; some of whom would have lived had the attendants been faithful to the directions. Parents knew that there were physicians opposed to my practice; they used the remedy timidly, sometimes, in my absence, not at all.

---

* The idea that the fever is reduced in exact proportion to the reduction of the local disease on which it is based.

Digitized by Google

Many physicians are embarrassed to know how to apply the ice. Allow me to quote from a paper published in 1864: "A piece of muslin twice folded, just long enough to reach from ear to ear, should be applied under the jaw, and pieces of ice slipped between the folds on the sides, over the glands, and the whole be secured there by aband of muslin tied *on top of the head*, not around the neck. It should be closely and continuously applied; removed but the thickness of a knife blade, it wouldbe almost useless. If too cold for a young child, two more thicknesses of muslin may be interposed. Some fear the melted water dripping on the breast "will give the child cold." Instead of that it is delightful to the heated child, and as the body is above the normal temperature, no chilliness is caused by it.

How shall the affusion of cold water be used? Allow me to cite two cases:—

CASE I.—In July, 1845, I was called to a girl four years and nine months old, who had been ill a day or two. She began with vomiting; the eruption had been out since morning (then 6 P.M.); the redness of the surface was as intense as I had ever seen; the pulse as rapid as it could be to be counted. The mother had been alarmed during the last few hours in consequence of delirium and jerking, which she feared were the prelude to convulsions. There was much swelling of the submaxillary glands, tongue furred, with projecting points, breath hot and offensive, and when she found some one holding her wrist, she started from her dozing state into one of the most terrific convulsions that I had ever seen. It lasted, in spite of ice water constantly poured upon her head, almost half an hour. I stayed with her, had her stripped and laid on a narrow cot, then placed two of my nieces by her side, had a large tub of water with cakes of ice floating in it, brought into the room, and during the whole night these two persons bathed her from head to feet with this water, applying it profusely by means of large sponges. It was to me a most painful case, and in order to be sure that it was one fit for the trial of the ice, I sent for my brother, then practicing in Norristown, only four miles away, where the disease prevailed and was quite fatal, to see whether it was the same kind or type of disease as that which, for some weeks, had been carrying off the children of Norristown and spreading terror among its citizens. He assured me it was a case of the most violent character, and that she would in all probability not live till morning. At that time, 11 P.M., she was free from convulsions but in a muttering delirium. As I had entire control of the case, he was assured that she should live if the fire that was burning up her vitals could be quenched by the use of ice and ice water. Not a moment did these nurses whom I had placed by her side intermit their labors. Before midnight reason had returned, and her mother, one of the nurses, said, "she is more herself than she has been during the whole day." I left, but returned at sunrise and found her cooled off perfectly. There was scarcely an appearance of eruption, the head was cool, intellect clear and the pulse moderate in frequency and force. She had been unable to drink for many hours, and the tongue, which had been cut during the convulsions, was swollen, so as to obstruct a view of the throat. Directed the mother to sponge her only once every two hours, until my return. My return was delayed until 4 P.M. The heat of skin, eruption and delirium had all returned, and she was moving her hands as if feeling for something, slowly protruding and withdrawing her tongue and muttering; took no notice of her mother's questions, and was unconscious of all that was going on about her. We threw on ice water in utmost profusion, and lapped cloths dipped in the water around the neck, changing them every few minutes, and poured it on her head constantly, in a small stream, from the height of a foot or two. In an hour reason returned. We continued it until the eruption almost disappeared, until the child shrank from it and was ready to shiver with cold. We then gave medicine to move the bowels, directed the water to be used as needed to keep down the heat, and afterward had no further trouble with her. I forgot to say, that as soon as she could swallow, the ice and water were given as drink and to cool the inflammation of the tonsils and fauces. She took no more medicine. The other

Digitized by Google

two younger children of the family were attacked, but the disease was held in check by the cooling treatment and recovered speedily.

A case of affection of the brain (convulsions) in a child ill with scarlet fever and treated successfully, more than twenty-five years ago, may be found in Dr. Meig's "Diseases of Children," published in 1848, and with the following case will show my mode of using affusion:—

CASE II.—Was called to a child, two years old; sick two days; found her, at 8 A.M., pale, skin cool, pulse barely perceptible; eyes wide open; pupils dilated; no evidence that she could either hear or see; suspected brain disease from scarlet fever infection—and here let me say that, when the brain becomes early affected, the eruption will not appear until the brain shall have been somewhat relieved. The body was wrapped in blankets, and cold water, from a height of two feet, poured from a pitcher, on her head, moving it continuously from place to place with brief intervals, until noon. Pulse then stronger, readily counted at 140, body warmer, pupils insensible to light. Affusion continued. Next morning, at sunrise, found her with sight and hearing restored, and a bright eruption covering her body. She convalesced without further trouble.

Allow me one more case, a brain affection from scarlet fever in a puerperal woman. Mrs. F., delivered of her fourth child November 7th, 1881. Her physician saw her on the 8th and 10th, and left her not as well as is usual for that time. On the 14th, he being away, I was called. She had been nearly four days poorly and with scarlet eruption; had pain in the head; was extremely restless; pulse 130, as nearly as could be ascertained; her restlessness and tossing of her arms precluding accuracy. A physician acting for me was directed to give her the cold treatment, regardless of her recent accouchement. "Never mind that she has just been confined; disregard the name of scarlet fever; aim to relieve the head by pouring cold water on it, after our manner of doing it; repeat till the delirium is relieved." Suffice it to say, that it was done heroically, and the patient saved. The case is given in detail in *Philadelphia Med. and Surg. Reporter*, of May 27th, 1882.

I may have spent too much time in showing the value of cold as a remedy in scarlet fever, but rarely has any one made a more careful trial of any remedy than I have made of this, in a practice of fifty-nine years, under the daily watch of intelligent physicians, anxious no doubt for my success, but doubtful of the propriety of my practice, and too timid to resort to measures, to them, so heroic. And now, in closing my career as a practitioner, and looking back on the countless fights had with death, in hovel and in palace, I can truthfully declare that no means ever used by me, or which have ever been known to me as used by others, in scarlet fever, have so successfully warded off his blows and shielded patients from harm and restored them to health as the use of cold water and ice.

My papers on scarlet fever and cold may be found in Pennsylvania State "*Transactions;*" *Montgomery County Reports*, of 1857, '62, '64, '67, '71, '72, '76; in *Medical and Surgical Reporter* of Philadelphia, January 18th, '73, October 21st, '71, March 18th, '76, May 2d, '82; in Philadelphia *Medical Times*, January 4th and 11th, 1873.

DIPHTHERIA.

As I have treated scarlet fever, so have I treated this disease. For many years, in my practice, the temperature was guessed at, as we had no clinical thermometers. Now, we can determine it so accurately that we need not err as to the need of reducing it. My treatment of the two diseases has been the same. When I speak of the great value of the cold treatment the severe cases are in my mind. It is in these dangerous ones that the remedy is so valuable. The physician then holds in his hands a remedy adaptable to every condition of the case. Besides this remedy, medical treatment so called was, of course, used by me, as called for by symptoms.

Digitized by Google

SMALLPOX.

The treatment of this, like the treatment of scarlet fever, should be of the cooling kind. In the very beginning the system should be kept cool by exposure to cool air or by sponging with cool water, and by light diet, and by cold drink freely used. These measures will insure a mild case. In the year 1840 was called to a young man covered with vesicles. His sister had died two weeks before; he was very delirious, face and head much swelled, and he was making attempts to get out of bed. I fairly enveloped his head and the back of his neck with bladders of ice and water, and continued these measures until his delirium was subdued; he convalesced well. In the graveyard of Boehm's Church there are, side by side, graves of five children of one family, of ages from five to fifteen years, all dead from smallpox, treated in the hot way in 1750–6. I have had in all my time not more than from fifteen to twenty cases. All did well but two, and one of them would not allow the cold treatment to be used at all. He lived alone, laid himself down near to the hot stove and staid there till he died. The other I did not see early; he was kept from home when first poorly, by snow storm. The others were managed without difficulty. In Philadelphia, at the Municipal Hospital—a smallpox hospital—in 1871, according to a report to the State Society, there were admitted 677 cases, of which 353 died. This was the treatment: "From six to eight grains of quinine, one drachm tinct. ferri muriatis, one quart milk, made into egg-nog, containing six or eight eggs, and six or eight ozs. of whisky, often increased to ten or twelve ozs. and one quart of beef tea. All this to be taken every day, in the vesicular stage and onward.

INFLAMMATION OF THE BRAIN.

After what I have already said, it seems unnecessary to give cases to show the value of affusion of cold water here. Dozens of cases have presented themselves, and the copious affusion of cold water—generally afterward—has been a means of inestimable value. To be efficient it must be done without stint. Whatever is worth doing, is worth doing well. I have not referred at all to the experience with cold, of John Smith, of Pitcairne, Hancock and others, who were advocates of its employment long before any man in this audience was born; nor yet of Currie or Gregory, of later time; but in 1847, Dr. Henry Hartshorne, of Philadelphia, published a valuable "Essay on Water and its True Relations to Medicine," from which, on page 68, allow me to quote the following utterances of Dr. Southwood Smith, in a case of fever, with marked cerebral affection and great heat of surface, and in which general and local bleeding had been already used. "Recourse," he wrote, "was had to a measure, the effect of which is but little known and less appreciated; a remedy, the power of which is second only (if under some circumstances it be not superior) to that of the lancet; a remedy which can never supersede the lancet, but which when added to it, forms by the combination a treatment so powerful and efficacious, that it might render death from the acutest cerebral inflammations as rare as recovery is at present. It consists of pouring a column of cold water, in a continued stream, from a height of from six to ten feet. "After the patient has been wiped dry and placed in bed, the symptoms may soon return in all their violence. The same process will again remove them, and as often as they recur, the process must be repeated. Three or four repetitions will commonly suffice to subdue the most intense cerebral affection." My long and thorough experience with a daring use of this remedy justifies me in making this language my own.

NARCOTICS.

Affusion of cold water on the head is not a new remedy for opium poisoning. Christison says it was proposed by a German physician in 1767, and Septimus Wray, in the *London Medical Repository*, gives three cases of opium poisoning successfully

Digitized by Google

treated by it. From a paper in the *American Journal of Medical Sciences*, April, 1852, by that excellent practitioner, Dr. Jesse Young, of Pennsylvania, I quote the following cases:—

CASE I.—"A child in its second summer; narcotized almost to death; countenance blanched and all power of muscular contraction entirely suspended; could not be aroused by any means then tried. I poured the coldest spring water, in a continuous stream, perhaps a gallon, on the crown of its head, when it began to show some signs of muscular contraction; I continued it a little longer, when it roused up, cried lustily, kicked and plunged about so that we desisted. It recovered without difficulty."

CASE II.—Negro girl, aged 18; had taken two ounces of laudanum within two hours, and had had it for several hours. "We had her held over the tub by an assistant, with the face down, the cold water poured on as before, and in less than five minutes she was completely restored."

No case of opium poisoning ever came to me, but a friend to whom I had spoken of the remedy had a case, which he soon restored, then went away without ordering the affusion to be repeated should coma return; it did return and he was allowed to die This should always be remembered; that in such cases, though the narcotism has been counteracted by the affusion, it will soon return if suspended, and will require to be renewed from time to time for a few hours, till the poison is eliminated from the body.

In two children poisoned by eating green stramonium stalks, after an emetic, the distressing delirium peculiar to this plant was speedily relieved by pouring cold water upon the head.

Allow me another case: A married woman of middle age, after a few days of suffering from an erysipelas of face, became deeply comatose. I was called after she had been wholly unconscious for many hours. Her head was brought over the tub at the side of the bed, in which great lumps of ice were floating in water; the water was poured from a pitcher, in a large stream, for a full quarter of an hour, before she was fully restored to recognize friends. It was then evening and I left for home, eight miles away, but not till I had impressed on the attending physician the importance of repeating the affusion occasionally, till the poison be eliminated. He had it done until he left for home—a brief time—then the family failed to do it, as they regarded her apparently only sleeping; thus she was allowed to drop into deep coma, and she died next morning. But even here the power of the affusion to remove a profound coma was well shown.

Two cases of the most profound coma, produced by whisky, in children, were restored by affusion in a few minutes, by me.

I am not ignorant of the practice which now prevails, of giving medicine to antagonize the effects of the narcotic, and which occasionally fails; but the affusion is so certain in its effects that it should always be borne in mind, as it is always at hand, while the medicine may not be. In the *American Journal of the Medical Sciences*, May, 1829, is reported a case of the deepest narcotism, caused by prussic acid, which it is likely no one of you has ever heard of, so allow me to give it to you. Dr. Herbst says of this poisoning: "Affusion can be relied upon when used immediately after the acid or during the convulsive stage, even as long as the eyes are insensible to light, or immovably fixed, the extremities and the head thrown back. To this state there succeeds relaxation of the muscles, the respiration becomes gradually imperceptible, the pulse slow, weak and scarcely to be felt, and in an instant afterward death supervenes. Even at this period of paralysis the cold water recalls the vitality ready to vanish; the muscles again contract and become hard, the limbs rigid, and everything resumes its natural state." Could any agency do more than this?

Digitized by Google

APOPLEXY.

Of the value of this remedy in threatened or even present apoplexy, I have no doubt, but yet have seldom ventured to use a remedy, which by no possibility could be as prompt as venesection. The latter, therefore, with me, precedes the affusion. The two, where there has not been too great an effusion of blood within the cranium, are measures which I have seen act as by magic. Henry Ward Beecher's case was one favorable for their use. It was a slow one, heralded by unmistakable symptoms, and yet day after day passed, hour by hour the dullness increased, and nothing stayed its deadly progress, not even daily "bulletins."

COUP DE SOLEIL.

This disease is said to differ from apoplexy. It is not one recently discovered, nor is the treatment by affusion of cold water a new remedy for it. In 1829 Dr. Samuel Dickson, of Charleston, S. C., published, in the February number of the *American Journal of the Medical Sciences*, a paper "On the Effects of Heat," in which he says: "Of coup de soleil there are two forms, the apoplectic and phrenitic," or cerebral and meningeal; and of the former he writes: "They fall, after exercise and exposure to great heat, senseless and motionless, the breathing stertorous, pupil of the eye insensible to light, the skin hot, but no hotter than the natural standard, pulse full and slow. Unless speedy assistance was brought, death ensued in an hour or two. The apoplectic was most fatal, and in both forms the lancet was first tried." Of the phrenitic or meningeal form he thus wrote: "All that could be hoped for from venesection at this period was as perfectly and surely effected by cold affusion. He who has not learned the efficiency of this simple remedy in cerebral affections may add another important agent to his list." "The patient raised to a sitting posture, cold water should be poured on his head, from a height of a few feet; the flushed face then will become pale, the hard, quick pulse will sink to a mere thread, and the coma and stupor will rapidly disappear." *Again and again the symptoms returning will call for a repetition of the affusion.* Italics mine. My object by this emphasis is to impress this fact, observed so long ago, and corroborated by every careful observer since. Thus fifty-eight years ago my attention was turned by Dr. Dickson to this mode of treating not only coup de soleil, but also congestions and inflammations of the brain, from whatever cause, and the results of the treatment have been most gratifying. Thirty years ago the engineer of the Spring Mill furnace worked in a small basement room, a hot place, in which he spent twelve consecutive hours every day during every other week; the alternate week his nights were spent in the same heated place. One day he was found in a comatose condition and died in a few hours. Another, who took his place, was a victim to the heat in the same way. A third man was overcome, but was gotten to his home; then his nose began to bleed, which soon relieved him, he became conscious, but as the bleeding continued he became greatly weakened, and I was called. Found him much exhausted; efforts were made to arrest the bleeding, but without success, until a stream of water was poured on the lower part of the forehead and over the nose, which soon arrested it. After a short time the bleeding began again, but was promptly stopped by a renewal of the remedy. Thus for many hours we held it in check, until the overheated system became cooled, after which, *without* stimulants, he was soon restored to health. What were these cases? Coup de soleil? Thermal Fever of Professor Horatio C. Wood? Apoplexy? It matters not what the name. The affusion would, no doubt, have been applicable to them all: and the epistaxis in the last shows that venesection, followed by affusion, would, in such cases, be even better than either remedy alone. How many people have been sacrificed to the stimulant practice! A practice not yet entirely abandoned. The paper of Dr. Dickson is quite worthy of perusal by those who fancy that the stimulant treatment is so indispensable. In all cerebral affections tending to reflex and coma I have used affusion, with best results.

Digitized by Google

PERITONITIS, PHLEBITIS, ETC.

Peritonitis.—In this disease cold is of great value as a local application. It is easily applied, and because of its nearness to the diseased surface, quickly efficient. In February, 1845, was called to Mrs. Aaron, aged 30; no children; lying on her back, limbs drawn up, belly tender to the touch and greatly distended. It was winter time, and the ground covered with snow. Masses of it were put between single thicknesses of old muslin and laid over the whole abdomen, renewed whenever needed, for two days and nights, with great comfort to the patient and relief from the malady. From that time to the present I have used it in all inflammations of the peritoneum, as a local remedy, instead of turpentine stupes, poultices and blisters, all of which are so unpleasant to the patient and inefficient, if not really hurtful.

PELVIC ABSCESS.

I was called, in 1850, by Dr. Martin, to see Mrs. Wertsner, and found her pale and weak from long suffering under the hot poultice treatment. Suppuration was evident; an opening made with the lancet gave exit to the pus and she slowly recovered. Since that time, in 1866, Mrs. Davis, some days after her confinement, had chills, followed by fever, with pain in the left inguinal region; her left leg was drawn up, and there was swelling and tenderness over an area of several inches in that region. The location and symptoms reminded me of Mrs. Wertsner's case and her suffering under poultices; also of Mrs. Aaron's case, in which the snow did so well. Two thicknesses of old wet muslin were laid on the part, and on that a bladder of ice and water, and kept there night and day for several days. Relief from the heat and pain was soon experienced, and in a few days she was well, without suppuration. Many physicians fear to apply the least amount of cold to a puerperal woman. There is not a particle of danger *while the part to which it is applied is above the normal temperature, or while the general temperature of the body is above its natural temperature*, but a certainty of comfort and relief being produced by it. In *crural phlebitis* I have twice applied it, with good results, in recently confined women. In peritonitis, where the inflammation is so near to the applied ice, and the cooling influence of the latter felt almost as soon as applied, it is a certain and efficient remedy.

TYPHLITIS.

This disease, so insidious in its approaches, so often fatal, is one in which ice is very efficient, besides being from the first hour of its application pleasant to the patient, who feels that it is doing saving work. August 14th, 1848, I was called to a friend ill with this disease, and despite "appropriate remedies," under the persistent use of poultices, he died October 1st, though Dr. Isaac Parrish aided me in the treatment. It saddened me greatly when, on examination after death, the parts were found gangrenous; the fæces had passed through the side of the bowel down among the glutei-muscles. When, seven years afterward, in 1855, I was called to a boy of eleven years of age, on his back, with legs drawn up and a dull swelling occupying the region of the ileo-colic valve, the fate of my friend came before me, as did the case of Mrs. A. and the snow. Regarding the swelling, tenderness and pain as evidence of a serious inflammation in a dangerous locality, the ice was at once placed on the affected part, because elsewhere the boy was well. I well knew that if the cold would remove that inflammation, my patient would be well. Opium was given, to allay pain, the ice continually applied, and next day he was much better; not so sore; not so much pain; the skin where the ice bag lay cold, almost as the bladder of ice and water; the patient not restless but comfortable under his ice cushion. In a few days he was well. I have often wondered what would have been the result had President Garfield been put on a cushion of ice and water and kept heavily under the use of opium (to allay pain) and nothing more. You say something more? A proper solution of corrosive sublimate injected into the wound a single time,

Digitized by Google

to kill the microbes, and a plaster over the wound to keep them out? Well, perhaps so; but I think it would have been rather cold for them to operate there. The ball lodged so near the surface that the ice would probably have prevented the inflammation and averted suppuration. It is pleasant to have a hobby? Yes, and more *useful* than to have no thought, and blindly follow teachers. Since the success in the boy's case, I have resorted to it in every inflammation of the abdominal organs which has presented, and with good results.

GASTRITIS.

In 1835 Mrs. R. began to reject her food and drink, and after several days of vain efforts to prevent it, I was called to see her. She could not keep either food or drink but a few minutes. It was a plain case of gastritis, and she was at once given small lumps of ice to swallow; the effect was quite marked; no vomiting of it occurred for many hours, while occasionally using it. Then we tried some mild food, which was soon rejected. Thus we went on during several days, occasionally making trials of food, before she was able to retain any. Herself and friends thought she would die of starvation, but there seemed to be no good reason why we should change the treatment, inasmuch as she was comfortable under its use. The ice was broken into pieces small enough to be swallowed, and it was amazing to me to see her swallow one piece after another, until a teaplateful of them would be used at a single meal. Seven days from the time she began the ice she was able to take food in moderate quantities, and soon recovered perfectly.

Ever since that time, in my cases of gastric irritation and persistent rejection of food, resort has been had to the ice. Ten years since, a physician, very ill with gastritis, had had a warm poultice over the epigastric region for a day or two; with his own consent, at my suggestion, it was removed and the ice applied. He has since told me that no one who has not experienced it can have a full appreciation of the relief which it brought to him. The poultices made him feel as if he could not get his breath properly; he was hot, oppressed and uncomfortable. The change from the poultice to the ice was delightful.

GLOSSITIS.

In May, 1864, was called by Dr. Vanartzdalen to see Isaac Rozelle, who had been during several days ill, with inflammation of the tongue. Dr. Vanartzdalen was called two days before my visit and found him under a heated steam treatment. He was desirous to apply ice, as he knew was my practice, to the inflamed tonsils of scarlet fever, but they all feared the change from such a heated condition and the steaming was continued. It was my first case of that disease, save that which occurred occasionally in salivation. His tongue was enormously swollen, separating his teeth in front, his voice a mere whisper, his whole body enveloped in steam. The kettle of boiling water had been kept on the stove night and day, so that the vessel under the blanket which entirely covered him might be refilled when needed. He had a feeling of impending suffocation. The treatment by incision or by leeches was not regarded with favor, as the latter were not accessible, and the former too dangerous, from hemorrhage. The hot water and blanket were taken away, and though he was in a profuse sweat from the feet to the top of the head, the ice was applied in large quantities, under and on the sides of the jaws, and small pieces were stuffed into the mouth, wherever room could be found for them. It was then evening; the next day our patient greeted us as though he were the happiest of men. He could talk; before he could but make the faintest whisper. From the first hour the ice gave positive relief, the swelling subsided rapidly and the cure was complete. A more grateful man never convalesced from a disease of danger.

Digitized by Google

MUMPS.

I have used the ice-water cloths where the swellings were large and painful, with comfort to the children and without causing metastasis to other parts.

ORCHITIS, AND HERNIA HUMORALIS, FROM GONORRHŒA,

Have been treated by me most successfully; and this reminds me that in gonorrhœa the injection of cold water into the inflamed canal, and application of cloths wet with ice water around the penis, is considered good practice by some physicians. Hard on the germs though.

INFLAMMATION OF THE MAMMARY GLAND—MASTITIS.

In 1884 I gave up the poultices and began the use of ice in this disease. It was a grand success. It is impossible for the inflammation to cause suppuration if the ice be applied before that result has occurred. It is, too, a most agreeable remedy. My first paper on this treatment may be found in the *Philadelphia Medical and Surgical Reporter* of August 16th, 1878.

CROUP

Is an inflammation of the lining membrane of the upper part of the windpipe. In the early stage, before the strong fibrinous membrane has been produced by the unchecked inflammation, the application of ice to the front of the trachea will almost certainly prove quickly curative, because the ice is so close to the inflamed tissue that the inflammation cannot continue and extend, but will as certainly and speedily be arrested as it is in mastitis. Many practitioners have from time to time spoken of its great value, and I can bear testimony of the same character. It is amazing to know that turpentine and other external irritants are still persistently used. How do these things act? Who ever has seen relief produced by them? Scores of times I have seen children thus tortured without an iota of relief. Laryngitis is, too, subdued by the application of ice in the same way. In what is called hoarseness from bad cold, I have seen prompt and pleasant effects from the same treatment, and in quinsy have used it for the last thirty years with the best results. It is, too, with ice water in the mouth, agreeable to the patient.

PNEUMONIA

Is an inflammation of the lung. The fever and pain are caused by the local disease. Just in proportion to its extent and severity will be the gravity of the general symptoms and the danger. If the local affection can be arrested, the suffering and danger will disappear. Believing, as I do, that every febrile condition of the system is caused by a local irritation or inflammation, and knowing from much experience how efficient the cold is when applied over the diseased lung, to aid in the arrest of the extension of the inflammation, I apply it with the utmost confidence that it will be pleasant to the patient and aid in his recovery by reducing local and general heat of the system. A few winters ago, two boys, fourteen and sixteen years of age, were ill with this disease, and the ice cloths and bladders of ice and water were freely used. They both said the application was most pleasant; the great sense of heat giving way to a cool, agreeable condition. I have used this remedy often; always with comfort to the patient and satisfaction to myself. You think poultices are better? You have never tried the cold. For nearly forty years I had experience with the blisters and poultices, and know how they caused the patients to suffer, and of what doubtful value they were.

TYPHOID FEVER.

More than twenty years ago I was called in haste to one of my patient's, who, on a previous visit, was doing well, and found her pale and prostrate, from an enormous hemorrhage from the bowels. A bladder filled with ice and water was at once placed

Digitized by Google

over the right ileo-cæcal region; the discharge soon ceased and she went on to recover. "She might have gotten well without it?" That is, perhaps, true. But it shows that the cold, so much dreaded, is not objectionable. Two years since was called to a similar case by a friend. A young married woman had an exhausting hemorrhage. Bladders of ice and water were applied. The pale, prostrate woman, worn down by her two weeks' illness and the hemorrhage, was not chilled, but comfortable under its use. I am not ignorant of the common belief that this crisis, as it is called, is a relief in some cases; but that there is no danger in the use of the cold over the inflamed area, is an important fact, and was well shown here, the patient being comfortable under its use and recovering speedily. For many years, whenever in this fever soreness was manifested in the right iliac region, I have had ice applied there, to allay irritation and avert hemorrhage; and, doubtless, with success in some cases, as none have since occurred, where the ice and ice water were freely applied over the tender abdomen.

In 1879, four adults of one family had typhoid fever; three suffered from much bronchial affection, with a tenacious, slightly bloody expectoration, with a sense of heat and oppression. Cloths dipped in ice water were freely applied over the front of the chest. The cold was agreeable, relieved the unpleasant sense of heat, and all recovered. Whenever, in this disease, the fever is high, my practice has been to sponge the body and limbs with cool water, and to give plenty of cold water as drink. The poultice is simply digusting, oppressive and useless.

RHEUMATISM.

I have never seen a metastasis of this disease to some other part caused by cold applications to an inflamed rheumatic joint. My patients have used them many a time, with great comfort and entire safety. There lives near to me now a woman of fifty years, who, when under twenty, was suffering greatly with rheumatism of the ankles; they were so hot, swelled and painful that she was advised by me to put them into a bucket of cold spring water. It was most comfortable to her, and since that time, whenever attacked, she resorts to that means of cure. To joints where it can be readily applied, I invariably have it used. Here let me call attention to the tradition so firmly fixed in the mind of nearly all practitioners, that "cold applications to the inflamed joints in rheumatism will drive *the rheumatism* to the brain or heart." What is it that will be driven? What is rheumatism? The heat, the swelling, the pain of the joint? And is the heat of the system, the fever, so-called, caused by the inflamed joint? Yes, that is my opinion. You think it a constitutional disease, do you? Well, what of it, if it be so? Shall I abstain from allaying the inflammation in the joint; allow it to torture the patient and keep up the fever, from a mere fancy that the disease is in the blood, possibly a germ, and that the cold, by heading off this outbreak in the joint, may compel it to seek an outlet elsewhere? Relieve the joints and your patient will soon be well. Be your general treatment what it may, aid it by the local application of cold to the affected joints.

In conclusion, the *fevers*, so called, claim attention. I group them, and say the general reduction of temperature by cooling drinks and cool sponging with water, and special local applications of ice or ice water to inflamed organs or tissues, to aid the judicious use of medicine, is good practice. But there is yet a question which you will be quick to ask me, namely, Where, in fevers, is the local inflammation which causes the fever, the increased temperature? And I shall answer as soon as the questioner proves that no local disease exists in these fevers.

Digitized by Google

The following paper was read by title :—

THE APPLICATION OF ALCOHOLIC STIMULANTS TO MEDICINE, HAVING ESPECIAL RELATION TO THE THERAPEUTICS OF ALCOHOL IN DISEASE.

APPLICATION DE STIMULANTS ALCOOLIQUES À LA MÉDECINE, AYANT UN RAPPORT SPÉCIAL À LA THÉRAPEUTIQUE DE L'ALCOOL DANS LES MALADIES.

DIE ANWENDUNG ALKOHOLISCHER REIZMITTEL AUF DIE MEDIZIN, MIT SPEZIELLER RÜCKSICHT AUF DEN THERAPEUTISCHEN NUTZEN DES ALKOHOLS IN KRANKHEITEN.

BY EDWARD N. LIELL, M. D.,

Of New York.

The subject of the application of alcoholic stimulants to medicine is of such vast importance, and the expression of opinion among medical men as to its application and utility in disease so varied, that a few words upon my part, I trust, will not be amiss in endeavoring to place the matter before you in, as I deem it, its true sphere of usefulness. I shall have reference, and concern myself in this paper only, to the indications presented for its use in disease; in fact, to deal with the therapeutics of alcohol in disease, leaving out of consideration all other questions pertaining to it.

There are those who to this day will be found enjoining the practice of abstention, believing the administration of alcohol to be wholly unnecessary; others are more guarded in the matter, in that they believe it useful in certain forms of disease; still others, and at the present time by far the greater number, take a more pronounced view and advocate the administration of alcohol, or the principle of judicious stimulation, to be legitimate, and the only rational course to pursue, especially in acute febrile disease.

But why should these varied opinions or disagreement of facts exist among the members of our profession? they who, as regarded by the laity, are specially trained to observe, because of intimate contact among their fellow men. In the words of Sir Dyce Duckworth ("On the Medical Injunction of Stimulants in Disease and Health." *British Medical Journal*, Nov. 10th, 1886,) "there is no middle course to pursue; either stimulants are all right if they be rightly used, or they are all wrong if they be used at all, or in any degree."

No routine practice can be made in the use of alcohol in disease, nor are we to understand such was intended; the true clinical acumen is called for in determining the necessity for its use, as for any other drug.

The injudicious or indiscriminate use of alcohol is certainly to be opposed; but to condemn it entirely because of its abuse, seems to me an argument that would condemn for the same reason most, if not all, of our reliable and more or less so-called dangerous remedies.

We are told by some that the practice of alcoholic stimulation frequently aggravates trains of morbid symptoms, establishing new centres of irritation, and thus proving somewhat disastrous in its effects upon disease; also leading to the undermining of the morals of the patient. Such, no doubt, is the fact where stimulants are administered indiscriminately, either in excessive doses or unnecessarily; when properly and judiciously administered, however, its action is highly favorable, increasing the power of resistance to disease.

The remarkable statement has but lately been made that "the range of the use of alcohol in disease is very restricted, and its use in disease with the object of keeping

Digitized by Google

up the strength of the patient is certainly unscientific; it is but adding another complication to the disease already existing, and frequently antidotes other remedies."

Let us see if there is any proof for the above statement, by asking, what is the property of alcohol in relation to animal heat, muscular power and nervous energy; is it utilized in the animal economy?

There are certain points of interest that bear directly upon its administration in disease, in that the true therapeutics of its employment has reference to its tolerance and the limitations as to quantity; the individual case and stage of the disease, age, sex, etc. Most of us are familiar, no doubt, with the fact that, in the febrile state, the relations between health and disease, as regards the quantity of alcohol taken, are certainly changed, in that, in disease, a much larger quantity can be administered without producing any morbid phenomena. The fact is evident, and there is little need, therefore, to state, that because of the moral and physical evils attending the abuse of alcohol, especially as a beverage, prejudice against its use in disease has naturally been excited.

In the employment of remedial measures, we should look beyond the immediate results of treatment, and have due regard to the remote effects of certain remedies, as not infrequently we are brought face to face with the fact of the injurious remote effects of measures which are immediately beneficial; frequently, therefore, in our anxiety to keep up the strength of the patient, we allow ourselves to forget certain precautions necessary in our treatment. To defend the administration of alcohol, especially in acute febrile disease, requires some conception of its action upon the human organism. Regarded as a stimulant, when given in moderate doses, its immediate action is upon the sympathetic, combined with a sedative influence upon the vaso-inhibitory nerves, thus increasing the frequency and force of the heart action, and causing dilatation of the capillaries, diffusing a temporary warmth throughout the body, at the same time increasing the supply of blood to the nerve centres.

The existence of high temperature includes active destructive metamorphosis, with excessive oxidation of tissue, constituting, generally, one of the most dangerous symptoms of disease. The researches of Anstie (*Practitioner*, Lond., 1874, xiii); Binz. (*Practitioner*, Lond., 1869, iii); Ringer (*Proc. Roy. Med. and Chir. Soc.*, Lond., 1866, v) Riegel (*Deut. Archiv f. Klin. Med.*, Leipzig, 1874, xii), and others, prove that alcohol, in moderate doses, causes a small, and in larger doses, a considerable reduction of temperature, the diminution in temperature being proportionate to the amount of alcohol ingested; this reduction, however, is but temporary, lasting, in moderate doses, but a few hours; the quick, tense pulse almost invariably becomes slower, and at the same time stronger.

On therapeutical grounds, the large proportion of hydrogen contained in alcohol, when oxidized in the body, would seem to augment the temperature; practically, however, as proved by Binz (the influence of alcohol on the temperature of the body, *Practitioner*, Lond., 1869, iii), "the fact has been overlooked, that the influence which checks other oxidation processes is more powerful than the effect of its own oxidation, accounting, therefore, for the reduction of temperature in the administration of alcohol; alcohol is, therefore, apyretic in its action, as also paratriptic, in protecting the tissues from oxidation by its own powers of oxidation." Another influence which alcohol possesses is its sedative and hypnotic effect, due to its action upon nerve tissue, calming the restlessness and erratic movements of an excited brain; in other words, lessening excitement and calming delirium; this can be brought about more quickly by moderate doses of alcohol, repeated more or less often, than by any other means; the parched and dry tongue and skin also become moist.

From these practical observations, therefore, not easily controverted, is shown the fact that alcohol, when introduced into the system, creates and evolves heat, due to

Digitized by Google

its rapid decomposition; in other words, alcohol feeds organic combustion, thus creating and evolving heat.

Too free alcoholic stimulation leads to interference with endosmosis and exosmosis in the lung capillaries, and thus with its free oxidation in the system.

In acute febrile and inflammatory disease the organs essential to the digestion and absorption of food are to a great extent affected; under the circumstances, the matter of a supporting treatment is to be strongly considered, and it is here that alcohol plays an important part, having a special and favorable influence as well upon digestion, in that it decidedly increases both the acidity and flow of the gastric juice; if given in such amount as not to be fully utilized and disposed of, its action is that of a veritable poison, in that the reverse of its favorable action is the case.

Does alcohol undergo combustion when introduced into the system, and thus act as a food, aside from its stimulant action? In other words, can alcohol be considered a food as well as a medicine, in relation to disease, or is it eliminated in greater part unchanged, by the various emunctories? The experiments of Lallemand, Duroy and Perrin (on the rôle of alcohol and anæsthetics in the organism, Paris, 1860), as also those of Richardson (*Medical Times and Gazette*, London, 1869, ii), showed that when taken into the system, alcohol is freely eliminated as alcohol, apparently unchanged, and could, therefore, in no sense be termed a food; these experiments were not conclusive however, in that actually they showed but a part of the alcohol was excreted and eliminated, the quantities administered at a time being excessive, oxidation of which was impossible within the time limited.

The researches of Liebig and Baudot (*Union Médicale*, Paris, 1863, xx, 1864, xxi), and later on those of Anstie (*Practitioner*, London, 1874, xiii); Duprè (*Practitioner*, London, 1872, viii); Brunton (*Practitioner*, London, 1876, xvi); Thudichum, H. C. Wood and other physiologists, however, tend to prove satisfactorily that, when administered in moderate quantities, and at the same time judiciously, it is practically wholly consumed in the system, and not eliminated as alcohol, showing, therefore, its actual value as a food. In the excellent paper by Duprè, on the "Elimination of Alcohol," is shown the fact, that there exists normally in the human urine a substance which, by distilling the urine with an acid, gives the reaction ordinarily employed for the detection of small quantities of alcohol; this has, no doubt, as stated by Duprè, led some experimenters to the belief that the elimination of alcohol continued much longer than in reality it did.

In the words of Brunton (Physiological Action of Alcohol, *Practitioner*, London, 1876, xvi), "We may conclude that, in moderate doses, alcohol undergoes combustion in the body, supplying energy, yielding warmth and tending to sustain life in the same way that sugar would do, and is therefore to be reckoned as a food, greatly increasing its utility in disease."

The apparent unphysiological and hurried acceptance of the conclusions of Lallemand, Duroy and Perrin, as regards alcohol not being a food, seems to pervade still, after many years, the minds of many medical men; furthermore, if alcohol is to be reckoned as a "nerve stimulant," incapable of adding substantial aid and force to the body, how are we to explain its happy action in acute febrile disease! Its action as a "nerve stimulant" is, as stated by Anstie, "merely to encourage its rise in states of chronic feebleness, marked by outward signs which are the reverse of those of the febrile state, and more especially for conditions of nervous exhaustion complicated with mental depression."

In acute febrile disease, alcohol undergoes combustion itself, instead of the tissues; in other words, though not adding to the growth of any tissue, it certainly protects it against oxidation by its own oxidation, thus retarding tissue change incident to the vital processes. This combustion and apparent entire consumption of alcohol in the

Digitized by Google

system is at the expense of the oxygen to be applied for the natural heating of the body, being, as stated, a process of oxidation, this chemical change taking place only in proportion to the amount of oxygen existing in the blood; whereas, in the experiments of Lallemand, the amount of alcohol ingested being excessive, or the percentage of oxygen in the blood lessened, a more or less greater proportion of the alcohol may be recovered by the excreta.

In numerous instances of disease, where the danger of failing heart power is to be averted for a few days or even longer, or where the patient either refuses or rejects any form of nutriment, the administration of alcohol will be found of great service, tiding the patient over the worst stages, being therefore the agent by which he is kept alive and recovery possible. We are all aware that in acute febrile disturbances the increase of temperature produces not only a lack of desire for ordinary food, but more or less arrest of the muscular acts necessary to the forwarding of the solution of the blood; alcohol here seems to fulfill the wants of the system in this respect; the quantity of alcohol appropriate, therefore, will depend almost entirely upon the nature of the disease and the strength of the patient to resist it, having special reference to the rapidity of oxidation of the tissues of the body; with greatly increased temperature and rapid oxidation of the tissues, its employment is especially beneficial, acting both as a medicine and food, the latter especially, because it replaces the tissues of the body, in that it affords material for the oxidation process to feed upon. It may, perhaps, be stated, in a concise way, that the meagreness of its nutritious properties is often made up in usefulness by the rapidity of its assimilation; also, that it requires the least expenditure of digestive and assimilative force, at the same time being an element of heat and force to the system. Finally, in the words of Dujardin-Beaumetz (On the Beverages in Common Use, *Therap. Gaz.*, Nov., 1886), "alcohol is a food, a waste-restraining food, which, instead of energizing the combustions, slows them, by robbing the hæmoglobin of the blood-corpuscles of a certain quantity of oxygen; the character of alcohol as an aliment, therefore, may not improperly be included, in a therapeutical sense."

Caution is certainly required in its employment, especially when employed for a prolonged time. Many of us have been made aware, however, of the fact that apparently unlimited quantities of alcohol may be given in some forms and stages of disease, which otherwise appear hopeless, without any untoward effects; in such cases, the carrying of it to intoxication is what is required. The physician should have due regard for, or be guided greatly by the feelings or desires of his patient, if any sense of discomfort is expressed from its previous administration; in fact, when a peculiar idiosyncrasy to its effects exists. He should not fail to ascertain for himself that the dosage required during the illness of the patient is relinquished when the occasion for its employment has ceased; also, that the amount administered in the acute stages of disease is gradually lessened toward convalescence; a failure to perform this duty not infrequently leads to a craving for alcoholic stimulants.

The question of alcohol being generated in the animal organism through the digestive processes, especially in the combustion of the carbohydrates, is one still undecided and open to further research; the probability of such, however, is highly suggestive. Not infrequently the unfortunate impulse to alcoholic excesses is due to its indiscriminate and careless prescribing in diseases "nervous" in character, especially in women. This is a melancholy fact, and to expose them to such temptation is certainly to be decried, when we are familiar with the fact that their power of mental resistance, under the circumstances, is generally weak. Cases are met with, however, not infrequently, and so constituted that they are utterly, and thus unfortunately, unable to bear any mental strain whatever, succumbing almost immediately to its effects. Relief is certainly afforded by having recourse to temporary alcoholic stimulation.

In reference to the assertion that the employment of alcoholic stimulants in acute

Digitized by Google

febrile disease is apt to lead to subsequent excesses; on the contrary, a more or less rapid intolerance is observable very early in its administration, due to the quantity indicated and ingested in the short space of time. The reverse, however, is evidently the case in chronic and nervous diseases.

In the application of alcoholic stimulants, to enumerate the various diseases in which its administration is deemed advisable, would consume too much time. I shall content myself by referring to the main indications and morbid conditions presenting themselves. To choose the proper moment for their employment is essential; we are not to administer them too early in the disease, where no special indication presents itself, especially so in severe general inflammations, where a dilatation of the arteries and obstruction of the capillaries exists. First of all, we are to take into consideration the fact of the resistance of the patient to the disease, whatsoever the latter may be. It is argued by some that their administration is to be deferred until the appearance of adynamia, but it may be stated, as a general rule, we are not to wait until signs of danger arise, but rather to anticipate them, resorting to stimulants with little hesitation when necessary. In acute febrile disease we are to take into consideration, aside from the fact of warding off threatened asthenia, the intensity of the pyrexia, the cerebral symptoms and state of the digestive and assimilative processes. The existence of pyrexia in itself, unaccompanied by any other signs, does not call for its employment. Once, however, the indications arise and are recognized, measures should be immediately taken to counteract or avert any threatening danger. Under whatever circumstances deemed necessary, its administration should never be left to the hands of the patient. Let me state here, also, that in the insufficient and thus imperfect administration of alcoholic stimulants in acute febrile disease, especially when accompanied by symptoms of adynamia, we permit the tendency to danger to increase. This would be obviated by a more strict attention to the indications presented for their employment in the course of the disease. Again, not infrequently, the injudicious and excessive administration of alcohol, especially in the form of brandy or whisky, undiluted, leads to a gastritis, more or less complicating the disease. Let the fact that alcohol requires no digestion be kept in view. It is rapidly diffused throughout the system. This diffusibility being more rapid when given undiluted. The indications for administering it in an undiluted form are, however, few.

The stronger alcoholics when diluted from five to ten times their bulk with water, in febrile disease, act both as diuretics and diaphoretics, in that the activity of the kidneys and sudorific glands are markedly increased. As the febrile condition advances the quantity of alcohol is to be proportionately increased, the reverse being the case upon the gradual decrease of the pyrexia and toward convalescence; when, however, with the decrease in the pyrexia, asthenia supervenes more or less rapidly, as in the latter stages of typhoid fever, pneumonia and diphtheria, stimulants are to be administered freely and often, without reserve; the appearance of adynamia, in these cases especially, frequently precludes efforts at nourishing the patient, through a decided lessening or failure in the power of the gastric juice to form peptones; to support the failing powers, therefore, our main dependence must be in the employment of stimulants.

In persons ill with acute disease and previously anæmic and reduced in strength, the necessity for alcoholic stimulants is more urgent than in those previously healthy and plethoric. In phthisis and other wasting diseases, the indications for alcoholic stimulants are undoubted; aside from its property of retarding waste of tissue, I believe its happy action is due partly to its solvent action on fats, in rendering them more easy of digestion and assimilation.

In sudden collapse or great prostration from recent hemorrhage or shock due to severe injuries, surgical operations, or from whatever cause, with feeble or labored heart action and preponderance of blood in the venous system, it is especially indicated, and

Digitized by Google

should be administered in the form of brandy or whisky, undiluted, its analeptic action being desired.

In septic disease, where the powers of life are actually oppressed, as also in puerperal sepsis, the resisting powers of the organism combined with a sedative and hypnotic action, are best maintained by the administration of alcohol in large doses, frequently repeated; in puerperal sepsis, tepid baths should also be given in this connection, as but recently recommended by Runge, of Dorpat.

In the grave forms of dysentery, with accompanying adynamia, we have an immediate indication for resorting to its use. In diphtheria, with its tendency to rapid heart failure, little danger exists in what may seem free stimulation; it is indeed surprising to what extent stimulation by alcohol may be carried in severe cases, without producing any ill effects or even signs of intoxication; it is especially in these cases marked by toxæmia, that, aside from its stimulating property, stamps alcohol as a food of great value. In this connection, quite recently, the question has been propounded as to whether the sudden death after diphtheria may not be due to thrombosis or embolism of the pulmonary artery, through the power of the alcohol to coagulate the albumen of the blood, induced by free stimulation? The question is, no doubt, an appropriate one, but I have failed to find any record of such, and in my own mind doubt very much its occurrence, in so far as induced by alcohol, as numerous cases have been reported of sudden death from diphtheria where no alcohol was administered in the course of the disease.

In the later periods of life, the necessity for stimulants is always greater, and in whatever febrile disease, where the patient is addicted to the free use of alcohol, the latter should never be withheld.

In nervous diseases great care and discrimination should be exercised, there being but little indication for the application of alcoholic stimulants. Where there exists an aversion to liquors, the dry or what are termed stronger wines, because of their richness in alcohol, are also indicated in febrile disease and states of prostration.

In chronic diseases with more or less anorexia, the giving of the red or light wines, because of their tonic character, will be found beneficial in increasing the appetite and aiding the digestion of food; again, in individuals possessing but feeble powers of resistance, especially in the aged, the administration of lighter or even the stronger wines, in moderate doses, will prove especially beneficial.

In this connection, I would refer to the effervescent or sparkling wines, especially champagne; containing but a small percentage of alcohol, and because of the presence of the carbonic acid, it is especially serviceable, not only as a quick stimulant and restorative in cases of exhaustion, but in allaying gastric irritability and obstinate vomiting, it being retained where other measures of treatment are rejected and prove futile; in numerous instances I have found it the best and only diffusible stimulant to be employed, its rapidity of action and exhilarating power being, in all probability, due to a vinous ether diffused by the liberated carbonic acid.

In all cases in which it is necessary to support life by nutritious enemata, the addition of alcoholic stimulants will be found of service, as also in accelerating their absorption. Its use, hypodermically, in sudden collapse or shock after surgical operations, is so well known that I refrain from any further mention in this regard. In this connection, it would be well to state, that in affections of the bladder and urinary organs, especially the various forms of Bright's disease; in the various hepatic affections and in cases of gout and rheumatism uncomplicated by severe endo- or pericarditis, alcohol is contraindicated. Finally, that the adulteration of liquors and wines has a distinct bearing upon a discussion of the therapeutics of alcohol seems to me evident, inasmuch as in its employment we apply the various liquors and wines, greatly because of their bouquet and flavor, in place of alcohol in its pure state; in desiring its true physio-

Digitized by Google

logical effects, therefore, we are to be careful always to ascertain and select a pure article.

A *résumé* of the preceding may be tersely given as follows: (1) In alcohol, we have one of the most powerful and substantial agents in the treatment of disease, combining, aside from stimulant, anti-thermic, paratriptic, alimentary and tonic properties. (2) The mechanism of its favorable action upon the animal organism and its utility in disease, more especially acute febrile, may be explained in several ways: (*a*) its dynamic action, particularly in greatly lowered arterial tension, in increasing and sustaining the vital powers, through its action upon the vascular system and nerve centres; (*b*) its antithermic or apyretic property, in causing a reduction of temperature apparently proportionate to the amount of alcohol ingested; (*c*) its alimentary and paratriptic property, due to its own combustion and oxidation, thus retarding disintegration and oxidation of the tissues. (3) No routine practice can be made in the use of alcohol in disease; each case should be treated upon its merits and the indications presented; because of the advantages accruing in most, its unqualified recommendation in all disease is not to be thought of; in this way, it should be prescribed, similar to any other drug or therapeutical measure.

(4) Finally, measures adopted with the view of modifying or protecting any changes that take place in the tissues in disease, are certainly legitimate and rational in procedure.

The following paper was read by title :—

THE PROTEIDS OF THE SEEDS OF ABRUS PRECATORIUS (JEQUIRITY) AND A SHORT ACCOUNT OF THEIR PHYSIOLOGICAL ACTION.

LES PROTÉIDES DES GRAINES DE LA LIANE À RÉGLISSE (ABRE) ET UN BREF RAPPORT DE LEUR ACTION PHYSIOLOGIQUE.

DIE PROTEÏN-STOFFE DES ABRUS PRECATORIUS-(JEQUIRITY)-SAMENS, NEBST KURZEM BERICHT ÜBER IHRE PHYSIOLOGISCHE WIRKUNG.

BY SIDNEY MARTIN M. D., B. SC. (LOND.),

In a communication made to the Royal Society* this year I showed that the proteids existing in jequirity seeds were similar in chemical properties to those occurring in other seeds, and consisted of a globulin and an albumose.

The globulin and albumose are best extracted by treating the ground seeds (which have been freed from the red cuticle) with an excess of 15 per cent. sodium chloride, filtering, and saturating the filtrate with neutral ammonium sulphate, which causes a precipitation of both the proteids. Collecting this precipitate, mixing it with distilled water and dialysing it for several days, throws down the globulin in great part, leaving the remainder of the globulin and the whole of the albumose in solution. The globulin was collected in a filter and washed with distilled water for two days, until the washings gave no evidence of a soluble proteid; the proteid in the filter was then only globulin. It was dried over sulphuric acid. The albumose was purified by filtering the dialysed liquid into rectified spirit and allowing the precipitate formed in the liquid to stand for three to six months, when the whole of the globulin was coagulated and the albu-

* *Proc. Royal Soc.*, Vol. XLII (1887.)

Digitized by Google

min could be obtained in solution by treating the dried precipitate with distilled water. The solution was re-precipitated by filtering into absolute alcohol; filtered again and dried over sulphuric acid.

Clinical properties of the proteids of Abrus seeds.

Globulin.—The globulin belongs to what I have elsewhere called the class of vegetable paraglobulins,* from their resemblance to animal paraglobulin. 1. Abrus-paraglobulin is readily soluble in 10 to 15 per cent. sodium chloride or magnesium sulphate solution; to a less extent soluble in a five per cent. NaCl solution, and scarcely at all in 0.75 per cent. 2. It is completely precipitated from solution by saturation with sodium chloride after acidifying, and with neutral ammonium sulphate. 3. It is coagulated by heat in ten per cent. magnesium sulphate solution between 75° and 80° C. 4. It is precipitated from solution by dialysis, but the precipitate is still soluble in saline solutions; it is not precipitated by exposure to a temperature of 35° to 40° C. for even 48 hours. In these two facts it differs markedly from the other class of vegetable globulins I have differentiated;† viz., the myosins existing in cereals, which are not only precipitated by dialysis, but are changed into an albuminate; *i. e.*, a body insoluble in saline solutions but soluble in dilute acids and alkalies; and which are precipitated or changed into an albuminate by exposure to a temperature of 35° to 40° C. for a few hours.

Abrus-paraglobulin is precisely similar in chemical properties to the globulin occurring in the fruit-juice of Carica Papaya, from which the ferment papain is extracted.‡

Albumose.—Abrus-albumose belongs to the large class of what I have called *phytalbumoses*, vegetable proteids which may be considered intermediate in properties between globulins and peptones. Though resembling the animal albumoses described by Kühne and Chittenden, they differ in so many particulars as to deserve a separate name. Abrus-phytalbumose has the following chemical properties: 1. It is soluble in cold or boiling distilled water, its chemical and physical properties not being apparently altered by boiling its solution. 2. It is not precipitated by saturation with sodium chloride, unless a large excess of acetic acid be added. It is readily thrown down by saturation with neutral ammonium sulphate. 3. Nitric acid does not precipitate it unless solid sodium chloride be added nearly to saturation. 4. Its solution gives a pink color with sulphate of copper and excess of potash ("Biuret" or peptone reaction).

Abrus-phytalbumose is identical in chemical properties with the phytalbumose occurring in the fruit juice of carica papaya, which is the proteid I have shown to be *closely associated with the ferment "papain."*

I have dealt thus fully with the preparations and the chemical properties of the abrus proteids because of the important bearing on their physiological action. Warden and Waddell have shown ‖ that the toxic principle of abrus seed is a proteid closely allied to Liebig's "plant albumin." They obtained it from a watery extract of the seeds. They did not further investigate the chemical properties present.

In testing the physiological toxic action of a proteid, three points are of prime importance:—

1. We must be sure that there is only *one* proteid present in the solution used.
2. The presence of an *alkaloid* must be eliminated.
3. The contamination or the presence of bacteria (schizomycetes) or their spores must be eliminated.

As is well known, the action of jequirity was at first thought to be due to a bacillus, the so-called "jequirity bacillus," but this has been shown to be incorrect by the

---

* *Journ. of Physiology*, Vol. VI, page 353. *Physiolog. Soc. Proc.*, Feb. 12, 1887.

† Op. cit., *Proc. Phys. Society.* ‡ Op. cit., *Jour Phys.*

‖ "The Non-bacillar Nature of Abrus Poison." Calcutta, 1884.

Digitized by Google

experiments of Klein and of Warden and Waddell. Klein, from his short investigation, came to the conclusion that the poison was of a "ferment" nature;* Warden and Waddell also came to this conclusion, with the addendum, however, that the "ferment" was closely associated with a plant albumin, which they called *abrin*. The chief reason for considering the toxic principle a "ferment," was that its activity was destroyed by boiling.

PHYSIOLOGICAL PROPERTIES OF THE ABRUS PROTEIDS.

This part of the investigation was done with Dr. Norris Wolfenden, who shares an equal responsibility with me; and the remarks now made must be considered only as a preliminary communication.

In our experiments, we have made use of a solution of pure abrus-paraglobulin in sterilized fifteen per cent. sodium chloride solution, and of a solution of abrus-phytalbumose in sterilized distilled water. The syringe used in inoculating was also carefully sterilized.

We have tested the physiological action in two ways: (1) by instilling the liquid into the eye; (2) by hypodermic injection. The chief points in the results we have obtained may be summarized as follows:—

1. *Abrus-albumose* is much less active than the abrus-globulin. In the eye, it does not always produce conjunctivitis; subcutaneously, it does not always produce death.

2. *Abrus-globulin* is very toxic. Instilled into the eye it produces "abrus-ophthalmia," which, in most cases, ends in recovery, though in some cases, the animal (rabbit) dies in a few days (a fact previously noted by Klein).†

If abrus globulin be placed in a solid form under the eyelid, it produces intense conjunctivitis, with *local ecchymoses*.

Hypodermically, it always kills the animal, even when given in small dose; and its toxic power is proportional to the dose. It produces great local congestion or œdema, with, in the majority of cases, ecchymoses, sometimes punctiform, sometimes diffuse. The post-mortem signs agree with those described by Warden and Waddell.

3. The activity of abrus-globulin is at once and permanently destroyed by heating its solution in saline up to 75° C. (The heating was performed gradually; the test tube containing the solution of globulin being placed in a flask full of cold water, which was heated; directly the temperature rose to 75° C., or any other temperature that was desired, the test tube was removed and rapidly cooled under the tap). Heating, in the manner described, up to 70° C. did not destroy the activity of the globulin, nor was this affected by any lower temperature. In ten per cent. sodium chloride solution, the globulin is coagulated between 66° and 73° C.‡ We find that the toxic power of the globulin is destroyed at this temperature, and we cannot but point out that the activity of the poison seems to be intimately connected with the uncoagulated condition of the proteid. Full details of the experiments, with further remarks on the albumose and the action of these proteids on protoplasm, will shortly be published by Dr. Norris Wolfenden and myself.

---

* "Microörganisms and Disease." 1884, p. 165.

† *Op. cit.*

‡ *Proc. Roy. Soc.*, Vol. XLII, p. 333.

Digitized by Google

Dr. JUSTUS ANDEER, of Munich, read a paper—

DAS RESORCIN UND SEINE PRÄPARATE.

RESORCIN AND ITS PREPARATIONS.

LA RÉSORCINE ET SES PRÉPARATIONS.

VON DR. JUSTUS ANDEER,
München.

Die vorliegende Mittheilung ist die Frucht meiner Beobachtungen während eines Zeitraumes von zehn Jahren. Ungeachtet der vielen Angriffe, welche dieses Mittel von Seiten oberflächlicher Controlforscher zu erfahren hatte, bricht sich dasselbe immermehr Bahn in der medicinischen Welt auf Grund gediegener Erfahrungen ernster Forscher, wie Bouchut, Bongouche, Callios, Cattani, Ihle, Moncorvo, Unna, und so vieler Anderer. Ein Hinderniss für die Einführung des Resorcins in die praktische Medicin, die absolute Reinheit des Mittels, ist nunmehr gehoben, indem wir die Firmen von Meister, Lucius in Höchst, und von Mormet & Co. à la Plaine bei Genf, tadellose Präparate herstellen sehen. Besonders die Résorcine médicinale crystallisée des Letzteren genügt allen bislang gemachten Anforderungen, sei es in Bezug auf Güte wie Billigkeit des Mittels. Neben der chemischen Vervollkommnung, ist auch die pharmaceutische Herstellungsweise des neuen Mittels erstrebt worden, so dass man dasselbe in passender Form äusserlich und innerlich anwenden kann. Für die äussere Anwendungsweise habe ich dadurch schöne Resorcinpräparate zu erhalten gesucht, dass ich aus Mischungen von Resorcin mit Cacaobutter Stangen und feine Platten herstellen liess, um selbe auf die kranke Haut oder auf Wunden zu legen. Als "herrliches Antisepticum", wie Nussbaum das Mittel nennt, macht es Haut und Wunde schnell rein und führt dadurch eine schnelle Heilung herbei. Ganz brillant ist die Wirkung dieser Resorcinpräparate in Verbindung mit den Resorcinseifen bei Erkrankungen durch Parasiten: Sycosis, Lichen, Alopecia, Eczemen, Condylomen. Herpetische Prozesse, lupusähnliche Geschwüre, die allem Anderen trotzten, heilten mit diesen Präparaten, und nicht minder wurden hartnäckige Eczeme und torpide Geschwüre vom Resorcin ganz besiegt. Einzig in seiner Art ist die Wirkung des Resorcins auf Keloïde, die bekanntlich bisher nahezu unheilbar genannt werden mussten. Die Resorcinpräparate, welche zu dermathologischen Zwecken verwendet werden sollen, brennen nicht, wenn sie nicht höhere Concentrationen erreichen, sie erodiren die Haut nicht wie Pyrogallol und reizen die Augen nicht wie Chrysarobin. Am besten wird Resorcin als Salbenmull gebraucht und mit Collodium bestrichen. Will man für den Nothverband in Ermangelung der technischen Präparate eine Resorcinsalbe ad hoc sich verschaffen, so passt die Butter, welche überall mit grösster Leichtigkeit zu haben ist, am vorzüglichsten und in anfänglicher Concentration von 1% Resorcin.

Ich erachte die Einführung der obengenannten Resorcinseifen in die praktische Medicin für einen grossen Fortschritt aus mehrfachen Gründen. Während beispielsweise die Carbolseife binnen weniger Tage einen grossen Theil ihres Phenols durch Verflüchtigung verliert und desinfectorisch unwirksam wird, ist dies bei den Resorcinseifen nie der Fall, selbst nach jahrelangem Aufbewahren nicht; ihre medicamentöse Kraft bleibt unverändert. Sie ist ferner geruchlos und reizt weder die gesunde noch die kranke Haut oder Schleimhaut und verleiht beiden einen weichen, zarten, elastischen Zustand im Gegensatz zur Carbolseife, welche die genannten Gebilde hart, unempfindlich, unelastisch macht und im Resorptionsfalle giftig wirkt, welche gefährliche Eigenschaft den Resorcinseifen völlig abgeht. Die Resorcinseifen empfehlen sich daher besonders für den Spitalgebrauch, für den Reise- und Kurgebrauch, wie nicht minder

Digitized by Google

als Ersatz für die gewöhnlichen Seifen in Haushaltungen, die auf Reinlichkeit und moderne Hygieine halten. Auch ist ihr Preis nicht höher wie der gewöhnlicher Seifen, die mit 50 cts. bis zu 1 frs. per Stück bezahlt werden.

Seit Einführung der keratinirten Pillen durch meinen Freund Unna in Hamburg ist die innerliche Localtherapie durch Resorcin ebenfalls bedeutend vervollkommnet worden. Diese Pillen, welche bekanntlich erst im Dünndarm gelöst werden, ohne vom Magensaft vorher angegriffen und verändert worden zu sein, liess ich mit Resorcin und Ricinusöl füllen (Rec. Resorcin puriss 1.0 solve in Ol. Ric. feroid 50.-100.0), und verabreichte ich sie dann mit bestem Erfolge bei verschiedenartigen Diarrhöen, Dysenterien, Cholera infantum, Typhus, kurz bei allen oberflächlichen und tief excorirenden Prozessen des Darmes.

Für den Genitalapparat und Tractus sind die Resorcin-Cocaostäbchen und Kapseln, wie sie Apotheker Sauter in Genf auch für andere medicamentöse Zwecke herstellt, sehr schön. In praktischer Beziehung möchte ich, nach meinen bisherigen Erfahrungen wenigstens, besonders für eine vollständige Reinigung der Vagina, mehr den wässerigen Einspritzungen wie dem compakten Präparat des Resorcins den Vorzug geben, weil die Flüssigkeit besser in die Falten und Krypten der Scheidenschleimhaut eindringt, um selbe wirksam zu ätzen und zu desinficiren. Wie das Resorcin, wie kein anderes Mittel der Pharmakopöe, in jeder Flüssigkeit leicht löslich sich erweist, ebenso leicht ist es mit jedem Fette zu verbinden, um dasselbe an schwer zugänglichen Theilen des Organismus, wie Speiseröhre, Kehlkopf, Uterus, Mastdarm, anwenden zu können. Wie und bei welchen Hautaffektionen das Resorcin richtig anzuwenden ist, habe ich weitläufig in *Unna's Monatsheften für Dermatologie*, Jahrgang 1883–87, angegeben, über seine innerliche Anwendungsweise behalte ich mir baldige Mittheilungen vor. Bei D., wo ich immer glänzende Heilergebnisse hatte, haben viele Aerzte das Mittel zu oft oder zu schwach angewendet und dadurch nicht bessere Kuren als mit anderen Mitteln erzielt. Hier wie überall liegt das Richtige in der Mitte, wie?

DISCUSSION.

Dr. L. Lewin.—Mit ausserordentlicher Ausdauer hat der Herr Vortragende seit Jahren das Resorcin in seinen mannigfaltigen Wirkungen erforscht. Die Ausdehnung, welche in neuerer Zeit Herr Unna dieser Substanz als reducirendem Stoffe gegeben hat, habe ich in der *Berliner Klinischen Wochenschrift* vor einiger Zeit zu bekämpfen versucht.

Ich glaube, wir müssen solche Fragen von einem etwas allgemeinen Gesichtspunkte aus betrachten und uns vor allen Dingen hüten, eine Substanz wie das Resorcin auf Kosten aller anderen zu erheben. Hat das Resorcin wirklich fünfzig überlegene Wirkungen über andere Stoffe? So flüchtig wie der Herr Vortragende meint, ist die Carbolsäure in Carbolseifen nicht, dass die Wirkung dadurch beeinträchtigt werden könnte. Aber auch andere Stoffe, wie das isomere Hydrochinon, wirken ganz analog antiseptisch, so dass das Resorcin hierin keine so ausserordentliche Bevorzugung verdient.

Der Herr Vortragende meint, dass eine innere Antiseptik damit erzeugt werden konnte, wenn es in keratinirten Pillen oder mit Ricinöl gegeben wurde. Aber ob nun auch die keratinirten Pillen im Magen unangegriffen bleiben oder nicht — ich habe sie bei Thieren im Magen zerfallen sehen — so kann man sicherlich mit anderen Mitteln in solchen Formen ähnliche Wirkungen herbeiführen.

Also auch in dieser Beziehung sehe ich keine überlegenen Eigenschaften des Resorcins. Zuletzt muss ich die Giftigkeit des Resorcins hervorheben, die derjenigen anderer Stoffe der Benzolreihe nicht nachsteht. Ich schliesse, indem ich nochmals

Digitized by Google

die Wichtigkeit solcher speciellen Untersuchungen betone, aber doch davor warnen möchte, eines auf Kosten des anderen zu erheben.

Dr. H. G. Beyer, u. s. n.—So far as concerns the assertion of Dr. Unna, of Hamburg, that resorcin is a reducing agent, I am inclined to agree with the last speaker, that it may also at times, and under certain conditions, act as an oxidizing agent. If resorcin be a reducing agent at all it can only be called so in its relations to certain definite chemical compounds. So far as its action on living mammalian blood is concerned, it would seem to be an oxidizing rather than a reducing agent; for blood, when mixed with resorcin in certain proportions, may be kept without undergoing decomposition and remain of a bright arterial hue for many days, thus showing that it has antiseptic powers, and that it does clearly not have the power of reducing hæmoglobin.

Dr. Andeer.—Resorcin wurde von mir nie als Reduktionsmittel, wie der Herr Opponent es meint, sondern ist zuerst von Unna gebraucht worden. In allen meinen Arbeiten habe ich immer nur vom Resorcin als Hydratationsmittel gesprochen. In gleicher Weise kann ich behaupten, dass ich ebenfalls nie vom Resorcin als einem Fiebermittel gesprochen, sondern nur von einem antiseptischen, kaustischen und hämostatischen.

Die Vorzüge des Resorcins gegen andere Mittel sind zu evident, als dass man hier opponiren sollte. Von der Haut aus resorbirt, tritt nie Vergiftung ein, wie nach Phenolgebrauch; es reizt und brennt nicht die Haut, wie Phenol, Thymol und andere; es macht nicht die Haut ungeschmeidig und hart wie die anderen, und desinficirt zugleich in 1 % Verbindung mit Vehikeln irgend welcher Art ebenso stark.

Die vorgeworfene Giftigkeit des Resorcins ist bei richtiger Anwendung desselben völlig unzutreffend. Die besten Resorcinforscher, welche meine Angaben controllirt haben, stimmen darin überein, dass man $5_0 - 6_0$ pro die ohne irgend welche Vergiftungssymptome verabreichen kann. Die von Rossbach, Brieger und ihren Anhängern vorgeworfene Giftigkeit ist demnach unrichtig oder richtig nur bei tadelhaften, resp. unreinen Resorcinpräparaten.

In allen meinen Arbeiten und mündlichen Mittheilungen habe ich nie ermangelt, darauf hinzuweisen, dass das Resorcin keine Panacee sei, sowohl bei äusseren, wie bei inneren Krankheiten. Bei Affectionen von saurer Diathese beispielsweise ist es nicht blos indifferent, sondern sogar schädlich, mithin keine Panacee. Ein direct specifisch antifebriles Mittel ist es ebenfalls nicht, mithin keine Panacee. Und so könnte ich noch durch viele Argumente beweisen, dass der Vorwurf, ich wolle das Resorcin als Panacee anempfehlen, wirklich allen Grundes entbehrt.

Digitized by Google

ÜBER DIE NOTHWENDIGKEIT EINER INTERNATIONALEN PHARMAKOPÖE.

ON THE NECESSITY FOR AN INTERNATIONAL PHARMACOPŒIA.

SUR LA NÉCESSITÉ D'UNE PHARMACOPÉE INTERNATIONALE.

VON DR. F. DRONKE,

Berlin.

Eine vergleichende Zusammenstellung über die Zubereitung der verschiedenen, in den Pharmakopöen der einzelnen Culturstaaten gebräuchlichen galenischen, namentlich der starkwirkenden Mittel, sowie der Stärke und des Wirkungsgrades der chemischen Präparate der Pharmakopöen zeigt meist eine nicht unbedeutende Verschiedenheit. Wenn nun auch selbstredend jedes einzelne Land innerhalb seiner Grenzen die Berechtigung zu eigenen Vorschriften hat und hiervon Gebrauch macht, so wirft sich in Folge des täglich sich steigernden internationalen Verkehres der gesammten Culturstaaten doch unwillkürlich die Frage auf,

(1) ob es nicht wünschenswerth, um nicht zu sagen nothwendig, erscheint, die Aufstellung einer Internationalen Pharmakopöe anzustreben,
(2) ob es möglich sein wird, dieses Ziel zu erreichen, und
(3) welche Wege eventuell zur Erreichung dieses Zieles eingeschlagen werden können.

Der erste Punkt, der Wunsch nach einer Internationalen Pharmakopöe, findet schon seit langen Jahren seinen beredten Ausdruck auf den verschiedenen in längeren oder kürzeren Zwischenräumen zusammentretenden Internationalen pharmaceutischen Congressen. Bereits auf dem ersten pharmaceutischen Congresse in Braunschweig, 1865, war die Gleichstellung aller starkwirkenden Arzneistoffe und Präparate in Bezug auf ihren Gehalt an wirksamen Bestandtheilen in allen Pharmakopöen angeregt und wurde dieser Punkt auch auf dem zweiten Internationalen pharmaceutischen Congresse, 1867, in Paris auf die Tagesordnung gesetzt. Der Vorschlag fand in der gesammten pharmaceutischen Welt den grössten Beifall und die lebhafteste Zustimmung und auch von maassgebenden medicinischen Kreisen wurde der Vorschlag als zeitgemäss und die Durchführung als nothwendig und wünschenswerth anerkannt; wie wir aber zu unserem Bedauern gestehen müssen, bislang leider ohne Erfolg.

Im Jahre 1869 legten zwar die Herren Robinet und Mialhe dem dritten Internationalen pharmaceutischen Congresse in Wien die Scizze zu einer derartigen, die Gleichstellung bezweckenden Arbeit vor; es wurden auch die genannten Herren ersucht, auf Grund der vorgelegten Scizze ein ausführliches Elaborat dem nächsten Congresse vorzulegen, aber das auf Grund dieses Ersuchens auf dem vierten Internationalen pharmaceutischen Congresse in Petersburg vorgelegte Elaborat entsprach nicht den gestellten Ansprüchen und musste trotz aller Anerkennung der ausgezeichneten Bearbeitung zurückgewiesen werden. Es wurden die Vertreter der St. Petersburger Pharmaceutichen Gesellschaft ersucht, die von Dr. Méhu, als Vertreter der Société de Pharmacie de Paris, angefertigte Arbeit in geeigneter Weise umzuarbeiten. Leider unterblieb die Arbeit und es wurde 1881 in London auf dem fünften Internationalen pharmaceutischen Congresse neuerdings beschlossen, die Gleichstellung aller in den Pharmakopöen der verschiedenen Länder enthaltenen, stark wirkenden Arzneistoffe und Präparate hinsichtlich ihres Gehaltes von stark wirksamen Stoffen in Angriff zu nehmen. Es wurde zu diesem Zwecke auf dem Londoner Congresse ein aus pharmaceutischen Vertretern aller am Congresse betheiligten Staaten bestehendes Comité gewählt mit dem Auftrage, sich durch Vertreter der am Congresse nicht betheiligten

Digitized by Google

Staaten zu verstärken und sodann baldmöglichst eine dem Beschlusse des Congresses entsprechende sogenannte "Internationale Pharmakopöe" auszuarbeiten. Das Comité bestand in Folge dieses Beschlusses aus 35 Delegirten, welche 18 Staaten angehörten, und trat sofort in Thätigkeit, so dass dem sechsten Internationalen pharmaceutischen Congresse in Brüssel, 1885, ein ausführliches Elaborat vorgelegt werden konnte. Der Wunsch nach einer Internationalen Pharmakopöe ist also gewiss schon oft genug ausgesprochen worden; die Nothwendigkeit tritt tagtäglich immer mehr hervor und mag durch einige Betrachtungen und einzelne Beispiele hier erläutert werden.

Ein deutscher Arzt liest in einem wissenschaftlichen Journale eine warme Empfehlung von Syrupus Ferri jodati gegen diese oder jene Krankheit in näher angegebener Dosis und nimmt in nächster Zeit Gelegenheit, Syrupus Ferri jodati in der angegebenen Dosis zu ordiniren; aber er ist nicht wenig unangenehm berührt von der Jod-Intoxication, welche nach der verordneten Dosis von Syrupus Ferri jodati eingetreten ist; er glaubt, dass irgend ein Missverständniss vorliege; doch dem ist nicht so; die Dosis im Journale stimmt, der Apotheker hat sich nicht verwogen, die Arznei ist richtig angefertigt, genau dem Patienten eingegeben, und doch Jodintoxication! Die Sache klärt sich einfach auf: der Autor im Journale hatte den Syrupus Ferri jodati des Codex medicamentarius Gallicus (Pharmacopée française) vor Augen, welcher in 100 Theilen 0.5 Theile Ferrum jodatum enthält, und der in Deutschland ordinirende Arzt erhielt den zehnmal stärkeren Syrupus Ferri jodati der Pharmakopöa Germanica II dispensirt!

Ein französischer Arzt liest in einem Journale die Anwendung von Syrupus Ferri jodati, er verordnet denselben auf Grund dieser ihm zuverlässig erscheinenden Angaben und ist nicht wenig erstaunt, dass von allen den gepriesenen Wirkungen nichts eintritt. Nun, die Lösung fand sich auch: der empfehlende Autor war ein holländischer Arzt; der Syrupus Ferri jodati Pharmacopöæ Neerlandicæ enthält in 100 Theilen 20 Theile Ferrum jodatum, d. h. ist 400 Mal stärker als der Syrupus Ferri jodati Pharmakopöæ Gallicæ! Einen Ueberblick von einzelnen Differenzen gewähren die hier folgenden Tabellen.

TABELLE I.

	In 100 Theilen des Präparates.						
	Ph. Americana.	Ph. Austriaca.	Ph. Britica.	Ph. Gallica.	Ph. Germanica.	Ph. Helvetica.	Ph. Neerlandica.
Acidum hydrochloric. enthält wasserfreie H. Cl	31.90	24.24	32.00	34.40	25.23	12.40	30.50
Acidum nitricum enthält wasserfreie Säure N_2O_5	59.50	48.00	60.00	54.50	25.50	18.57	46.20
Liquor Natri arsenicosi enthält Natrum arsenicosum *siccum*	1.00	...	1.00	0.1123	...	...	...
Syrupus Ferri jodati enthält Jodeisen	10.00	12.00	5.70	0.50	5 00	1.00	20.00
Tinct. Jodinæ enthält reines Jod	8.00	5.80	3.00 + 3.00 Ka. J.	7.70	9.10	10.00	7.70

Digitized by Google

TABELLE II.

Zur Herstellung von 100 Theilen der betreffenden Tinctur werden angewendet.

	Ph. Americana.	Ph. Austriaca.	Ph. Britica.	Ph. Gallica.	Ph. Germanica.	Ph. Helvetica.	Ph. Neerlandica.
Tinctura Aconiti	40.0	20.0	12.5	20.0	10.0	20.0 Folia sicca.	...
	Tubera siccata.						
	+ 0.4 Acidum tartaric.				Pharmakopöa Italica eodem modo.		
Tinctura Belladonnæ	15 0 Folia sicca.	20.0 Radix sicca.	5.0 Folia sicca.	20.0 Folia sicca.	10.0 Folia sicca, resp. 50.0 Folia recentes Pharmakopöa Italica eodem modo.	20.0 Folia sicca.	...
Tinctura Digitalis	15.0	20.0	12.5	20.0	10.0	20.0	25.0
	Folia sicca.						
					resp. bei Pharmakopöa Germanica 50.0 Folia recentes.		
Tinctura Opii simplex	10.0	10.0	8.2	14.0	10.0	10.0	20.0
Das zur Herstellung der Tinctura Opii verwendete Opium soll Morphium enthalten in Procenten	12–16	10	9.5–10.5	10–12	10	10–12	ohne Vorschrift

Noch schwieriger und unangenehmer sind die Verhältnisse bei der Darstellung der narkotischen Extracte, z. B. bei Extractum Aconiti.

Das Extractum Aconiti wird bereitet in der

Pharmakopöa Britica aus frischem Kraut und Blüthentrauben mit Zusatz des ausgeschiedenen Chlorophyll. Consistenz zwei.

Pharmakopöa Neerlandica aus frischem Kraut (aquosum); aus frischem Kraut mit Spiritus (spirituosum). Consistenz zwei.

Pharmakopöa Gallica aus trockenem Kraut mit kochendem Wasser. Consistenz zwei.

Pharmakopöa Helvetica aus trockenem Kraut mit Spiritus von 50 Procent. Consistenz ein.

Pharmakopöa Americana aus den trockenen Tuberis mit Spiritus 90 Procent und Säure ein Procent. Consistenz zwei.

Pharmakopöa Germanica, Pharmakopöa Austriaca, Pharmakopöa Italica aus trockenen Tuberis mit Spiritus von bez. 60 Procent, 70 Procent, 90 Procent.

Alle pharmakodynamischen Untersuchungen stimmen darin überein, dass die Tubera siccata bedeutend reicher an Aconitin sind als die Folia, und dass das beim Erhitzen von frischem Blattsafte sich ausscheidende Chlorophyll ganz unwirksam und frei von Aconitin ist.

Trotz diesen ganz bekannten Thatsachen lässt die Pharmakopöa Britica ihr Extractum Aconiti durch Auspressen der frischen Blätter und Blüthen mit Stengeln, Erhitzen des ausgepressten Saftes und Zufügen des ausgeschiedenen und abkolirten Chlorophylls zu dem eingedampften Safte bereiten, so dass jedenfalls das Extractum Aconiti Pharmakopöæ Britica das an Wirkung schwächste ist und das in demselben enthaltene Chlorophyll Zersetzungen und Verderbniss des Extractes noch möglichst beschleunigen wird.

Digitized by Google

Ein in Deutschland verschriebenes und in England angefertigtes Recept wird sicher nicht die gewünschte Wirkung haben! Umgekehrt kann ein in England verschriebenes und in Deutchland angefertigtes Recept sehr leicht zu unangenehmen nicht erwünschten Complicationen führen.

Stärker, und zwar mindestens um das Doppelte, ist das nach der Pharmakopöa Neerlandica e succo foliorum recentium bereitete Extractum aquosum, da das beim Erhitzen ausgeschiedene Chlorophyll u. s. w. dem eingedampften Safte nicht wieder zugesetzt werden darf. Erfahrungsmässig beträgt das ausgeschiedene Chlorophyll circa 60 Procent gegen 40 Procent des eingedampften Saftes.

Noch stärker ist das nach der Pharmakopöa Neerlandica analog dem Extractum Belladonnæ Pharmakopöæ Germanicæ aus dem frischen ausgepressten Safte der Blätter, nach dem Abkoliren des beim Erhitzen abgeschiedenen Chlorophylls durch Spiritus erhaltene Extractum Aconiti spirituosum, da hierbei sowohl das unwirksame Chlorophyll, als auch noch ein grosser Theil der sonstigen unwirksamen Schleim- und Extractivstoffe ausgeschieden werden, und somit eine weitere nicht unerhebliche Steigerung des Aconitingehaltes und der Wirksamkeit des fertigen Präparates bedingen. (1:2:3 nach Schroff.) Die aus den getrockneten Blättern durch Infusion mit kochendem Wasser (Pharmakopöa Gallica) oder mit verdünntem 50-procentigen Spiritus (Pharmakopöa Helvetica) bereiteten Extracte entziehen sich betreffs der Wirksamkeit jeder weiteren Beurtheilung, da bei dem Trocknen und der Aufbewahrung der narkotischen Blätter sich Einflüsse der mannigfachsten Art geltend machen und die wirksamen Bestandtheile in grösserem oder geringerem Maasse verändern.

Wirksamer als die aus den Blättern dargestellten Extracte sind jedenfalls die aus den getrockneten Tubera erhaltenen: 1. sind die Tubera a priori reicher an Aconitin, 2. enthalten dieselben weniger sonstige, leicht zersetzbare und weitere Zersetzung hervorrufende und bedingende Stoffe, 3. werden dieselben nur durch Extraction mit Spiritus hergestellt. Aber auch selbst die vier Pharmakopöen, welche die Anwendung der Tubera vorschreiben, also die Pharmakopöa Germanica, Pharmakopöa Austriaca, Pharmakopöa Italica und Pharmakopöa Americana, weichen in der Bereitungsweise untereinander ab. Die drei zuerst aufgeführten Pharmakopöen schreiben resp. 60 Procent, 70 Procent und 90 Procent Spiritus bei gleicher Extractions-Consistenz (2) vor, während die zuletzt genannte Pharmakopöa Americana den 90-procentigen Spiritus mit 1 Procent Weinsäure versetzt anwenden lässt und das wirksamste Extract erhält. Wir dürfen uns daher nicht wundern, wenn nach den betreffenden Publicationen das spirituöse Extractum Aconiti e tuberis 6–7 Mal stärker wirkt als das Extractum e succo foliorum recentium!

Das sind in der That traurige Verhältnisse, die eine Verständigung auf dem Wege internationalen Zusammengehens gewiss dringend nothwendig machen!

Eine Internationale Pharmakopöe würde derartigen Uebelständen und Verschiedenheiten gründlich abhelfen; dieselbe würde dem Arzte und dem Pharmaceuten in gleicher Weise nützlich sein und würde jedenfalls von allen Seiten als eine wichtige Errungenschaft mit grosser Freude begrüsst werden.

An der Möglichkeit, das vorgesteckte Ziel zu erreichen, wird wohl Niemand in den betheiligten Kreisen zweifeln.

So wie sich das metrische System nach und nach Bahn gebrochen und in wissenschaftlichen Werken eingebürgert hat, so wird sich auch bei ernstem Willen und eifrigem Streben eine Internationale Pharmakopöe herstellen lassen und einbürgern, wenn von allen betheiligten Kreisen ein solches Verlangen einmüthig ausgesprochen und von allen Seiten gefördert und betrieben wird.

Und es dürften sich wohl auch trotz den mannigfachen, gewiss nicht zu unterschätzenden Schwierigkeiten die geeigneten Wege finden lassen, um dieses Ziel zu erreichen.

Digitized by Google

Es sei gestattet, hier einige passend erscheinende Vorschläge zu machen. Zur Motivirung derselben gehen wir auf die bisherigen Bemühungen der verschiedenen Internationalen pharmaceutischen Congresse zurück.

Bei der Ausarbeitung einer Pharmakopöe, namentlich aber einer Internationalen Pharmakopöe, welche für eine grosse Anzahl von Kulturstaaten maassgebend sein soll, muss die Therapie und Pharmakologie als gleichberechtigte Wissenschaft neben der Pharmacie stehen und in einer Internationalen pharmaceutischen Commission muss der Mediciner zuerst gehört werden, bevor der Pharmaceut mitspricht.

Der Mediciner muss sich darüber aussprechen, welche Medicamente er für nöthig oder wünschenswerth erachtet, er muss darüber Auskunft geben, ob ihm stark concentrirte oder verdünntere Arzneimittel angenehmer zur Dosirung sind, und an Hand und nach Kenntniss dieser medicinischen Wünsche muss der Pharmaceut bemüht sein, die nöthigen Drogen zu beschaffen, die chemischen Präparate und galenischen Mittel herzustellen und dem Arzt zur Prüfung übergeben.

Finden sich dann Schwierigkeiten, lassen sich die Anforderungen des Arztes nicht erfüllen, entsprechen die erhaltenen Präparate nicht den ärztlichen Anforderungen, dann muss durch Versuche, Verhandlung und Besprechung ein Compromiss herbeigeführt werden, und eine Verständigung dürfte sich wohl immer herbeiführen lassen.

Mein Vorschlag würde dahin gehen, eine Commission zu erwählen, welcher Therapeuten, Pharmakologen und Pharmaceuten der verschiedenen Länder angehören. Dieselbe würde einen Vorsitzenden, zwei bis drei Stellvertreter desselben und einen *ständigen* Sekretär haben, und in der oben angedeuteten Weise nach näher festzustellendem Plane an die Bearbeitung des Entwurfes einer Internationalen Pharmakopöe herantreten.

Diese Commission würde die einzelnen Arbeiten unter sich vertheilen, geeignet erscheinende Persönlichkeiten um Auskunft ersuchen, durch ihre eigenen Mitglieder oder durch andere zuverlässige Personen die nöthigen Untersuchungen und Arbeiten vornehmen lassen und auf Grund der erhaltenen Resultate die nöthigen Ausarbeitungen vornehmen.

Die Haupt-, um nicht zu sagen die einzige, Schwierigkeit würde die finanzielle Seite bilden.

Wenn auch die Mitglieder der Internationalen Pharmaceutischen Commission ihre eigene persönliche Arbeitskraft vielleicht unentgeltlich der ganzen Sache widmen und die Arbeiten als eine wissenschaftliche Ehrensache behandeln, so dürften doch durch Portos, Correspondenz, Reisen, u. s. w., u. s. w., immerhin Kosten enstehen, deren Deckung durch einen Einzelnen nicht verlangt und erwartet werden kann.

Vielleicht dürfte es gelingen, durch Beiträge Seitens der einzelnen ärztlichen und pharmaceutischen Vereine das nöthige Geld zu beschaffen, vielleicht übernehmen die einzelnen Staats-Regierungen die Unkosten, vielleicht auch finden sich reiche Private oder Fabrikanten, welche im Interesse der Sache die nothwendigen Mittel flüssig machen und zur Verfügung stellen, so dass es auf diese Weise ermöglicht werden kann, eine Internationale, die gesammten Kulturstaaten umfassende Pharmakopöe auszuarbeiten und einzuführen.

Digitized by Google

Dr. Wm. Ward, of Washington, read a paper (which was not concluded before adjournment) on—

SUPPLEMENTAL RESPIRATION SUPERINDUCED IN CHOLERA AND POST-PARTUM HEMORRHAGES, BY ADMINISTRATION OF SUITABLE OXYGEN-YIELDING COMPOUNDS, IN SOLUTION, THROUGH THE STOMACH, AS A RATIONAL MEANS OF TREATMENT.

RESPIRATION SUPPLÉMENTAIRE APPORTÉE DANS LE CHOLÉRA ET LES HÉMORRAGIES POST-PARTUM, PAR L'ADMINISTRATION DE COMPOSES D'OXYGENE, EN SOLUTION DANS L'ESTOMAC, COMME UN MOYEN RATIONNEL DE TRAITEMENT.

DIE HERVORBRINGUNG SUPPLEMENTÄRER ATHMUNG BEI CHOLERA UND PUERPERALEN BLUTUNGEN, DURCH EINFÜHRUNG VON GEEIGNETEN, SAUERSTOFF ABGEBENDEN LÖSUNGEN IN DEN MAGEN, ALS RATIONELLE BEHANDLUNGSMETHODE.

Has the alimentary tract in man the power of supplementing the action of the lungs by absorption of oxygen-yielding substances and admission of them into the circulation, for the purpose of effecting those tissue changes incident to the respiratory acts?

In certain diseases, the alimentary canal has the power of thus supplementing the action of the lungs, of passing oxygen into the tissues for nutrimental purposes, and of expelling carbon monoxide as excremental matter.

As far as oxygen is concerned, it is intestinal respiration and not intestinal digestion; and as far as both oxygen and water are concerned, it is respiratory absorption and not digestive absorption, which I advocate. This phenomenon is observed also in the hypodermic tissues, under the same conditions, when oxygen, or oxygen-yielding substances in solution are injected therein; oxygen being absorbed without the action of digestive ferments, and oxygen in oxygen-yielding substances, liberated by electro-chemism of the animal membrane, being absorbed in like manner. Letourneau, however, in his work on Biology, says: "So far, at least, it has not been shown that the chemical reactions of (animal) nutrition set at liberty oxygen gas." His views, which are of great scientific value, are from a physiological standpoint; mine from clinical observation.

Physiologists universally hold the opinion that oxygen is admitted into the system through the lungs only, and guided by them, physicians generally entertain the same views. In health, respiration and nutrition are so perfect, that it is very probable that all the oxygen necessary is carried into the system through the lungs; but in certain diseases—cholera, cholera-infantum, post-partum and secondary post-partum hemorrhages—in which the lungs and blood fail in function, in consequence of an enormous loss of blood or serum by the vital organs, the system doubtless has the power, through the chemical, physical and electrical processes of absorption and nutrition, of liberating oxygen gas, and utilizing it in tissue changes, if that gas in suitable chemical and absorbable combination be given through the stomach.

Heretofore, pulmonary respiration has seemed to fulfill so completely the purposes of ventilation of the human organism, that this question of supplementary respiration in disease has never before been advanced. However, it is neither a vain theory nor a philosophical conjecture; it is a clinical fact.

Letourneau distinguishes "between the fundamental biological fact of the absorption of oxygen which is one of the first conditions of nutrition, and the physiological function, properly so called, which is simply the physiological process employed to render this absorption possible and easy."

There are two chief modes of breathing in the vertebrates, one by the pulmonary,

Digitized by Google

the other by the branchial apparatus; and in these there is no fundamental difference. The gills can act in the air, provided they be kept sufficiently moist, while the pulmonary mucous membrane is ever in a state of humidity. Now the fundamental phenomenon of respiration is extremely simple; it is merely the abstraction of oxygen from the air, formation of carbonic acid and its expulsion from the tissues—an electro-chemical and a physical process—falling under the laws of chemism, diffusion and osmosis. In the pulmonary tissues, in addition to the forces of diffusion and osmosis, the electro-chemical couplets impel the oxygen across the membrane into the blood, where it unites with the hæmoglobin, and dissociate and expel carbonic acid; in the stomach and bowels, in diseases mentioned, when dilute sulphuric acid and water are given, a variety of aquatic respiration is produced. Diffusion and osmosis carry the acid and water into the walls of the alimentary canal, and these same electro-chemical couplets decompose the acid and liberate oxygen; the red corpuscles carrying the oxygen into the circulation, while the tissues, yielding to the harmony of chemical law, associate that oxygen with the carbon and hydrogen of regressive organic matter, forming carbonic acid, and water, which are carried to the lungs with the reduced hæmoglobin, whither the blood is sent to expel carbonic acid, and whence it issues again oxidized, and again sent forth on its mission.

Electromotricity of the tissues is produced by an action similar to that of electrolysis. We are indebted to Becquerel for our knowledge on this subject. In a series of papers, extending from 1867 to 1870, he communicated the results of his investigations to the Academy of Sciences, Paris.

According to the felicitous idea of Claude Bernard, all organisms, in fact, live an aquatic life; the histological elements are aquatic; the blood of animals and the sap of plants alike bathe them in a fluid medium. The oxygen we breathe is condensed and liquefied in the blood, is fixed in the blood corpuscles; our food is liquefied during digestion and is carried to the tissues in a fluid state. Life is therefore maintained, as it were, in a sea of histological and aqueous elements; and be it the air we breathe, or food we eat, all is reduced to a fluid state before absorption into the human system.

The essential act of respiration is oxidation of the effete anatomical elements, and the lungs are but "ports of entry" for the oxygen of the air carried thither. The red corpuscles therefore, acting as carriers of oxygen, functionate as indispensable in the respiratory acts. The corpuscles of the blood being present in every part of the circulation, when the lungs fail in function, by "spasm of the pulmonary arterioles," or anæmia of the pulmonary tissues, supplemental respiration may be superinduced by vicarious action of other tissues, through the blood, in other parts of the circulation, notably the intestinal and hypodermic tissues.

In case of an excessive drain on the vascular system, as in cholera and post-partum hemorrhages, when life is threatened by anæmia of the lungs and brain-asphyxia—if the suitable oxygen-yielding substance be given in solution, it will be absorbed by the vascular network of the stomach and bowels, the chemical compound broken down—de-oxidized—and oxygen carried hither and thither by the blood corpuscles and the liquor sanguinis— according to the selective affinity of this or that organ—to meet the respiratory and nutritive demand of the tissues. Oxygen thus evolved and condensed by the hæmoglobin of the blood, like that liberated by electrolysis on the surfaces of plates of metal, is said to be nascent. The employment of bodies in the nascent state has proved of the utmost service in organic chemistry; and it is said that the great advances in this branch of science are largely due to the utilization of this property. Per contra, Dr. H. C. Wood, as elsewhere quoted, says: "It is seemingly self-evident that the physiological action of a remedy can never be made out by a study of its use in disease." Besides, Beaunis states that "Intestinal respiration, which presents a certain importance in some animals, as the cobitis fossilis, has hardly any in man."

Digitized by Google

The principal function of the stomach is the secretion of gastric juice—this characteristic being that of a secreting organ, and unfavorable to absorption generally, which takes place mostly in the small and partly in the large intestines. Kuss cites the case of a man suffering from obstruction of the pylorus, whose stomach appeared perfectly healthy, but who experienced constant thirst, in spite of having swallowed large quantities of water. Thirst was relieved only by injection of water into the rectum. Aliments pass out of the stomach in a semi-fluid mass, and attain additional fluidity when they reach the small intestines and acquire bile, pancreatic and enteric juices. Here this mass rapidly loses its fluidity by absorption, and is driven into the large intestines, whence it is issued, after further absorption, in a semi-solid form. The state of the blood has also a great effect on the intensity of absorption; it is slow in hydræmia, normal in health and active after bleeding and purgation. Absorption, however, takes place only when the membranes of the stomach permit, and those of the intestines favor this action. The decayed and wasting epithelium of the intestines, which is removed in the normal state partly by the bile, which is poured into the intestines seven or eight hours after the ingestion of food, may be removed also in the abnormal state of cholera, etc., partly by the influence of dilute sulphuric acid, if that acid, properly diluted, be given through the stomach in that disease. Absorption resulting from the former action is digestive, that from the latter is respiratory. In cholera, the shrunken tissues and the thickened condition of the blood point to the systemic want of water, but the tissues will not allow it passage; and it is only when we remember that the physiological exigencies require not water alone, but oxygen also, that we can obtain that result. In cholera and post-partum hemorrhages, when normal absorption is arrested, or reduced to a minimum, water may be absorbed through the agency of the oxygen in sulphuric acid; the restoration of diffusion, osmosis and electro-chemical action being effected thereby; the blood globules appropriating all of the oxygen, and the sulphur, seeking its next affinity in a union with hydrogen, forms hydrogen sulphide. The tissues, however, are the active, and the acid and water the passive agents, in these histological changes; for assuredly, if the tissues did not control absorption, the acid would destroy them.

"Every living element" says Letourneau, "is athirst for oxygen, and to such a degree, that sometimes certain organisms steal it even from stable chemical compounds."

"Vibriones, studied by Pasteur, decompose the tartrate of lime and transform lactic acid into butyric acid, to procure for themselves oxygen. It is, besides, by an analogous process that, in most of the vertebrates, the anatomical elements deoxidize the blood corpuscles."

These facts and observations indicate that every living being, under the stress of circumstances, has the power of exerting deoxidation for the preservation and maintenance of life. What those circumstances and conditions are, I shall now endeavor to show.

To elucidate this point, I deem it important to explain the similarity between the symptomatology of barometric and corpuscular deoxygenation of the blood in man; because it is of paramount importance that a sufficiency of red corpuscles should always be moving in the ceaseless circuit of respiration from the lungs to the tissues and from the tissues to the lungs. I therefore call attention to the effects of mountain climbing and balloon ascension on the human system, before speaking of veritable diseases. Of course, I speak within a certain limitation. In ascending lofty mountains and in balloon ascensions, according to physiological observations, as soon as a certain height is reached, varied by the individuality of him who goes thither, the pressure of the air is felt to be greatly lessened; the blood vessels of the surface, and the right side of the heart become filled, and those of the internal organs correspondingly empty; a quasi disease is produced, which is characterized by hyperæmia of the superficial, and anæmia of the deep tissues. The mountain climber and the aeronaut alike suffer from afflux of blood—in fact, hemorrhage—into their cutaneous vessels; the more blood there is in the

Digitized by Google

superficial, the less in the deep tissues; and, in losing blood, lose plasma and corpuscles, lose oxygen stored up in the corpuscles and the plasma in which they are carried. There is consequently anæmia of the abdominal viscera, with attendant respiratory and circulatory disturbances. Many of these identical symptoms occur in cholera and post-partum hemorrhage; the afflux of blood, however, goes to the bowels in cholera, to the uterus in post-partum hemorrhage, in both of which the system suffers actual loss of blood and serum, and anæmia of the lungs and brain follows: The ascent of great altitudes produces congestion of the capillaries of the skin, and free surfaces of the mucous membranes, with diarrhœa and nausea, and sometimes hemorrhages and sweating; while in cholera there is congestion of the bowels and closed mucous membranes, with diarrhœa and nausea, and sometimes sweating and hemorrhages; in great altitudes, the blood pressure on the skin increases the cardiac beats, and respiration becomes impeded; while in cholera the vascular pressure in the bowels increases the cardiac beats, and respiration becomes impeded; in great altitudes there is insufficient supply of oxygen and imperfect elimination of carbonic acid, owing to anæmia of the abdominal viscera, with extraordinary muscular weakness; while in cholera there is insufficient supply of oxygen and imperfect elimination of carbonic acid, owing to anæmia of the lungs with extraordinary muscular weakness; in great altitudes, the vagal centres are stimulated by insufficiency of oxygen and vomiting generally occurs; while in cholera, the vagal centres are stimulated by insufficiency of oxygen, and vomiting nearly always occurs; in great altitudes, the blood being drawn from the internal organs to the surface, the brain is imperfectly supplied with oxygen, and faintness results, as well as diminished secretion of urine; while in cholera, the blood being drawn to the bowels, the brain is imperfectly supplied with oxygen, also the lungs, and faintness results, as well as diminished secretion of urine; in great altitudes the traveler and the aeronaut alike complain of intense cold, owing to extraordinary loss of heat by radiation through the skin, and the failure of the system to reproduce heat sufficient to supply the extra demand in consequence of anæmia of the internal tissues; while in cholera there is a typical cold stage —the collapse—owing to the extraordinary loss of heat by discharges from the bowels and the failure of the tissues to reproduce normal heat in consequence of anæmia of the lungs and brain; in great altitudes there is a peculiar form of venous congestion, particularly marked in the superficial tissues and right side of the heart; while in cholera there is another form of venous congestion particularly marked in the deep tissues—abdominal viscera and right side of the heart; in great altitudes there is barometric deoxygenation of the blood; while in cholera there is corpuscular deoxygenation of the blood.

All the world knows that the remedy for "mountain sickness," anoxyhæmatism, is to descend to a lower altitude. No better remedy can be given. This parallel is drawn to show that the immediate cause of death from cholera, like that from mountain-climbing and balloon ascension, is from oxygen-starvation—asphyxia; the same is true of post-partum hemorrhage and cholera-infantum.

Fothergill, on the authority of Parkes and George Johnson, says: "The now generally acknowledged starting point of the pathological processes in cholera is spasm of the pulmonary arterioles. Elaborate experiments as to how to relax such spasms, ending in clinical application of various agents, as belladonna, etc., have not yet, however, attained any practical results of moment." My observations, based on other premises, lead me to another conclusion. In cholera there are out-pourings of the fluids of the body so enormous, abstraction and inspissation of the corpuscles of the blood so great, and anæmia of the lungs so marked, that the interstitial tissues, the greatest sufferers, and those corpuscles which have escaped destruction, as well as the young corpuscles which act as carriers of oxygen but are unable to make their way to the lungs, find themselves in both an oxygen and a water famine. This condition clinically denomi-

Digitized by Google

nated the collapse, or approaching collapse, is produced by asphyxia. This is why cholera is sometimes called cholera asphyxia.

This pathological condition of Asiatic cholera, though merely outlined, suggests the physiological exigencies of the victims of that disease. In cholera there are impeded circulation and insufficient development of animal heat; consequently the nitrogenous elements of the food and tissues fail to be oxidized and converted into urea. There is also ample material for combustion in the human economy, but no oxygen to effect it, and no water to transport that oxygen. Sulphuric acid and water respond to the systemic wants of cholera in furnishing both oxygen and water, which relieve the thirst and vomiting, gradually check the diarrhœa, favor the formation and excretion of urea, and restore the circulation and respiration. At the same time the decaying and wasting epithelium of the alimentary canal, upon which the microörganisms of cholera live and thrive, are oxidized and removed. Moreover, clinical observation leads me to infer that sulphuric acid and water sterilize the membranes of the alimentary tract and all other tissues of the body.

This acid has long been used empirically in cholera and other diseases also, and has its enthusiastic advocates on the ground of experience.

If sulphuric acid, largely diluted with water, be given through the stomach during an epidemic of cholera, cholera will be prevented by furnishing oxygen in a fluid medium, thus maintaining and restoring the dynamical forces of the circulation and sterilizing the membranes; given early in the disease, the collapse will be averted; given in the collapse as an oxidant—oxygen-yielding substance—it is the most rational treatment. Of course, other medicines will be required if there be complications; in all cases a suitable diet will be requisite, and absolute rest must be enjoined. This experience is based on observation made while I was a student.

The observations of Dr. R. G. Curtin, in the Philadelphia *Medical Times*, are as follows: "A very severe epidemic of cholera ceased in the Insane Department of the Philadelphia Almshouse within twelve hours after the lunatics were all put upon the free use of sulphuric acid lemonade; the only new case after this being a man who refused to use the prophylactic. Two days after the use of sulphuric acid was stopped two new cases occurred, and the epidemic was again arrested by the use of the acid. In the surgical wards of the Hospital Department, the acid was used from the beginning of the epidemic; and these wards, although in no way isolated, were the only parts of the institution unvisited by the disease."

In post-partum hemorrhages, we have practically the same pathological conditions and the same physiological exigencies. The following cases are given to illustrate these views: In a case of secondary post-partum hemorrhage, No. 1, occurring July, 1880, ten days after delivery, the hemorrhage was stopped by manual expulsion of clots, the administration of teaspoonful doses of fluid extract of ergot every two or four hours, introduction of a silk handkerchief saturated with vinegar into the uterine cavity, and lastly, by repeated irrigation of the rectum with hot water, to remove all scybalæ, and to propagate heat to the uterus and its blood vessels, with a view to the transmutation of that heat into motion, and motion into contraction; besides, by abstracting carbonic acid with the hot water, favor the same action. These means were successful, but as the patient had lost so much blood before I arrived, she was very weak and prostrated. Milk and beef tea were given every two hours, and drachm doses of ergot every two or four hours. The next day the prostration continued, and the temperature remained subnormal. Milk, beef tea and ergot were continued. The third day after the hemorrhage her condition was most deplorable. She had cold hands and feet, subnormal temperature, weak and feeble pulse, and repugnance to all food; yet she was thirsty. Gave granulated ice and added lime water to the milk. I returned in an hour and finding her no better, gave a

Digitized by Google

hypodermic of ergotine and brandy. I saw her again in an hour and a half; she was greatly changed; changed for the worse. In my absence a draught of whisky and mint water was given her, but her stomach rejected it at once. Her hands and feet were then stone cold, there was no pulse at the wrist, she vomited everything given her, even grated ice, soon after it melted. Her breath felt cold to the palm of the hand, respirations were slow, and heart sounds scarcely audible. Her features were pinched and shrunken in appearance, her mind was apathetic, and, as I learned the next day, there was a sense of impending death. The collapse was complete and intense. At this moment I gave her thirty drops of aromatic sulphuric acid in twelve ounces of water, all of which she drank eagerly while the nurse held up her head. She went to sleep immediately, and slept like a child. The hour was four o'clock, P. M. In an hour and ten minutes there was a most remarkable change. Her hands and feet became warm, the pulse returned to the wrist, and her features and complexion were natural in appearance. My examination of the patient awakened her, and she asked for a drink of water. I gave her twelve ounces of cold water with ten drops of aromatic sulphuric acid, and she at once slept again, sleeping until eleven o'clock, P. M., when she awoke and asked for a drink of water. The nurse gave her twenty drops of aromatic sulphuric acid in twelve ounces of cold water, and she slept soundly until five o'clock, A. M. She was then given her last dose of medicine—twenty drops more of the acid, and twelve ounces of water. At this time she remarked to the nurse, "I thought I died yesterday evening at six o'clock." Her condition at eight o'clock in the morning was marvelously improved. Her pulse was quite strong, temperature natural and her appetite craving; she asking for something more than beef tea and milk. The uterus was firmly contracted and much reduced in size; while the lochia, the true lochia, that yellowish or yellowish-green matter, the ash and cinder of oxidation after parturition, was established and found on the expectant napkin. The patient was declared to be convalescent. She made a speedy recovery.

Now what caused this collapse? A breach in the continuity of the tissues of the capillary walls caused a great drain on the system in the hemorrhage which ensued; a loss of the dynamic force of the circulation, known as "the vis a tergo;" an accumulation of venous blood in the venous column up to the heart; slow movement of the blood in the capillaries of the lungs, and from the right to the left side of the heart, producing arterial anæmia. There was impeded respiration, in consequence of the resulting anæmia of the lungs, arrest of osmotic and electro-chemical action in the tissues, and insufficiency of blood in the respiratory circuit, from the lungs to the tissues, from the tissues to the lungs, to keep up animal heat.

Another case of secondary post-partum hemorrhage; No. 2; appearing suddenly eleven days after labor, at one o'clock, A.M., February, 1881, was most profuse and alarming. The patient was twenty-four years of age, ruddy, plump and robust; and up to this time had remarkably good health. This was her third child; I had attended her in all her confinements, and never found it necessary to give her any medicine, save two or three drops of ergot, which were given as a preventive, after each parturition. After this confinement she was not doing so well; on the third day her temperature arose to 101° F., on the fourth, to 102° F. Her face was flushed and swollen, and she seemed plethoric. I prescribed for her, but the medicine was not taken, as she said she felt quite well. On the ninth day I found her sitting up in a chair; when surprise was expressed, she declared she felt well, and could not stay in bed. At this time the discharge was scant, yet sanguinolent; and, as involution had not been effected, the dangers of hemorrhage were suggested. She sat up only one hour on this day; on the next day, however, the tenth, she remained up nearly all day. At three o'clock, A. M., two hours after the hemorrhage appeared, I gave her thirty drops of aromatic sulphuric acid in a tumbler of cold water; and ten drops of the acid in half a tumbler of cold water

Digitized by Google

every two hours thereafter. Beef tea, rice water and chicken tea were given as diet, every two or three hours. This patient, from the first dose given, did so well, that I resorted to nothing else to restore her. The next day this treatment was continued until twelve o'clock, M., when the true lochia appeared, the product of oxidation and hydration liquefied. The uterus, felt through the abdominal walls, was found to be firmly contracted and small in size. There was much more blood lost in this case than in the first (No. 1), yet the patient did not get cold or even cool, although it was winter; there were no signs whatever of collapse. The second night after hemorrhage, however, I was compelled to give eighty drops of tincture of opium to produce sleep, giving twenty drops every hour. No other medicine was given, and she made a complete recovery in seven days. There was no loss of animal heat.

No. 3 was a case of post-partum hemorrhage, following immediately after parturition, without a moment's notice. It commenced before expression of the placenta, the uterus and abdomen being found greatly distended with blood, after ligation of the funis umbilicalis. Expressing the placenta, the uterus was well squeezed and contracted; as soon as the hands were removed, however, a most profuse hemorrhage set in; great pallor, anxiety, jactitation, and sighing respiration were manifested. Two doses of ergot were given, a drachm in each, one in cold, the other in iced water, and both were immediately vomited, not partly, but entirely. She turned and tossed on the bed, and rolled herself on the other side, seemed sleepy, the hemorrhage continuing. Thirty drops of aromatic sulphuric acid were given in a tumbler of cold water, and it acted with marvelous rapidity. The hemorrhage was arrested at once, color returned to the face, the pulse which was 130 fell to ninety, respiration became normal, and the patient in less than twenty minutes gave expression of admiration for the medicine, which she said went tingling all over her, like electricity. This was March, 1883, eight o'clock P. M. I gave her another dose of twenty drops in a tumbler of water at eleven o'clock, and as the uterus was firmly contracted, gave her no more. However, I watched her all night, resting in the same room. The nurse also kept vigil. No other medicine was given. She made a complete and rapid recovery, and arose from her bed on the twelfth day after delivery. There was no loss of animal heat.

In another case, No. 4, of post-partum hemorrhage, apparently resulting from uterine inertia, occurring February, 1885, in which the uterus remained patulous after delivery, a piece of ice being placed over the hypogastrium produced immediate contraction and stopped the hemorrhage. Fluid extract of ergot was then given in teaspoonful doses every two hours until one ounce was thus used. The patient was in a comfortably warm room and well blanketed, yet her hands, arms and feet were very sensibly cold to the touch until midnight. Nor was animal heat entirely restored until twenty-four hours after the flooding was arrested, although the patient was fed on milk and beef tea every four hours. A sanguinolent discharge kept up for twenty-four days; after that period it became yellowish in appearance.

Case No. 5 was one of secondary post-partum hemorrhage, occurring four days after delivery. Dyspnœa was very marked in this case—the patient imploring the nurse to fan her. As soon as thirty drops of aromatic sulphuric acid in twelve ounces of cold water was given her, I silently withdrew the fan from the servant and placed it on a table. The hemorrhage was immediately arrested, nor did the patient again ask to be fanned, as the dyspnœa was at once relieved. Another like dose was given her in two hours. She required no more medicine, and made a complete recovery in a few days. There was apparently no loss of animal heat.

Now it may be asked: How are this and other acids absorbed? How are they utilized in the human system? According to the theory of tissue respiration it is the increase in the intra-molecular actions, in the cells themselves, that occasions the demand for oxygen, and a more active condition of the circulation and respiration; and

Digitized by Google

although the quantity of hæmoglobin in the blood is the measure of the oxidizing power within the body, it is the tissues themselves that determine the amount of oxidation. Many experiments show that oxygen enters the circulation through the lungs by diffusion of gases, by osmosis and by chemism. To these already acknowledged forces, Becquerel adds that of electro-motory actions.

This important and progressive view of the physiology of the capillaries, which is well supported by numerous experiments, brings us to the consideration of a new phenomenon of the circulation, and adds another phase to the connection between respiration and the circulation. If, through the agency of the Voltaic pile, we decompose water placed in a vessel divided by membrane, we shall see the level of the surface rise in the negative division; the water is, therefore, carried from the positive to the negative pole. According to Becquerel, analogous action takes place in the capillaries. Owing to the innervation of the tissues, the chemical and physical phenomena going on in them, electro-capillary currents are produced, phenomena peculiar to biological osmosis, and never effected in the endosmometer. In the capillary vessel, arterial blood is positive to all the other tissues thereabouts, hence the electro-capillary currents drive the oxygen into the surrounding histological elements wherein oxidation of the tissues occurs, producing carbonic acid, which is driven into a corresponding or an adjacent venous capillary, and turned into the great venous current on its way back to the lungs. In the pulmonary capillaries, electro-chemical actions are reversed. In the lungs, in consequence of the oxygen tension being less than the tidal air, the oxygen is impelled into the circulation by electro-capillary couplets. The greater positive of the stratum of tidal air prevails over the less in the pulmonary tissues, and oxygen is thus carried into the circulation, whence carbonic acid is eliminated. According to Becquerel "Electro-chemical capillary actions rest on a principle fertile in application, in nature organic as well as inorganic; since they are always manifested when two liquid conductors of electricity—having affinity for each other—are separated by a partition, of whatever nature, with capillary interstices in which these liquids are introduced by capillary affinity, then reacting one on the other disengage electricity, whence results an electric current by the intermediary of the excessively small liquid layer—or stratum—which adheres to the surface and acts like a solid body conductor; it is always indispensable that the current should be constant, because these latter surfaces are constantly depolarized; this is necessary so that the elements deposed by electro-chemical actions may be raised by the surrounding liquids. Such is the couple, which we have called electro-capillary, by the aid of which one can reduce to the metallic state the majority of the metals and deoxidize others of them. To produce this effect, place the metallic solution in a cracked tube sealed at one end, and immerse it in a solution of monosulphide of sodium, or still better, two broad laminæ of glass connected with each other by lines, and plunge them likewise in the solution."

"Electro-capillary action is produced in all bodies with capillary pores of some $\frac{1}{100}$ of a millimetre in extent which divide two liquids, according to the condition just indicated." Becquerel then proceeds to elucidate every point in the electro-chemical capillary couplet, and explains how this action takes place in organic as well as inorganic tissues, noting the difference that organic tissues have nervous connection, which have great effect and increase the action, while inorganic tissues have none.

It is well known that sulphuric acid, largely diluted with water, has a degree of diffusibility specially great; it is moreover, the most powerful acid known. When thus diluted and introduced into the stomach, in cholera and post-partum hemorrhages, chemical reactions and transformations of great clinical consequence follow. In these diseases, the tissues are famishing, perishing for want of oxygen and water; oxygen to sustain the failing circulation and eliminate effete organic matter; water as a medium in which to float both the nutritive and waste materials of life in requisite molecular

Digitized by Google

division. This waste matter retained in the system tends to keep up a state of high tension and fullness in the venous capillaries, to annul all physiological laws, and establish those of transudation. Oxygen, as the motive power of the circulation, owing to its great affinity for decaying organic cells, removes waste and liberates heat, while it produces motion in the circulation, by its electro-chemical effect.

Nor must the influence of oxygen on the nervous system be omitted. The splanchnics act as inhibitory nerves on the movements of the intestines so long as a normal current of oxidized blood traverses the vessels; but when the blood becomes venous, inhibition is held in abeyance. This is a marked pathological condition in cholera, and consequently an immense accumulation of blood occurs in the abdominal vessels. The splanchnics are also the chief vasomotor nerves of the arteries of the abdominal viscera. This point should be considered in post-partum hemorrhage also.

Now, it is only through the intermedium of the red corpuscles and the plasma floating them, that the tissues hold commerce with the air; and, when the paucity of these corpuscles in the respiratory circuit is great, the whole system suffers. As to the stomach, when the vagal centres are stimulated by insufficiency of oxygen, vomiting generally occurs. This is owing to the fact that the vomiting centre in the medulla, which has a close relation with the respiratory centre, is excited in a reflex manner by efferent impulses descending the vagi.

The tissues laden with carbonic acid are athirst for oxygen, and the young corpuscles ever springing up in the vascular area and waiting to functionate as vehicles of oxygen, and the red corpuscles despoiled of oxygen present themselves in the venous capillaries of the intestines, project themselves into the organic walls, or by amœboid movement wander into the intra-molecular spaces.

The organic membranes of the stomach and bowels which separate the blood from the tissues, are as living as the blood circulating therein. In these diseases of which I have spoken, powerful osmotic and diffusive currents arise, when diluted sulphuric acid is given medicinally. The epithelial tissues of the bowels thus absorbing the acid, chemical transformations in the walls themselves, and in the strata of liquids on either side, one of which deporting itself as an acid and the other as an alkali, give rise to electro-chemical actions; sulphuric acid is deoxidized, the oxygen is impelled into the interstitial tissue, unites with the hydrocarbons and the carbohydrates of the regressive tissues and forms carbonic acid and water, which are driven into the venous capillaries, and hurried into the general venous current; at the same time, urea and the urates, formed by oxidation of the disintegrated nitrogenous elements of the tissues, are eliminated by the kidney. Doubtless in health the sulphates and phosphates are formed by decomposition of the alkaline and earthy salts of the blood; not so in the diseases mentioned, in which the affinity of oxygen for the red corpuscles is greater than that for the alkaline and earthy salts, and greater for the tissue than the corpuscles; for the anatomical elements have their own special affinities which are entirely in harmony with chemical laws. This action of the electro-chemical couplets, in the capillary walls of the stomach and bowels, relieves the irritation caused by want of oxygen—as manifested by emesis or irritability of that organ—while the alimentary tract is thus supplementing the action of the lungs; but the full organic expression of the tissues, arising from this action, is evinced by the changes effected in the vascular network and hypertrophied walls of the uterus. The effete and spongy cells in the capillary wall, which cause the breach in that vessel and keep it patulous, are oxidized and disassimilated by the oxygen of the red corpuscles carried thither by the blood. New cells glide into the places of the old cells, and, exerting their contractile power, heal the breach; while the old cells, liberating energy in their death by the action of oxgyen, give rise to the so-called "vis a tergo," which removes the venous congestion, causes the blood to traverse the lungs more rapidly, passing from the right to the left

Digitized by Google

side of the heart, and filling up the arteries. Oxygen and water thus restoring the pristine vigor of physiological respiration, and the circulation also. In post-partum hemorrhages, the physiological hypertrophy of the uterine walls has to be cut down by this same electro-motory action, by oxidation and elimination, otherwise, uræmic symptoms supervene; in cholera, the effete and porous cells of the glands and membranes of the intestinal tract, which favor the enormous discharges of that disease, are oxidized and cast out, while new cells take their places and, exerting their contractile function, restore the healthy continuity of the tissues. I venture to say that in the future electro-motility of the tissues will find recognition in clinical medicine.

This exposition, well founded or not, does not seem necessary to explain the action of dilute sulphuric acid in the cases under consideration. It is sufficient to remember that these diseases tend to produce death by asphyxia; and, in the words of Austin Flint, "The distention of the heart in asphyxia is due to the fact that unaerated blood cannot circulate in the capillaries." It has lost its chemical affinity for the vessels and tissues, and cannot traverse the lungs as in the physiological state. In post-partum hemorrhage there is a breach in the continuity of the vascular wall; in cholera, the blood falls under the influence of the laws of transudation. We have, therefore, in these diseases, as a consequence, anæmia of the lungs, a tendency to death by collapse, or loss of animal heat. Aromatic sulphuric acid and water, in case No. 1, restored animal heat, averted death by collapse, relieved vomiting, returned the pulse at the wrist, revived the failing respiration, and established the lochial discharge—the product of oxidation and hydration. This is just what oxygen, and nothing but oxygen, could have effected. It is the source of animal heat; it gives rise to the formation of carbonic acid and water, products of complete oxidation in the animal economy; and it causes the formation and elimination of urea and uric acid. Oxygen is the cause and agent of all combustion, of all oxidation which takes place in the body. Sulphuric acid and water given in this case (No. 1), in which the patient was rapidly sinking for the want of oxygen, that acid was decomposed—deoxidized—and its oxygen appropriated, *ex necessitate rei*, such was the vehement eagerness of the tissues for oxygen; and since contemporaneous chemistry fails to explain the process by which the tissues obtain oxygen in this case, we may say with Letourneau that they "steal" it. Indeed, chemistry teaches, in one of its first lessons, that the oxygen of this acid is some of the same oxygen that we breathe, taken from the air to make that acid; and according to the doctrine of the correlation and conservation of forces, if that oxygen be liberated from its sulphur compound, and properly diluted with nitrogen gas, it can be again utilized as respirable air.

Impressed with the views that the tissues in this case (No. 1) were famishing for want of oxygen, that they called even from the depths of the stomach for it; that sulphuric acid abounding in oxygen possessed great affinity for water, and when that affinity was gratified it would yield its oxygen, preferably to waste organic tissue; that sulphur, in dilute sulphuric acid, was excreted primarily by the glands of the intestinal tract; that oxygen thus disengaged in the walls of the stomach, bowels, and uterus, in these maladies, was immediately precipitated toward the carbon and hydrogen compounds as in the combustion of tissue respiration; that affinity of the red corpuscles for oxygen was very great, and that oxygen reached the anatomical elements through the intermedium of those corpuscles only; that it was oxygen that produced motion in the blood, removed waste and made repair possible; that water was used in the system as a medium of irrigation, histological transportation and molecular division; I gave water and sulphuric acid, for the purpose of producing what may be called aquatic, or more properly supplemental and intestinal, respiration, as the only means of relief. This acid, which had long been used in empirical practice, as an astringent, and was known to be absorbable, as well as most diffusible, was given to produce this hitherto

Digitized by Google

unknown effect in theoretical and practical medicine. How it succeeded, I have already stated.

To those who have not accepted the beautiful theory of the capillary circulation, as given by Becquerel, and who give no heed to clinical evidence, I present another interpretation of the action of this acid, in the diseases of which I have spoken.

Many physiologists, for many cogent reasons, have assumed the existence and storage in the body of the intra-molecular oxygen. The electro-chemical couplets present in all of the tissues, as illustrated by Becquerel, and incessantly at work in hæmatosis, may fail to functionate in these diseases, and dilute sulphuric acid by chemical transformation, or by catalysis if you please, may evoke this intra-molecular oxygen thus stored up, which, acting as an oxidant, enables the system to utilize the energy arising therefrom in driving the heavy column of venous blood that inhibits nutritive action, until the physiological function of respiration shall have been restored.

To the doubting Thomases, the tenth legion of the medical profession—and I speak in no disparagement of them—those who go to the kennel and laboratory to prove all things, I will say that I, too, have gone thither. The experiments which I there made, however, are too incomplete to form a part of this paper. It is necessary in experimentation to start with certain pathological conditions, associated with cholera and post-partum hemorrhages, always keeping in view the attendant respiratory and circulatory disturbances. This action under consideration cannot be effected in the physiological state; and an artificial condition, simulating in all characteristics one of disease, is not easily, if at all, attainable, at least in the canine race.

Arguments from comparative anatomy, as I have before stated, sustain the theory of supplemental and intestinal respiration. The water loach (cobitis fossilis), a singular fish, swallows air by the mouth, and, after having absorbed a part of the oxygen, gives off carbonic acid by the intestines. Clinical experimentation also supports this theory. A plethoric man, thirty-nine years of age, who complained of fullness in his head, and feared apoplexy, was given an enema to unload his rectum, which was followed by a hydragogue cathartic and a foot-bath. His temperature was 103.2° F., and his pulse was 132. He had been afflicted with hæmorrhoidal tumors for five or six years; indicating that there was more or less general venous congestion, commencing in the portal circulation. After the action of the cathartic ceased, at his own request he was bled from the arm, bled until he was quite faint, taking eighteen ounces of blood. He was then given thirty drops of aromatic sulphuric acid in twelve ounces of water, sucked through a glass tube, suitable preparations having been made previously to collect the gaseous emanations from his intestines, and have them condensed in a flask of lime water. A white pulverulent deposit was seen afterward in the flask, which was fetched home, and the supernatant water decanted. The white deposit was treated with acetic acid, which was followed by effervescence, indicating the presence of carbonate of lime; there was also the characteristic odor of hydrogen sulphide. This result proves that the gas that emanated from the intestines was carbonic acid; this substantiating the theory of supplemental and intestinal respiration, in certain diseases and conditions in man. Under additional treatment and a sustaining regimen, the patient made a rapid recovery, and is now in good health.

The failure of the lungs and blood to functionate, and the successful resort to the administration of sulphuric acid and water, as shown in this paper, to irrigate the tissues and keep up respiration through deoxidation of this acid by the hæmoglobin of the blood, and cession of the oxygen to the anatomical elements until the physiological function proper was reëstablished in its original vigor, as a necessary and indispensable antecedent, is identical with a cause, being that without which the effects would not take place. Moreover, logicians call a phenomena explained, when by illustration it has been discovered to what general classification of natural laws it can be assigned.

Digitized by Google

If the length of this paper permitted, it might be stated how nitric and phosphoric acids in certain other diseases, act also in accordance with this hypothesis; one in cirrhosis of the liver attended with dropsy from alcoholism, the other in nervous exaltation from sunstroke with inability to appear in the sunlight. In practice, these acids also have acted with great promptness, manifestly by yielding oxygen, in the first place, through the hæmoglobin of the blood to the tissues of the liver; in the second place, by the same means to the nervous tissues.

Physiological chemistry has yet to speak on this subject; let her hearken to the clinical necessities.

At a meeting of the Council, Dr. Phillips of London in the chair, the following resolution was unanimously adopted:—

"*Resolved*, That the officers and members of the Section in Therapeutics and Materia Medica tender their cordial acknowledgment and thanks to the President, Prof. Traill Green, M. D., of Easton, Pa., for the dignity and urbanity he has manifested in presiding over the deliberations of this Section; and for the faithful manner in which he has performed his arduous duties, both before and since the assembling of this Congress, and by which he has contributed materially to its signal and unprecedented success."

FRANK WOODBURY, M. D.,
Secretary.

A. S. GUBB,
French Secretary.

Digitized by Google

Digitized by Google

SECTION VII—ANATOMY.

OFFICERS.

PRESIDENT: WM. H. PANCOAST, M. D., Philadelphia, Pa.

VICE-PRESIDENTS.

Prof. C. M. P. ALBRECHT, Hamburg, Germany.
Dr. R. E. F. ALTMANN, Leipzig, Germany.
Prof. L. AUERBACH, Breslau, Germany.
Dr. WM. MITCHELL BANKS, F. R. C. S., Liverpool, England.
Prof. CARL BARDELEBEN, Jena, Germany.
Dr. BARFURTH, Bonn, Germany.
Dr. L. BECKER, Würzburg, Germany.
Prof. R. BONNET, Munich, Germany.
Prof. G. BORN, Breslau, Germany.
Prof. C. W. BRAUNE, Leipzig, Germany.
Prof. A. VON BRUNN, Rostock, Germany.
Prof. L. T. BUDGE, Greifswald, Germany.
Dr. J. CUNNINGHAM, Dublin, Ireland.
Prof. J. EBERTH, Halle, Germany.
Prof. F. MERKEL, Göttingen, Germany.
Prof. ECKHARD, Giessen, Germany.
Prof. FLEMMING, Kiel, Germany.
Prof. FLESCH, Berne, Switzerland.
Prof. FRORIEP, Tübingen, Germany.
Prof. FRITSCHE, Berlin, Germany.
Prof. FREY, Zürich, Switzerland.
Prof. FROMMANN, Jena, Germany.
Prof. GEGENBAUR, Heidelberg, Germany.
Prof. VON LA VALETTE ST. GEORGE, Bonn, Germany.
Prof. VON GERLACH, Erlangen, Germany.
Prof. L. GERLACH, Erlangen, Germany.
Prof. JNO. GLEELAND.
Prof. KOLLMANN, Basel, Switzerland.
Dr. REGINALD HARRISON, F.R.C.S., Liverpool, Eng.
Dr. C. E. A. HARTMANN, Berlin, Germany.
Prof. HASSE, Breslau, Germany.
Prof. HENKE, Tübingen, Germany.
Prof. HERTWIG, Jena, Germany.
Prof. HIS, Leipzig, Germany.
Prof. HOLL, Innsbruck, Austria.
Prof. JOESSEL, Strassburg, Germany.
CLINTON W. KELLY, M. D., Louisville, Ky.
Prof. KERKER, Freiberg, Switzerland.
Prof. KÖLLIKER, Würzburg, Germany.
Prof. KOLLMANN, Basel, Switzerland.
Prof. KUPFFER, Munich, Germany.
Prof. VON LANGER, Vienna, Austria.
SAMUEL LOGAN, M. D., New Orleans, La.
Prof. ALEX. MCALLISTER, London, Eng.
Prof. JOHN GRAY MCKENDRICK, Glasgow, Scot.
Prof. S. MAYER, Prague, Austria.
Prof. VON MEYER, Zürich, Switzerland.
Prof. M. NUSSBAUM, Bonn, Germany.
Prof. OELLACHER, Innsbruck, Austria.
DAVID CHAS. LLOYD OWEN, Birmingham, Eng.
Prof. PANSCH, Kiel, Germany.
Prof. JAS. BELL PETTIGREW, Edinburgh.
Prof. PFITZNER, Strassburg, Germany.
Prof. RABL, Prague, Austria.
Prof. RAUBER, Dorpat, Russia.
THOMAS RICH, M. D., Leeds, England.
DALLA ROSA, M. D., Vienna, Austria.
Prof. E. ROSENBERG, Dorpat, Russia.
Dr. RÜCKERT, Munich, Germany.
Prof. H. RABL-RÜCKHARD, Berlin, Germany.
Prof. RÜDINGER, Munich, Germany.
Prof. RUGE, Heidelberg, Germany.
Prof. PHILE SAPPEY, M. D., Paris, France.
Prof. SCHAAFFHAUSEN, Bonn, Germany.
Prof. SCHWALBE, Strassburg, Germany.
Prof. SCHIFFERDECKER, Göttingen, Germany.
Prof. O. SCHULTZE, Würzburg, Germany.
NOBLE SMITH, M. D., London, England.
Prof. SOLGER, Greifswald, Germany.
Prof. SOMMER, Greifswald, Germany.
Dr. GRAF VON SPEE, Kiel, Germany.
Prof. E. C. SPITZKA, M. D., New York.
Prof. STIEDA, Königsberg, Germany.
Prof. PH. STÖHR, Würzburg, Germany.
Dr. STRAHL, Marburg, Germany.
Prof. STRASSER, Freiburg, Germany.
Prof. M. STRUTHERS, M. D., Aberdeen, Scot.
THOS. P. TEALE, Leeds, Eng.
Prof. TEICHMANN, Krakau, Russia.
BENJAMIN H. THROOP, M. D., Scranton, Pa.
Prof. TRUBIGER, Amsterdam, Holland.
Sir WM. TURNER, M. D., London, Eng.
Prof. WAGENER, Marburg, Germany.
Prof. WALDEYER, Berlin, Germany.
Prof. WIEDERSHEIM, Freiburg, Germany.
Prof. WELCKER, Halle, Germany.
WALTER WHITEHEAD, M. D., London, England.
GLADIUS GALEN WHEELHOUSE, Leeds, England.
Dr. ZANDER, Königsberg, Germany.
Prof. ZUCKERKANDL, Vienna, Austria.

SECRETARIES.

Dr. JNO. J. BERRY, Portsmouth, N. H.
Dr. GALLET, Brussels, Belgium.
Dr. C. REMY, Paris, France.
Dr. WM. C. WILE, Philadelphia, Pa.

COUNCIL.

Dr. F. C. Ainsworth, U. S. Army.
Dr. C. S. Briggs, Nashville, Tenn.
Dr. Herman M. Briggs, New York.
Dr. N. E. Brill, New York City.
Joseph N. Dickson, M. D., Pittsburgh, Pa.
Fred'k B. Downs, M. D., Bridgeport, Conn.
Dr. L. H. Dunning, South Bend, Ind.
C. L. Ford, M. D., Stamping Ground, Mich.
Dr. Alfred Gubb, London, England.
Dr. George Halley, Kansas City, Mo.
Dr. William H. Helm, Sing Sing, N. Y.
Dr. Addinell Hewson, Philadelphia, Pa.
Prof. C. L. Lord, Michigan.
Dr. James McCann, Pittsburgh, Pa.
Dr. Orville H. Menees, Nashville, Tenn.
Dr. John E. Owens, Chicago, Ill.
James B. Murdoch, M. D., Pittsburgh, Pa.
Dr. Thornton Parker, Newport, R. I.
Prof. Charles T. Parks, M. D., Chicago, Ill.
Dr. D. C. Patterson, Washington, D. C.
Dr. Joseph Ransohoff, Cincinnati, Ohio.
Dr. R. L. Rea, Chicago, Ill.
F. C. Schaefer, M. D., Chicago, Ill.
Dr. Francis J. Shepherd, Montreal, Canada.
Dr. M. Stamm, Fremont, Ohio.
Geo. Thin, M. D., London, England.
Dr. Hal. C. Wyman, Detroit, Mich.

Digitized by Google

FIRST DAY.

The Section met in the Georgetown Medical College, and was called to order by the President, who read the following—

ADDRESS BY THE PRESIDENT, WM. H. PANCOAST, A. M., M. D.,

Professor of General, Descriptive and Surgical Anatomy, and Professor of Clinical Surgery, in the Medico-Chirurgical College of Philadelphia, etc.

GENTLEMEN OF THE SECTION OF ANATOMY:—As your President, I take great pleasure in welcoming you. It is very pleasant to see, in this the first actual International Medical Congress ever held in this country, the first time that the Congress has officially adjourned to this new continent, so many gentlemen of our profession, not only from abroad, but so many of my fellow-countrymen.

We are a Congress, composed of delegates from the countries of the old world and this new one; it is very pleasant to see so many fellow-workers in our profession, who have thus assembled from so many climes, in the interest of humanity, to advance the various departments of our noble science, for the benefit of all peoples.

I cordially unite with our President, and my fellow-presidents of the different Sections, in warmly welcoming you all, each and every one. I trust that our American hospitality will prove acceptable to you, and that you will make as long a stay with us as you possibly can.

I know that I am speaking the sentiment, not only of the American profession, but of all my countrymen, when I say to our foreign brethren that they are doubly welcome, and that we will do all in our power to make their visit pleasant and instructive.

In this, our Section of Anatomy, we have many interesting papers promised, and many of them will have a practical value, which will make them of general interest.

Anatomy is recognized as being the foundation of our medical science, and is in direct connection with all its subdivisions.

How can one mend, or manipulate properly, the living human mechanism, who does not understand that machinery? It is a living, daily useful knowledge.

I have often, in my lectures, as Professor of Anatomy, both in the Medico-Chirurgical College and in the Jefferson Medical College, in Philadelphia, stated that I taught a "live anatomy from the dead body," and believe that I have introduced the phrase.

I have always impressed the student that anatomy was of essential knowledge in the practice of medicine. That he should study anatomy on the dead body, so as to vitalize the knowledge so acquired upon the living; to practically apply it to his own living structures. That anatomy is not a dead science, nor a mere closet study, but one of living, essential value.

Not to be learned as a mere task, but so that it may be remembered, and applied

Digitized by Google

in daily use. Hence my phrase that I "teach a live anatomy from the dead body." Anatomy, so mastered, remains a basis of active, real knowledge. Physicians who have so studied it, are not in after years as prone to say that their anatomy has grown rusty.

The great surgeon, Sir Benjamin Brodie, once said to me, "If you wish to be a surgeon, teach anatomy and practice surgery."

It would be well if this advice was more generally followed.

Anatomy for practical use is almost a complete science, but great work is being now done in minute anatomy.

Any advance in anatomical discovery should be regarded as an event by our whole profession. Anatomy has been well called "the key and guide of medicine."

Prof. Leyden,* in his opening address to the Congress at Wiesbaden, in April of this year, well said: "What raises medicine above all the natural sciences, is the fact that its object is man himself."

The most interesting and practical advance in anatomical knowledge in the last few years, I think we will all agree, has been in the study of the brain. The researches of Flourens ("Recherches Experimentales sur les Proprietes et les Fonctions du Systeme Nevreux," 2d edition, 1842; Longuet, "Traite de Physiologie," 2d edition, 1866) gives us our chief knowledge of the results of the cutting away of the cerebral hemispheres, aided by other workers chiefly among the French, as, notably Vulpian, in his "Leçons sur la Physiologie du Systeme Nevreux," 1866.

By these experiments upon animals, cutting off or destroying different portions of the brain, much clinical knowledge has been obtained; but such investigations must always be more or less complicated with the question of shock to the whole brain structure.

Anatomical and physiological researches must always go hand in hand; are complementary to each other. The anatomical investigations of Lockhart Clarke, Meynert, and others, with those of physiologists, such as Brown-Séquard and Schiff, have advanced materially our knowledge of the nervous system and its centres. Fritsch and Flitzig have made a further advance in brain study by their discovery of the so-called electric excitability of the brain. These investigations have been brought to a practical focus by the clinical and pathological work of Dr. Hughlings Jackson, the physiological work of Ferrier and the surgical application made by Mr. Victor Horsley, at the Hospital for Epileptics, Queen Ann's Square, London. Mr. Horsley does some admirable surgical work. I saw him operate, with antiseptic precautions, for the removal of a cicatrix of the membranes of the brain, resulting from a fracture of the skull, in the upper parietal region, removing the cicatrix and the portion of the brain substance to which it was attached. I also saw two epileptics on whom he had operated. Diagnosing the tumor and its position, in each case, from the peripheral symptoms. In both he removed the tumor and a large piece of the substance of the brain; if I remember correctly, nearly two inches square, and one inch of thickness of brain tissue.

These cases were afterwards shown, at the meeting of the British Medical Association at Brighton, by Mr. Horsley, where I again saw them. They excited great interest in the crowded Section.

In a recent murder case in Mount Holly, New Jersey, I diagnosed the location of the pistol ball in the brain from the injury to the lip, tongue, and speech centres, in front of the fissure of Sylvius; the opposite side of the body being affected with

* *Report* of Fiftieth Congress of German Physicians at Wiesbaden.

Digitized by Google

paralysis. I trephined alongside the wound of entrance, about an inch in front of and above the left ear, letting out pus and remarkably relieving my patient, Miss Anderson. The paralysis almost entirely disappeared; my patient regaining consciousness and speech. She sank again, and by the use of the induction balance of Dr. Girdner, used by him, thanks to his generous courtesy, and with the aid of the gentlemen engaged in the consultation, I was induced, as a last chance, to trephine on the opposite side obliquely backward, behind the parietal boss, opening an abscess resulting from the injury of the ball. I believe I touched the ball, with a Nélaton's probe, in this abscess, for the porcelain became leaded, showed the marks of lead upon it. I would have removed the ball, but for the exhausted condition of my patient, compelling me to stop instantly.

I established, however, excellent drainage for the brain pus, and gave the patient another chance for life. The ball could have gravitated near this last opening. My surgical interference certainly prolonged Miss Anderson's life about four weeks, and as an important fact, when she regained consciousness, in the most distinct manner, she stated who shot her, and where. On the post-mortem, the tract of the ball was found as we had diagnosed. The ball had traversed the brain obliquely backward, piercing the Falx major cerebri, in which it had left a hole. In the preparations by the undertaker, the body was disturbed and shaken, contrary to my directions. The ball had consequently gravitated through the slushy, decomposed brain matter, and was found at the base, just behind and below the first clinoid processes of the sphenoid, resting upon the membranes.

I have used with advantage, as other surgeons have, the new topographical and physiological knowledge of the brain in cases when it has been injured.

There are some anatomical points I would like to bring to your notice. I do not think that anatomists make sufficiently distinct that all muscles arising from one bone, and passing over the articulation to the bone below, are articular muscles. Hence, if any one of these is paralyzed, the articular surfaces separate, as far as the white, fibrous ligaments around them will permit. This is notably shown in the paralysis of the deltoid, making a simulated luxation. The white, fibrous ligaments around the joint, while they have some representative fibres of elastic tissue, are not made to stretch, and do not stretch in health. They are made to hold the opposing articular surfaces a certain distance apart, so as to permit motion by the muscles, and to prevent too wide a separation of the articular surfaces. The definition of a movable joint, as I teach, is that normal solution or division of the skeleton, to deaden shock and permit motion; as in all the diarthrodial joints. The ligaments are made flexible to permit the motion, and strong, inelastic, so as not to stretch and allow the articulations to separate but a certain distance. As the muscles contract intermittingly and so variously, if the ligaments did not strongly hold the bones of the movable articulations together, we could not stand erect. The enlargement of the ligaments that we see in pathological conditions of the diarthrodial joints, as hydrops articuli, white swelling or synovitis, is not immediate, as it would be if the ligaments were elastic, and stretched immediately, but is a slow, progressive enlargement or expansion, under the maceration of the tissue, during the inflammation; and the reduction is equally slow and tedious, often requiring the alterative impression of the iron at a white heat. Like a sponge which swells when soaked with water, and becomes smaller when the water is removed. It does not stretch, it swells and shrinks. Surgeons make anatomical mistakes, when they say that the non-elastic ligaments stretch. When stretching or elasticity is needed, then we have the preponderance of yellow, elastic tissue, and we call this tissue yellow,

Digitized by Google

elastic, fibrous tissue. How could a man stand on his toes if the tendo-Achillis stretched?

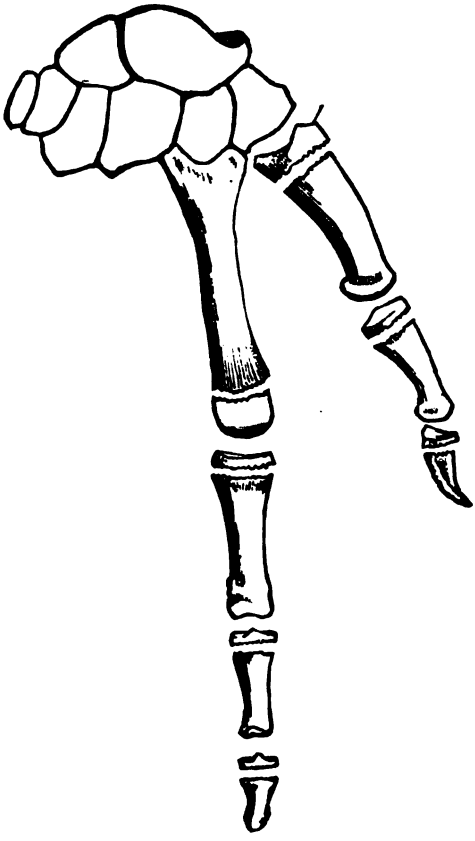
In regard to movable joints, again, while as histologists we recognize the low grade of inflammation that the articular cartilages are liable to, with the accompanying infiltration into their lymph spaces; in joint inflammations, as coxalgia, white swelling, it is the synovial membrane which, when inflamed, does the mischief, and gives the agonizing pain. The articular cartilages slowly macerate, become opaque and dirty colored, and can be scraped off with a groove director without giving pain; their organization is so low. This I have tested in two cases of knee-joint amputation, where I could scrape off the articular cartilage without the patient knowing it. Beneath this macerated cartilage was red, injected and inflamed bone. The synovial membrane is so highly organized, that inflammation of it causes agonizing pain, and the sufferer instinctively holds the joint steady by his muscles. When inflamed, it becomes vascular and loses its polished, transparent appearance. In health we never find it between the articular surfaces, because it is too highly organized to bear the pressure. Admitting Toynbee's statement, that in the early stages of fœtal life it is a closed sac between the joints, and then covers the articular surfaces when motion of the joints commences, then the intervening synovial membrane is absorbed, for we never find it on the articular cartilage in the new-born. In inflammation of joints lined by synovial membrane, it is this membrane that gives the pain and causes the destruction of the joint. If this inflammation is not arrested, then nature makes her own cure, which is anchylosis. The synovial membrane grows and spreads over the articular cartilage, which has now become macerated; sucks it up in its lymphatics, and leaves bare bone against bare bone, vascular and inflamed, when blood vessels throw out callus and solidify the joint. Using the finest ether and vermilion injections, I have never been able to inject the articular cartilages in the adult, new-born cadavera, or in fœtal calves. I have dissected several joints amputated as a result of synovitis, and have found this the invariable condition. The anatomical teaching has great practical value in surgery. Many who use extension and counter-extension in joints, think that they are doing good, by forcibly separating the joint surfaces, and taking off pressure from the articular surfaces. I have been called in consultation frequently, where I found that the extension and counter-extension was causing pain, unrest, and doing harm. When it is recognized that it is the synovial membrane that must be treated, and not irritated, and not the articular cartilage, then the importance of rest is recognized, and the extension and counter-extension are used judiciously and only for rest. That is the requisite for curing joint inflammation. Rest, so as not to increase the synovitis; sufficient fixation to prevent the inflamed synovial membrane from being rubbed against, and then the proper lotions, or counter-irritants, aided by constitutional treatment, to cure the inflammation. Extension and counter-extension are often abused.

I would like to bring to your notice that I believe there are only four metacarpal bones in each hand and foot. I do not think the anatomical teaching is correct, that says there are five metacarpal bones in each. A metacarpal bone is developed by two centres—one for the shaft and one for the head; a phalanx, one for the shaft and one for its base. The thumb has, therefore, by this explanation of development, three phalanges. The first, or upper phalanx, is one, and is not a metacarpal bone, by anatomical definition. It is the same way in the foot; hence, the great mobility of the big toes, as well as the thumbs, and the facility with which some people, born without hands, can use their feet. In looking at the hand, it is made up of two solid triangles, each having for its base line of junction, one drawn from the index

Digitized by Google

finger to each pisiform bone. As a practical deduction from this, I have always found it necessary to use a different treatment for fractures and dislocations involving the metacarpus and the thumb. Again, the metacarpus can be all removed, with its adjacent phalanges, in cases of injury, and, if the thumb is intact, it can be moved over, and its so-called metacarpal bone, or first phalanx, takes position against the carpal row. This I first saw done by my father, when the result was a useful and active *thumb at the end of the stump* on the carpus. I have also had the same good result in a like amputation.

Among the anomalies of muscles there is an interesting and important one, in



which I have several times found an unequal development, and hence contraction of the soleus and gastrocnemius muscle. That is, the soleus will be hard and tense, while the gastrocnemius will be too long apparently, and will be loose and movable; can be moved to and fro on the soleus beneath. This was first pointed out by my father, and he explained, by this, how certain cases of talipes equinus were not benefited by cutting the tendo-Achillis. After the tendon united, the same deformity recurred, because the gastrocnemius and soleus being inserted into the tendo-Achillis, the same tenseness of the soleus and relaxation of the superficial gastrocnemius occurred. This he met practically by slipping a knife between the gastrocnemius

Digitized by Google

and soleus and cutting the soleus loose, by shaving it off flatwise from the tendo-Achillis. This operation I have repeated several times, and have made a long, sabre-shaped myotome to do it with facility. A simple puncture is made, and I then push the myotome flatwise between the gastrocnemius and soleus, and shave off the latter in the same way. By keeping your knife flat, cutting in this way, you are in no danger of injuring the deeper-lying tibial artery, and the hemorrhage is slight and easily controlled. I think an additional teaching might be emphasized with regard to some of the deep fascias, as of the hand and foot. They not only protect the deeper structures, but they act in some places as cords or ligatures. This I have noticed in deep cicatrices and club deformities of hand and foot. As an illustration, in talipes varus or valgus, the pathology, I think, is that the plantar ligaments, long and short, are not sufficiently developed, and, from the same cause, or as a consequence, the deep or plantar fascia is not long enough. Hence, when the bones grow they are restrained or compressed on the undeveloped side, and become proportionately large on the opposite—the convex side. Hence, in operations for talipes varus, or valgus, I always cut subcutaneously the deep fascia, to lessen its tenseness. Again, if it is an old deformity, the bones have been forced to grow in the manner I have just mentioned. In such cases I have found I could not correct the deformity without cutting out the projecting bones on the outside of the foot. This I have done, producing the best results.

In regard to the coronoid process of the ulna, there is a point that seems worth mentioning. Surgeons have often spoken of the fracture of this process as the result of muscular contraction. This, I think, is partly the fault of an anatomical teaching and works on anatomy, because it is not sufficiently emphasized that the coronoid process is covered by the anterior ligament of the elbow, so that its summit is always within the ligamentous covering of the joint. The projecting eminence which this process makes can thus never be broken off by the contraction of the brachialis anticus. This muscle is inserted at the base of the process, into a rough depression on its anterior surface. As it is thus inserted into the anterior surface of the very base it cannot pull off the upper portion of the process. The tendon of insertion lies entirely outside of the ligaments surrounding the elbow joint, on the surface of the anterior ligament, while the free articular tip of the process is within the joint, covered completely by the ligaments, and grasping by its concavity the anterior part of the trochlear surface of the lower articular end of the humerus. I don't believe that the coronoid process is ever broken off by muscular contraction. I have seen this fracture occur very often in injuries to the elbow joint, and always from direct violence, such as a fall. In falling, the arm being outstretched for protection, the violence is applied to the open palm or to the wrist, and thus the forearm is driven so forcibly up against the humerus that the coronoid process is broken off in the joint. I believe that this very often happens, and is not recognized, on account of the subsequent and almost immediate swelling. As a consequence, the joint is not treated so as to place this fractured process in the best position for a cure. It may get between the articular surfaces, or in such a position as to inflame the synovial membrane, and thus produce, as we often see, a permanent deformity of the joint, impairing its usefulness.

Among some smaller points of interest, that time will not permit me to refer to, I would like to mention to you anatomists how constantly misused is the term anastomosis, when applied to the description of the nerves, by surgeons and other scientists; even by anatomists.

The derivation of anastomosis is from ana, stoma and osis. Meaning, the appli-

Digitized by Google

cation of one mouth to another mouth, for the purpose of facilitating the circulation of a fluid. This term is well adapted to the vascular apparatus, where one vessel connects with its open mouth to the mouth of another, as is so well shown in the circle of Willis at the base of the brain. But it is not applicable to the nervous system. We know that the nerves are cords, and that the nervous fluid circulates along these cords, delicate as so many of them are, in a manner similar to the passage of the electric current along a wire. The nerves are continuous cords, from their origin to their termination. One nerve may join, side by side, with another, and both run to their destination, but in no case do we find them to inosculate, or join end to end with open mouths. Nerves do not anastomose, and, according to my judgment, it is a great misnomer to so state. To say so is an anatomical mistake, as precision is a most important point in anatomical teaching. By accurate teaching and nomenclature we prepare the way for the subsequent elucidation of physiological truths.

In reporting to you what has recently been done for the advancement of our department of medical knowledge, it may be well to mention the inauguration of the Anatomical Institute of Vienna, within the past twelve months.* Although but one division, the erection of several buildings for allied sciences being contemplated, it is admirable in its appointments. It has two lecture-rooms, each with a seating capacity of 300. Rich in museums and material; a most complete library of anatomical work. Working rooms for professors and students. The dissecting rooms are two grand halls furnished with marble tables, electric lights, marble floors and water appliances that stand unrivaled. The whole building can be flushed with water, and thus be guarded against miasmatic influences.

In reviewing the practical work that has lately been accomplished, we must bear in mind that but little can be added to the details of human anatomy, except in histology; but much remains to be done in the way of rational explanation and elucidation. While the material at hand for the purpose is inexhaustible, the labors we have to record, although by no means small, make but little impression upon it.

Upon the researches in regard to the osseous system, I will first record to you the result of the investigations of Mr. J. Bland Sutton.† He has found that the ossification of the superior maxilla in the mammalia proceeds from four centres, of which the originally separate parts are:—

(1) The *premaxilla:* the innermost part of this is of cartilaginous origin; being formed in the anterior extremity of the ethmo-vomerian plate. If anything interfere to prevent the premaxillary bone from meeting the ethmo-vomerian plate, the tip of this cartilage ossifies independently, and the double premaxillary bones, described by Albrecht in cases of cleft palate and hare lip, are thus explained.

(2) The *prepalatine* portion, comprising the palate plate and a considerable portion of the inner wall of the antrum.

(3) The *maxillary* centre, including all the facial and orbital parts of the bone, internal to the infra-orbital canal.

(4) The *malar* piece, situated external to the infra-orbital canal.

He has also found that in the early life of the human fœtus, a cartilaginous bar is given off from the upper end of Meckel's cartilage, subsequently the malleus, to

* Reports in many German Journals, for 1886.

† *Proceedings* of the Zoölogical Society of London, 1886; *Journal of American Medical Sciences*, 1886.

Digitized by Google

near the extremity of the fronto-nasal cartilage, and which undergoes the following metamorphosis:—

(*a*) The anterior portion ossifies as the pterygoid bone, the internal pterygoid plate of human anatomy.

(*b*) The middle part remains throughout life as the cartilaginous portion of the Eustachian tube.

(*c*) The posterior extremity degenerates into fibrous tissue, and becomes the anterior ligament of the malleus. The number of centres given by him agree with other descriptions, but there exists some difference in the dates of their appearance and junction.

Mr. W. Kitchen Parker* has made the structure and development of the skull a special study, and during the past year has published two monographs upon the subject, one upon the skull of the edentata, and the other upon the skull of the insectivora. Another contribution is that of Mr. Herman Welcker, "The Capacity of the Encephalon in the various Nations." Although of much smaller scope than the former monographs, we may refer to that series which are published by my friend, Prof. Macalister,† of Cambridge University, England, upon the cranial bones. It is to be regretted that they are so little available to the ordinary student.

Dr. Benedict.‡ of Vienna, has completed his instruments for the craniometric and cephalometric method. It is unquestionable that, by this method, much will be accomplished toward bringing anatomy to a fixed science. Such instruments as these are of the utmost value in the study of our department of the medical science. I firmly agree with Benedict when he says: "If anatomy shall become a fixed science, it can none too soon be reduced to mathematical morphology."

The axial skeleton has had its attention at the hands of Prof. Cunningham,§ who has discussed embryologically the connection of the os odontoidum with the body of the axis vertebræ.

Turner‖ has shown that a difference exists in the spinal curvature in the skeletons of the various races, which if not compensated for in the living by an equivalent in the intervertebral discs the appearance would be marked, as the lumbar spine is less curved in the Negroes, Andamanese, and especially Australians. To express the relation of the anterior and posterior depths, a lumbar index is calculated, the anterior vertical diameter being taken as a standard—100. The average index of the five lumbar vertebræ in 12 Europeans is 95; in 2 Esquimaux, 98; in 3 Negroes, 98.9; in 2 Sandwich Islanders, 104.7; in 5 Australians, 105.8. The lowest was 84.8 in a Chinese skeleton, and the highest 106 in a Bushman.

Prof. Turner¶ has given further data to the ethnological classification which uses the pelvic brim as a basis, and proposed one similar to that in use for the various forms of the cranium. The base of operation was in all cases the male pelvis, as the female basin showed a greater similarity of form in the different races.

In the researches which have the brain and nervous system and its development as a basis, we first refer to the work that great student, Herr His,** has contributed

---

* *British Medical Journal*, 1886. † *Transactions* of the Royal Irish Academy, for 1886.

‡ *Wiener Medicinische Blätter*, 1887. § *British Medical Journal*, 1886.

‖ *Journal of Anatomy and Physiology*, April, 1886.

¶ *Journal of Anatomy and Physiology*, 1885.

** Report of the Fifty-ninth Congress of German Scientists, Section of Anatomy—*Archiv für Anatomie*, 1886.

Digitized by Google

to the origin and development of nerve fibres. He has demonstrated that after the closure of the spinal canal an apposition forms between an inner and dense, and outer and somewhat less compact layer of cells (the inner plate and the enveloping layer). From the cells of the former a superstructure is developed, the myelo-spongium, which with its outer covering tops the nucleated cell tubes, and delivers the foundation for the future white fibres of the spinal cord. The development of nerve fibres in the human embryo is begun with the fourth week. The cells of the enveloping layer develop into a primary conical process, a prolongation of the axis cylinder, and which from its incipiency shows fibrillar markings. The fibres developed from the anterior half of the enveloping layer leave the spinal cord as motor roots. The posterior half are given off in a saggital manner, and develop circularly toward the anterior (formatio arcuta). A part of these fibres go over to the anterior commissure, which, in the beginning, contains but few strands; coincidently with the latter longitudinal fibres appear which are the commencement of the anterior columns.

Branched sprouts appear from the enveloping layer of cells, the anterior motor horn cells, later, as the prolongations of the axis cylinder.

The foundation for the ganglions are divided and become completely separated from the spinal marrow, their cells spread and originate two roots, one of which enters the spinal cord as the posterior, the other develops toward the periphery. The nucleus of the spinal ganglia moves eccentrically to the side, and thus allows the building of the "T" formed fibres. These forms are readily recognizable in an embryo of four or five weeks, as the cells do not yet possess the endothelial division. The root fibres encroaching on the spinal marrow collect in thin, and later on, in heavier oval bundles; subsequent fibres can pierce these bundles and make their way between the cells. The peripheral branches burrow their way through the loose connective tissue, and in the beginning have a relatively enormous power. The central fibres find their way out in the meshes of the myelo-spongium. The primary conduct is in every case a free development of the divided and undivided fibres; just how far secondary combinations can begin between the cells, centrally as well as at the periphery must, for the present, remain an open question. He positively asserts that motor fibres always appear before the sensory.

Prof. W. Bechterew * has pointed out a region of the lateral column of the spinal cord, immediately in front of the posterior nerve roots, between these and the crossed pyramidal tract. The fibres differ from those of the neighboring parts by their small size; they begin to acquire their medullary sheath when the fœtus is about thirty-three centimetres in length, at a time when the posterior columns are completely medullated, and while the pyramidal tracts still consist entirely of pale fibres. The name *posterior root-area of the lateral column* is given to this region, since it is composed of the small fibres of the posterior roots, which ascend for a short distance at the outer part of the substantia gelatinosa, and then turn inward to the posterior horn. Bechterew's observations on the fœtal cord confirm Lessauer's, who found this area degenerated in tabes dorsalis. The fibres of the ascending root of the fifth nerve are medullated in fœtuses from twenty-five to twenty-eight centimetres long. They can be seen arising from the lower part of the medulla oblongata, from the cells of the base of the posterior horn, being placed internal and anterior to the gelatinous substance of Rolando. They pass outward through the latter to gain its outward surface, along which they ascend. They have no origin from the substantia gelatinosa.

---

* *Archiv für Anatomie*, 1886.

Digitized by Google

The same observer, in his study of the development of the restiform body, from various periods of fœtal life, finds it is made up of five constituents, which are enumerated in the order of acquiring their medullary sheath.*

1. The direct cerebellar tract of the cord.
2. Fibres sprouting from the nucleus of funiculi cunextus of the same side.
3. Fibres springing from the lateral nucleus of the same side.
4. Fibres issuing from the nuclei of the funiculi graciles of both sides. The set derived from the nucleus of the same side ascend to the restiform body as the posterior superficial arcuate fibres; while those from the opposite nucleus pass forward in the inter-olivary layer and emerge on the inner side of and through the pyramid, as well as between the pyramid and the olive, and from the anterior superficial arcuate fibres.
5. Fibres from the lower olive of the opposite side. Neither the formatio reticularis nor the pyramid of the medulla oblongata has any relation to the restiform body; nor does the latter receive any fibres from the fifth and eighth nerves, as Edinger states. The auditory nerve is connected to the cerebellum by a special bundle, which ascends to the middle peduncle. The fifth nerve has no connection with the cerebellum. The restiform body is distributed in three distinct bundles within the cerebellum. 1st, containing fibres from the direct cerebellar tract of the cord, from the nucleus of the funiculus cunextus, and from the lateral nucleus passes to the cortex of the forepart of the upper worm; 2d, composed of the fibres from the nuclei of the funiculi graciles to the middle portion of the upper worm on the same side; and, 3d, comprising the fibres from the opposite olive, to the gray substance of the corpus dentatum, and in part, perhaps, to the cortex of the hemisphere.

Baginsky† has devised a unique operation by which he has demonstrated the origin and central course of the auditory portion of the eighth nerve. The resulting degenerative changes affected only the posterior root of the nerve. The anterior root in all cases remained unchanged. It was thus shown that in the posterior root no connection existed with the inner and outer auditory nuclei, but that it sprang, in part, from the accessory auditory nucleus and the tuberculum laterale of Stieda, and in part to course round the restiform body into the arcuate fibres of the medulla oblongata. Some of the other fibres appeared to pass through the corpus trapezoides to the superior olive of the same side. Having crossed the mesial plain, the auditory fibres are found in the lower fillet of the opposite side, running to the inferior quadrigeminal and interior geniculate bodies, which are believed to be related to the auditory nerve in the same way that the superior quadrigeminal and exterior geniculate bodies are to the optic nerve. No changes when found above the mesocephalon in the cerebrum, or in the cerebellum and its inferior peduncle.

Prof. Hamilton‡ has continued his researches upon the corpus callosum, the result of which he has published in his paper, "Is the Commissural Theory of the Corpus Callosum Correct?" Our own anatomist, H. F. Osborn,§ has also given an excellent account upon the same subject. The lectures of Alex. Hill, in which he pointed out an interesting similarity in the minute structure of the sense organs—the eye, the ear and the nose—and his ingenious arguments of various abstruse points is, doubtlessly, familiar to all. Dr. Herringham, in a paper on the brachial plexus, can claim to have placed this difficult subject upon a more rational basis. A.

---

* *Archiv für Anatomie*, 1887.

† *Virchow's Archiv*, für 1886.

‡ *Transactions* of Royal Society, 1886.

§ *Journal American Medical Sciences*, 1886.

Digitized by Google

M. Patterson* has, by dissections, been enabled to show that the construction of the sacral plexus agrees in principle with that of the brachial. He deduces that, in the primitive condition of the limbs, the nerves passing into them are divided into dorsal and ventral branches, which supply corresponding surfaces, and that indications of this arrangement are retained in the mode of origin of the nerves and their distribution to the structures of the limb.

F. H. Hooper,† in his study of the recurrent laryngeal nerve, has shown that their recurrence is due to certain changes in the brachial arches and the descent of the heart into the thorax. At one time in the period of development the laryngeal nerves are straight, but, as the heart descends, they are brought down. The proof of this is found in the abnormal condition of the nerve in cases of irregularity of the great vessels which branch from the aorta. These nerves (at least in dogs and cats) contain no sensory fibres. The larynx possesses three functions controlled by three distinct sets of muscles, all innervated by the recurrent nerves. These functions are: 1st, respiration; 2d, sphincter action, which closes the larynx and prevents the entrance of foreign bodies, and plays an important rôle in expulsive acts; 3d, phonotory action.

The results of the extensive researches of Braune and Stahl‡ upon the calibre of the large air tubes, in relation to the capacity of the lungs, are an admirable addition to our knowledge upon this subject. They have found that the trachea is smallest immediately below the larynx, increasing its calibre to about the middle of its length, and then diminishing to about three centimetres above the bifurcation; from whence it enlarges to its end. The sum of the sections of the two bronchi at their beginning is greater than the section of the trachea three centimetres above the bifurcation. The calibre of the right bronchus at its origin to that of the left is, on an average, as 100 : 77.9. The calibre of the left diminishes from its origin to its termination. The right bronchus is too short to allow of observation being made upon it. The section of each bronchus at its origin is normally greater than the sum of the sections of its primary branches. There is a constant relation between the weight of the lung and the calibre of its bronchus. A definite relation between the calibre of the bronchi and the capacity of the lungs is also shown in pathological conditions. The calibre of a bronchus depends upon the volume of air that traverses it.

To our knowledge of the organs, some excellent contributions have been added, notably by Dr. Schieffendecker,§ who gives the position of the duodenum, the ileocolic junction, and of the sigmoid flexure, after some two hundred dissections, and the results of his observations are as follows: The only deviation in the duodenum is in the length of the second part. This changes the level at which the third part crosses the spine, and which may be as high as the disk between the second and third lumbar, or as low as the top of the fifth lumbar vertebra. The site of the duodeno-jejunal junction is constant. The opening of the ilium into the large intestine is on a level with the sacro-iliac articulation. It may be lower, and we have found it in the cavity of the true pelvis; only two cases were found in which the ending of the ileum was higher; in one it was opposite the third lumbar vertebra, in the other in front of the second lumbar vertebra; so there was practically no ascending colon.

---

* *Journal of Anatomy*, 1887.

† Transactions of American Laryngological Association. *New York Medical Journal*, July 23d, 30th, 1887. ‡ *Archiv für Anatomie*, 1886. § *Archiv für Anatomie*, 1887.

Digitized by Google

The position of the sigmoid flexure was found to be very variable, and the different forms are collected into groups as follows:—

I. When the flexure lies to the inner side of the descending colon.

(*a*) The loop hangs down in the pelvic cavity. This is regarded as the normal position.

(*b*) The loop is turned upward and applied to the posterior abdominal wall.

(*c*) The loop is directed upward, but the other parts of intestine intervene between it and the posterior wall, and it may come into more or less contact with the anterior wall.

II. When the flexure lies to the outer side of the descending colon.

There was only one example of this met with.

Waldemayer * describes the normal position of the pelvic organs in the female, and from observation on some twenty nulliparæ between the ages of fifteen and thirty, shows that His's and Schulte's description is accurate. He goes further and states that the contracted bladder in the adult is entirely behind the symphysis pubis, and is much flatter on its upper aspect than the male organ. The ureters lie against the side wall of the pelvis, close below the ovaries, very near to the hilus. The projecting fold of peritoneum which runs across from side to side over the empty bladder, and which is shown to exist also in the adult female, is named by him *plica vesicalis transversa.*

Symington † has made observations from frozen sections, and his views are coincident with the former. He states that it is erroneous to suppose the uterus to be normally antiflexed, and that it is less antiverted in the child than in the adult. The ovary they have both pointed out is not normally fixed, but possesses a large range of mobility; alterations are mainly due to change in uterine position. The uterus does not undergo any marked increase in size between infancy and puberty. The period at which the arbor vitæ disappears from the uterine body is variable. In one of the cases it was well marked at thirteen years.

Mr. F. Trevis has also given his attention to the alimentary canal and peritoneum. He has systematically dissected and described the appearances found in two hundred mammalia. Pillet and Bonlart ‡ have contributed a valuable paper, bearing upon the same subject.

A. Strekeisen, § in an elaborate article, furnishes us with some points in the anatomy of the thyroid gland. He found a pyramidal process, including cases of a superior accessory gland, in seventy-nine per cent. of one hundred and fifty-three cases examined. In the majority of cases it sprang from one of the lateral lobes, no variation being found in favor of the right or left, but rarely from the isthmus. The superior accessory gland, he says, arises from the separation of the pyramidal process, either before or after birth. It is nearly always connected to the hyoid bone by continuous gland substance, by connective tissue, or by a special muscle.

A musculus glandulæ thyroidæ occurs in nearly forty per cent. of subjects. It is connected with the pyramidal process, or with the body of the gland. The muscle may be a hyo-pyramidalis, derived from the thyro-hyoid or from the sterno-hyoid, or a thyro-pyramidalis, derived from the sterno-thyroid; in rare cases a muscular bundle was found running transversely from the lower edge of the thyroid cartilage to the pyramidal process. Accessory fasciculi may be found attached to the body of

---

* *Anatomischer Anzeiger,* 1886.

† *Edinburgh Medical Journal,* 1886.

‡ *Journal de l'Anatomie et de la Phys.,* 1886.

§ *Virchow's Archiv,* 1886.

Digitized by Google

the gland and coming from the crico-thyroid, thyro-hyoid, or the inferior constrictor of the pharynx.

Anastomoses between the arteries of the right and left side are scanty. Out of one hundred and twenty subjects examined, in twelve an arteria thyroidea ima was found twelve times. Its origin was from the innominate in six cases, from the right common carotid in two, and from the right subclavian in four.

He found that accessory glands were of very frequent occurrence in the neighborhood of the hyoid bone, and classified them into four groups according to their site:—

1. Pre-hyoid glands, superficial to the mylo-hyoid muscle.
2. Supra-hyoid glands, between or in the substance of the genio-hyoid muscles.
3. Epi-hyoid glands, above the genio-hyoid muscles.
4. Intra-hyoid glands, lodged in the hollows of the hyoid bone.

In one case a duct was found leading from the foramen cæcum of the tongue to the epi-hyoid gland. The author concludes that the foramen cæcum indicates the spot where the diverticulum leaves the pharynx. Accessory glands near the great cornu are much rarer than the others, although they are derived in the same way from the lateral diverticula which forms the lobes of the main gland.

That much-abused adenoid tissue of the nasal mucous membrane has been described by Zuckerhandl* as a normal, although largely variable, constituent in man. The adenoid tissue is found most abundantly in the respiratory division of the nose, and especially in the posterior region. In the olfactory part, the connective tissue of the mucous membrane contains interspersed lymphoid cells, but no follicles. Adenoid tissue, both of diffuse and forming follicles, is well developed in the pituitary membrane of the domestic animals, and in all of them the follicles are more numerous and constant than in man.

E. Kaufmann† has demonstrated that the prominences seen in frontal sections of the meatus auditores are caused by fine ridges of the cutis, running transversely to the axis of the canal and parallel to the margin of the tympanic membrane. The ridges are found in greatest profusion in the floor of the canal, and less frequently in the roof, and make their disappearance on the anterior and posterior walls. It is only exceptionally that one can find a few small papillæ projecting from the ridges. The ridges first make their appearance in the fourth and fifth month of fœtal life, and become less marked in old age. This corrects the usual statement which describes the skin covering the deeper part of the canal as presenting papillæ disposed more or less regularly in longitudinal rows.

Schwabach,‡ in view of the conflicting statements of F. J. C. Mayer, Luschka, Ganghofner and Trautmann, undertook fifty-three dissections to determine the *bursa pharyngea*. He finds no such pouch *normally*, but states that when it is present it is of pathological significance.

The curious phenomena of migratory testis has again received its usual attention, and the paper of Messieurs Debierre and Pravase§ places the transition of the testis upon a better footing. They recognize that the internal cremaster is an upward prolongation of the gubernaculum. Lockwood has forestalled them, and treats of the same subject in his paper on "Infantile Hernia." Lockwood attributes an important pathological function to the cremaster fibres, and considers them com-

---

* *Wiener Jahrbuch*, 1886.

† *Wiener Jahrbuch*, 1886.

‡ *Archiv für Microskopische Anatomie*, 1887.

§ *Lyon Médicale*, 1886.

Digitized by Google

petent to its production, because of the migration of the cæcum and the production of the sac of an infantile hernia.

Dr. Dalla Rosa * has pointed out to the Vienna Society the growth of the temporal muscle, and has formulated the following laws: There is a regular progressive surface growth, from birth to the end of the first dentition; from thence to the end of the second dentition, there is a regular increase with the appearances of each molar tooth; while in the interval which elapses between the first and second dentition, as well as the periods between the appearance of the molar teeth, there is a relatively small surface growth of the muscle. He further shows how, through extension of its attachment, it produces the characteristic markings on the bony surface, and finally points to some pathological value this knowledge gives.

David Wallace,† of Edinburgh, describes a case in which the musculus sternalis occurred on the right side of a female subject, and was well marked, being about an inch wide and six and a half long, with a tendon of two inches. Its origin was the costal cartilage of the sixth rib, and it received slips from the third, fourth and fifth ribs, and also from the right half the anterior aspect of the sternum, between the fifth rib and the manubrium sterni. Its fibres passed upward, and became continuous with the sternal origin of the right sterno-mastoid, giving a few fibres to the great pectoral muscle. The muscle was supplied by a nerve which was traced to the *external* anterior thoracic nerve. This differs from former observations. In the cases reported by Shyhard, Cunningham and Abrams, the nerve supply was reported from the *internal* anterior thoracic nerve.

H. St. John Brooks,‡ in an elaborate essay, has communicated that the outer head of the flexor brevis pollicis muscle most frequently receives branches from both ulnar and median nerves. In thirty-one dissections he has seen the following variations: The outer head, in five cases, was supplied by the ulnar nerve alone; by the ulnar and median in nineteen cases; the outer head by the median, the inner by the ulnar, in five cases; the outer head by the median, the inner by the median and ulnar, in two cases. He has also shown that the muscle known as the flexor brevis minimi digiti, in human anatomy, and the larger part of the opponeus minimi digiti are derived from Cunningham's adductor layer, while the ulnar head of the true flexor brevis is only represented by that part of the opponeus beneath the deep branch of the ulnar nerve; the radial head of the flexor brevis forms the third palmar interosseous muscle. The so-called deep head of the flexor brevis pollicis is really a part of the adductor, and the proper ulnar head of the short flexor is the first palmar interosseous of Henley. The separation and reduction of some of their parts, shown by their muscles, is attributed to the pressure of the more developed long flexor tendons.

Dr. Adolph Ott,§ in three hundred sections of the heart of a five-month embryo, demonstrated that no ganglion cells were found in the ventricular walls below the aperture of the pulmonary artery and aorta. From the semilunar valves upward they regularly increased in number. Particularly profuse were they in the septum atriorum or auricularum cordis. Taking his clue from these observations, he demonstrated ganglion cells in the corresponding sites in the adult heart.

Fleming‖ has attempted to show that in the rabbit the Wolfian duct originates

---

* *Wiener Medicinische Blätter*, Nov. 24th, 1886. † *Journal Anatomy and Physiology*, 1887.

‡ *Journal Anatomy*, 1886. § *Prager Medicinische Wochenschrift*, 1886.

‖ *Quarterly Journal of Microscopical Society*, 1886.

Digitized by Google

from the epiblast. If this observation could be clearly proven, it would reconcile many discrepancies; but it is so out of accord with what occurs in other vertebrates, that, without doubt, even after Speer's paper showing like origin in the dolphin, more evidence will be demanded upon this point before its acceptance.

Bailey and Lockwood's valuable researches upon the fascia, muscles and ligaments of the orbit have excited the interest which they deserve.

I regret that complete translations of the work of Hennum, of Christiana, are not extant, for in his experimental research he has formulated some new anatomical laws. He applied pressure to variously-formed elastic globes, and showed that they conformed to strictly geometric crystallographic figures. He could demonstrate that similar relations result in the developments of all formations in the various organs, and particularly in the epithelial.

Mr. W. Balton Spencer, in a study of the pineal body, attributed to De Graaf the merit of having clearly shown that in one particular animal the pineal gland is actually modified into a structure comparable to the invertebrate eye. Mr. Spencer carries the researches much further, and shows that in many lizards the invertebrate eye possesses a cornea, lens and retina of unanticipated perfection.

Much had, of course, to be omitted, owing to the limited time at my disposal, but I have endeavored to give you, in concise language, the results of recent labors of anatomical workers in our field. Time only prevents me from mentioning to you some of the numerous anomalies that have been reported.

With this modest introduction to our work, the Chair, which is your executive only, gentlemen, is ready, if the Section is, for the first paper in order.

THE DEVELOPMENT OF THE TORUS LONGITUDINALIS IN TELEOSTIANS AND ITS HOMOLOGY IN HIGHER VERTEBRATES.

DU DÉVELOPPEMENT DU TORUS LONGITUDINALIS DANS LES TELEOSTIENS ET SON HOMOLOGIE DANS LES PLUS HAUTS VERTÉBRÉS.

DIE ENTWICKLUNG DES TORUS LONGITUDINALIS IN DEN TELEOSTIERN UND SEINE HOMOLOGIE IN HÖHEREN WIRBELTHIEREN.

BY PROFESSOR H. RABB-RÜCKHARD,

Berlin.

In a series of former publications, I succeeded in proving that the different parts of the brain of Teleostian fishes in no way essentially differ from what anatomy and embryology teach us on the higher classes of vertebrates. All previous authors had overlooked the existence of a pallium or rind of the hemispheres, taking the rounded hills which are found just in front of the lobi optici (or mesencephalon), and which are separated from each other by a median fissure, for a solid cerebrum without anything comparable to a cortex. This, however, I discovered, in the form of a very thin epithelial membrane, enveloping the hemispheres from above. I succeeded in removing this membrane, which is always destroyed by breaking off the skull from above, and therefore constantly escaped the notice of former investigators, by cutting the brain together with the skull, after having softened the bones. I had previously made out, by studying the embryology of the Salmonides, that the hemispheres were composed of a pallium (cortex) and a corpus striatum, both separated by an ample cavity, which is the

Digitized by Google

homologue of the united lateral and median ventricles, and which I proposed to call ventriculus communis. What till then was supposed to be the whole hemisphere, turned out to be only the corpus striatum (Insula-Stammlappen of German authors). The brain of a mammal, of a mouse or rabbit, for instance, may be transformed to the appearance of a Teleostian brain by breaking off the dorsal cortex of the hemispheres, and thereby laying open the ventricular surface of the corpora striata.

I only hinted at the interesting fact that in the Teleostian fishes that part of the brain which in higher vertebrates is the seat of the highest psychical functions, namely, the cortex cerebri, with its innumerable ganglion cells, is reduced to a simple layer of ciliated epithelial cells. A localization of the different cerebral functions in higher vertebrates, according to the experiments of Fritsch, Hitzig, Ferrier, Munk and others, is impossible in the Teleostians, simply by the absence of any nervous tissue in the cortex.

I am glad to state that this view of the brain of fishes is now almost universally accepted by anatomists. At the same time, it is proved that no homology whatever is possible between parts found in the lobi optici of the Teleostians and the hemispheres of higher vertebrates. The lobi are homologous to the mid-brain (mesencephalon, corpora quadrigemina) of the mammalia, and, when looking for equivalent parts in both, we are restricted to those found in the latter. Now it happens that the lobi optici of Teleostian fishes present a seemingly much more complicated structure than the mid-brain of the other vertebrates. There is a folded mass projecting in the cavity of the lobi from behind, which is nothing else than a mighty valvula cerebelli. But there are other parts, not less complicated, we find no equal of in the mid-brain of higher vertebrates.

First of all, there exists the Torus longitudinalis, a double protuberance of a longitudinal stripe immediately beneath the dorsal roof of the lobi optici (tectum loborum), which was found by Carus in Clupea hazengus, and declared to be the fornix. A few years ago Professor Fritsch, of Berlin, to whom we are indebted for a splendid monograph on the brain of Teleostian fishes, accepted this view, trying to corroborate it by minute investigations. It needs no further proof that this opinion is perfectly erroneous. The fornix is a part of the hemispheres of higher vertebrates, and must not be sought for in a region of the fish brain, which is homologous to the mid-brain. But the fact that there was not a vestige of a part comparable to the Torus longitudinalis to be found in the mid-brain of higher vertebrates excuses Carus and Fritsch for seeking an homology in other regions of the brain. Neither was the development of the Torus known.

I hope the following paper will cast a light on both points :—

As to the development of the Torus, studied in the Salmonides, I found that the roof of the lobi optici in an early period is represented by a simple layer of tissue, attenuated in the median line, without even a trace of the Torus. Consequently the roof on either side of the median line is very much thickened, whereas the thin median part does not undergo any change for awhile. Then there begins a proliferation of the inner cells, producing a double longitudinal stripe, which, protruding in the ventricle, is gradually transformed to the Torus longitudinalis of adult fishes.

One must, before all, bear in mind this development of the torus as a proliferation of the inner cells, which form the ependyme in higher vertebrates. When seeking for an homologous formation in the latter, we are obliged to look for the innermost epithelial layers covering the walls of the Aquæductus Sylvii, which is the reduced ventricle of the lobi optici of fishes.

In examining the mid-brain of Rana esculenta, Lacerta ocellata, Psammosaurus terrestris, Iguana tuberculata, Chamæleon, Chelonia midas, Alligator mississippiensis, I was struck by the fact that the epithelial ependyme is not equally developed

Digitized by Google

in all parts of the ventricle. There exists a locality immediately behind the commissura posterior, beneath the roof of the mid-brain, where the cells show a quite extraordinary lengthening, forming a sort of double longitudinal fringes, which are most remarkable in the mid-brain of Chelonia, Alligator and the bigger Saurii. By their symmetrical disposition on both sides of the median line, they resemble the embryonic stage of the Torus in Teleostians.

On further investigation I stated the existence of the same fringes in the brains of birds (Columba domestica), and I am sure they will also be found in mammalia in the same place. As to mammalian embryos, I made sure of their existence, they being beautifully developed in the Edentata (Xenurus gymnurus).

I am therefore compelled to conclude that the Torus longitudinalis of the Teleostians, hitherto unknown in all the other classes of vertebrates, is represented in the latter by a singular and constant proliferation of the ependyme cells beneath the roof of the corpora quadrigemina, immediately behind the commissura posterior.

Thus, the Torus longitudinalis is deprived of its isolated existence in fishes, and demonstrated as a rudimental formation, even in the highest vertebrata.

ON THE CEREBRAL VESICLE IN THE HUMAN EMBRYO.

SUR LE VESICULE CÉRÉBRAL DE L'EMBRYON HUMAIN.

ÜBER DIE GROSSHIRNBLASE DES MENSCHLICHEN EMBRYOS.

BY PROFESSOR VON GERLACH, M.D.

In the previous century Vicq d'Azyr observed on the medial surface in the gray substance of the convolutions of the occipital lobes a white stripe, by which stripe the gray layer of these convolutions was divided into an external dark gray stratum, a middle white one—the stripe of Vicq d'Azyr—and an internal light gray stratum.

Baillarger described, in the first half of the present century, a second white stripe in the gray substance of the cerebral convolutions, by which the number of strata was thus increased to five ; an external gray, an external white, a middle gray, an internal white and an internal gray layer.

Through Meynert, a more scientific microscopical basis for these five strata of the cerebral convolutions was obtained.

About the year 1857, while I was studying the effect of stains on animal tissues, the magnificent colored spectra obtained from sections of the convolutions of the cerebellum engaged my attention to a high degree. Sections of the cerebellum of even 1 mm. in thickness were stained so beautifully after a short treatment with ammonia-carmine, that single strata gave a picture sharply enough outlined in their different shadings to have served as samples in any calico printing or wall paper manufactory.

I was much surprised that the same results could not be obtained upon staining sections of the convolutions of the cerebrum.

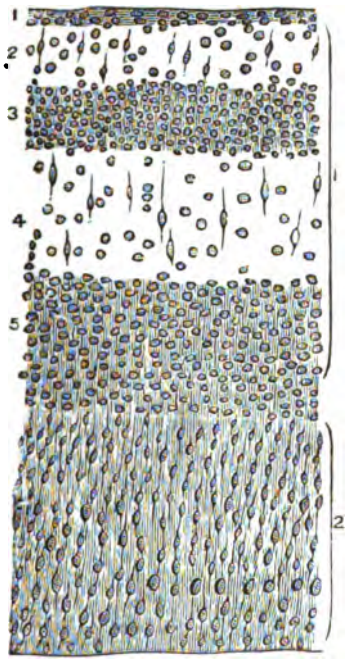
In most of the sections which had been prepared for staining in the same manner as those of the cerebellum, the white substance was but slightly stained, the gray peripheral more intensely. In the brain of the newborn only were the two stripes of Baillarger faintly but distinctly visible, absorbing less pigment than the three other strata of the gray substance separated by them.

Digitized by Google

A fortunate circumstance enabled me to obtain a quite fresh human embryo, three cm. long, from head to nates. This specimen I treated with chromic acid, and for a short time with dilute alcohol, my usual procedure with the convolutions of the cerebrum and the cerebellum.

In eight days the specimen was ready for dissection. I cut through the middle of the body, the neck and the head, and made from the latter sagittal sections. Upon examining these after staining with ammonia-carmine, I was surprised at the sharp differences of color presented—intensely red where there were cells only, less red where there were both cells and interstitial formations, and a very light red where there were but few cells. My interest was especially awakened by the examination of the cerebrum, which really consisted of the anterior cerebral vesicle only. The cerebral vesicle

FIGS. 1, 2.



Section through the cerebral vesicle of a human embryo, 3 cm. long from head to nates (Enlarged 200 times.)
1. Gray substance. 2. White substance. 1, 2, 3, 4, 5. The five gray layers of the gray substance.

was sharply divided into two layers (Figs. 1 and 2), which can hardly be explained in any other manner than that they were the gray and the white substance of the cerebrum. The external layer, that is, the gray substance, was nearly twice as thick as the internal ; and in it the stratification appeared with the same distinctness as in the convolutions of the cerebellum, in the adult. A very thin external cellular layer was separated from a middle thicker one, and that in turn from a still thicker internal layer, by the two stripes of Baillarger, which were almost destitute of cells and were of a pale red color. All three cell layers were characterized by an intense red, forming a color spectrum unsurpassed for representing the layer in the gray substance of the convolutions of the cerebrum. I have used them only in my lectures on the anatomy of

Digitized by Google

the brain, when demonstrating to the students, by means of a projecting apparatus, the stratification of the gray substance of the cerebrum. The white substance of the cerebral convolutions appeared quite different from that of the adult, which, as is known, consists only of fibres in cylinders, surrounded by medullary sheaths, radiating like a fountain to the gray substance.

I once noticed, in a section of an older embryo, that the chief mass of the white substance consisted of oblong cells, on account of which it gave an intensely red color.

My assistant, Dr. Herrmann, who was occupying himself particularly with nucleus segmentations, observed in these cells of the white substance a striking number of nucleus divisions, which could not be ascertained in the cells of the gray substance.

In all older embryos, in which the convolutions of the cerebrum were more or less perfectly developed, I could demonstrate the stratification of the gray substance, after preparation with ammonia-carmine; but the difference between the five layers appeared the more distinct, the younger the embryo was.

In an embryo still younger than that described above, but which, unfortunately, I did not receive when quite fresh, and which was less satisfactory for staining on account of the beginning maceration, in consequence of the hardening in chromic acid and alcohol, I could still ascertain that the white substance of the cerebral vesicles, compared with the gray, existed in a less proportion than in the embryo three cm. long.

I think that the following conclusions may fairly be drawn from what has been said: The stratification of the gray substance of the cerebrum, after preparation with ammonia-carmine, appears the more distinct the younger the embryo. The destruction of color decreases with the advancing development preceding birth, and still more after birth. In an adult, it is hardly possible to show the five layers of the gray substance of the cerebral convolutions, after preparation with ammonia-carmine. The first rudiment of the cerebral vesicle is to be considered as morphologically equivalent to the gray substance of the convolutions of the cerebrum. The white substance of the primary rudiment is only minimum, but increases its ratio to the gray along with the development of the embryo.

THE ANATOMY OF THE RECTUM IN RELATION TO THE REFLEXES.

L'ANATOMIE DU RECTUM PAR RAPPORT AUX RÉFLEXES.

DIE ANATOMIE DES MASTDARMS MIT BEZIEHUNG AUF DIE REFLEXE.

BY JOSEPH M. MATTHEWS, M.D.,
Louisville, Ky.

It is a matter of every-day occurrence that diseases are observed with a history of reflex symptoms. The organs and tissues of the body are so intimately connected by sympathy, which is made manifest in disease, that it would be a wonder indeed if they could be overlooked. And yet we are constantly meeting with cases which have been misunderstood, for the reason that the reflexes have not been properly considered.

Perhaps the field of gynæcology reveals more evidence of this than any other department, yet, after a number of years in the observation of rectal troubles, I am persuaded that this portion of the anatomy is much neglected, not only in relation to the diseases that affect it, but also to it as a seat of many of the reflexes. That I may more clearly elucidate my subject you will permit me to recall some of its anatomical bearings. I

Digitized by Google

have had to speak quite recently of the part that the sphincter muscles play in disease, and the subject will be touched upon in this paper. The mucous membrane of the rectum is thicker than that of the colon, and just beneath it is found an increased layer of cellular tissue which connects it with the muscular layer beneath. In this membrane the follicles of Lieberkuhn are freely distributed. In structure they are very like the villi of the small intestine and covered by the same form of epithelium, and in their walls is a similar arrangement of capillaries. I shall only refer to the so-called Houston valves in the mucous membrane of the rectum, to say that I doubt their existence, and if found in some few cases I doubt the claim that their office is to support the weight of fæces. The rectum receives blood from three different sources; the upper part is supplied only by the superior hemorrhoidal, a branch from the inferior mesenteric, which also supplies the lower part of the colon. The terminal branches of the superior hemorrhoidal pass to the lower part of the rectum, but the principal blood supply to this part comes from the middle and inferior hemorrhoidal, which are primary and secondary branches from the internal iliac, which artery affords the principal blood supply to all the pelvic viscera. The middle hemorrhoidal is distributed to the pouch of the rectum, while the inferior, a branch from the internal pudic, passes across the ischio-rectal fossa and reaches the rectum at its lower part. The internal pudic, besides giving a large supply of blood to the rectum, supplies blood to the bladder, prostate, vagina, perineum, and external organs of generation. The veins which return the blood from the rectum are numerous. The hemorrhoidal plexus communicates in front with the vesico-prostatic in the male, and the vaginal plexus in the female. While the inferior and middle hemorrhoidal arteries supply the principal part of the blood to the lower part of the rectum, the corresponding veins return but a small portion of this blood. Almost all the blood from the rectum passes through the superior hemorrhoidal veins and into the portal system. The nerve supply of the rectum comes from two sources. It receives an abundant supply from the hypogastric plexus of the sympathetic system. In addition to these, we find a supply direct from the spinal system of nerves, those to the rectum coming from the fourth anterior sacral nerve. This is the only part of the intestinal canal which receives branches direct from the spinal nerves. The great irritability and sensibility of this part can be easily understood. It is a fact that it requires deeper anæsthetization to perform operations upon the sphincter than upon the eye. Its nerve supply is greater than that of any other muscle of the body. They come from three different sources, from the internal pudic, the fourth sacral, and the posterior sacral nerves. It is a rule in the distribution of nerves that the same nerve supplies a muscle and the integument over it. There is no exception here. They pass in beneath the external sphincter until they reach the space between the inner border of this and the internal sphincter; there they divide into two sets of branches, ascending and descending. The ascending branches are distributed to the mucous membrane crossing the internal sphincter, the descending to the integument. The principal one of the nerve branches to this part comes from the internal pudic, a branch from the lower part of the sacral plexus. The pudic nerve is distributed to the muscles and integument of the perineum, to the penis, and integument of the scrotum in the male, and to the corresponding part in the female. Hence, the relation and great sympathy between the lower rectum and all parts of the perineum, and external organs of generation. The sphincter ani and sphincter urethral muscles are supplied by the same nerve. We have also traced branches from the fourth sacral nerve to the bladder, prostate and vagina. Tracing all these nerves to their origin, we find that the spinal nerves, supplying all the pelvic viscera, all the structures forming the perineum, and external organs of generation, are given off from the same point in the spinal cord. Hence, it is easy to understand that the rectum can irritate and be irritated in return, from many sources and by many conditions. It is not within the scope of this paper to speak of

Digitized by Google

all the reflexes that have their origin in the rectum, but I wish to refer to a few of the most important. Because of the abundant blood supply of the rectum it is natural, in the exercise of its peculiar office, that its blood vessels should often be in a state of congestion, and this alone excites to many reflexes. Constipation is a great breeder of disease, for this reason. When there is no response to the daily call of nature to go to stool, the anti-peristaltic action of the gut causes a portion of the fæces to pass back into the sigmoid and leaves a portion in the rectum proper. This is added to each day, and the liquids of the fecal mass being absorbed leaves the mass in a hardened and dried condition, which acts as a foreign substance, irritating and congesting the parts. As a result, we frequently find, in women, the reflexes producing pain in the bladder, mouth of the urethra, womb, back, thighs, ovaries, vagina, perineum, etc.; in men, pain in the bladder, penis, urethra, scrotum, prostate, etc. Many of these troubles are directly traceable to the rectum as their source, and just as often is the origin overlooked.

The relation of the rectum with the peritoneum is so close that oftentimes abdominal pain is but a reflex from these parts. The meso-rectum, dipping as it does within a finger's length of the outlet, is contiguous enough to take on inflammatory trouble from many conditions that may exist in the rectum. I have had occasion to point out, for this reason, the dangers that might arise from injecting internal hemorrhoids with carbolic acid. We have seen how easy it is for the rectum to become congested because of its inmost blood supply and dependent position. Being equally supplied with nerves, this congested state causes great reflex action to every part to which their filaments extend. It is my observation that when the rectum is congested, from whatever cause, the discharge of mucus is often taken as a symptom of more serious trouble than exists. Charlatans are in the habit of parading these symptoms as of the gravest importance, and many fall into the trap. The reflex from this congested condition of the rectum is usually shown upon the womb and its appendages.

CASE I.—A lady, aged twenty-four, married, was referred to me by a gynæcologist. History. Had complained for months of backache, pains in the thighs, general lassitude, melancholia, bearing-down sensation in both the vagina and rectum; pain over seat of both ovaries; constipated habit, leucorrhœa, loss of flesh, irregular menstruation, difficulty of urination, a slight discharge of mucus from the bowel. Upon examination of the womb the gynæcologist had not seen enough trouble to account for her symptoms. He treated her for several months and advised her to consult me. Upon examining the rectum with speculum I found it highly congested, very red and sensitive; a discharge of mucus covered the entire circumference for several inches up. The cause for this extensive congestion was not discernible. I was satisfied, however, that all the symptoms mentioned were purely reflex, and proceeded to treat the rectum. Hot water injections were ordered to be taken twice daily for several days, after which the entire portion of the congested gut was brushed over with a forty per cent. solution of nitrate of silver. After three or four days I began to make application of liquid hydrastis and water, equal parts. This was changed for the pure liquid hydrastis. The redness and pain gradually disappeared, the discharge ceased, and all reflex trouble vanished. This is but a sample case of many that have come under my observation.

The rectum having a nerve supply direct from the spinal system through the fourth anterior sacral nerve, it is often the case that a diseased rectum with reflex to the cord, and symptoms simulating grave trouble of the same, may present.

CASE II.—An army officer was referred to me for an examination and treatment for a large rectal prolapse. He was then under treatment for supposed locomotor ataxy. Upon questioning him concerning his general condition, the following symptoms and history were elicited: Pain in both legs, with a decidedly unsteady gait; great nervousness, weakness of both legs, loss of sexual power, persistent constipation, heavy forcing

Digitized by Google

pain in the rectum, numbness in feet and legs, melancholia and general restlessness. Upon forcing out the rectum, after enema, a very large prolapse was discovered, which had existed for many years. Taking the diagnosis of ataxy as correct, I could not promise him much by the operation for prolapse, save to free him from the inconvenience of the same. The operation was done, however, and as the wounds healed, all symptoms here described began to gradually disappear. After two months he pronounced himself cured of all trouble, rectal, ataxy, etc. He now walks a steady gait and great distances; bowels regular, sexual appetite and capacity returned, no pains or numbness in legs, and altogether a different man. I am satisfied that the whole train of symptoms reflected from the rectum. It is well known that a large number of diseases of the eye are traceable to disturbances in distant organs through the sympathetic system. Instance the sexual apparatus, as in masturbation, stricture of the urethra, uterine and ovarian troubles.

Dr. Joseph A. White, of Richmond, Va., in an excellent article, speaks of hysteria being intimately associated with reflexes, and cites such cases as so-called anæsthesia or hyperæsthesia of the retina, paralysis or spasm of the ocular muscles producing myosis, ptosis, strabismus, etc. In evidence that the rectum is no exception for these same reflexes, I would mention that I have under treatment at the present time several patients that were referred to me by Drs. Reynolds and Cheatham, of Louisville, in which the eye troubles are produced and controlled by rectal trouble. These cases are of frequent occurrence. It is an admitted fact that the rectum is the frequent seat of gummatous deposit, the result of syphilis. This adds a large list of troubles that are reflex, especially so of the cord. In 1877 I had the honor to report a case of this kind to the Kentucky State Medical Society. You will permit me to refer to it briefly. A young man applied to me for treatment for a most persistent constipation, going, as he said, often for weeks without a movement from the bowels. He was put upon the usual remedies, without benefit. I had searched for rectal complications but found none. He began to present symptoms very like one with locomotor ataxy: weak legs, a bad gait, marked strabismus, numbness in lower limbs, violent pain over sacrum. These presenting, I sent him to Prof. J. W. Holland for treatment. The treatment which was given him for said condition for weeks availed nothing. He again came to me, and I resolved to explore the rectum higher up. I did so, and detected a strictured surface near the sigmoid flexure, of small calibre. I traced his history and found that he had syphilis a year before. I diagnosed his case to be syphilitic stricture of the rectum. The obstruction being nearly total, I advised an operation. Colotomy was thought of, but the patient being a very large man, with immense belly, it was discountenanced. The stricture being located so high up precluded all idea of dilating with instruments. The patient was chloroformed and I introduced my hand into the rectum and carried it up to the site of stricture. Making a cone of my fingers, I gradually pushed them through the strictured surface. He suffered great shock after the operation, but rallied and was soon able to be up. He was put upon anti-syphilitic medication and all the symptoms that I have described cleared up, showing them to have been purely reflex. We are all familiar with the reflexes as shown upon the genito-urinary organs, the womb, prostate, etc., that are witnessed daily. Of these I will make no mention. Neither will I refer to those diseases having their origin elsewhere which show themselves by reflex on the rectum. Although these are germane to the subject, I would prefer to hear their recital by others.

Digitized by Google

ON TWIN SUDORIFEROUS GLANDS.

SUR LES GLANDES JUMELLES SUDORIFÈRES.

ÜBER ZWILLINGSSCHWEISSDRÜSEN.

BY PROF. LEO GERLACH,

Of Erlangen, Bavaria.

In the development and growth of a secreting gland, the spreading of the gland epithelium, or, in other words, the secreting surface, may go on in one of two ways: (1) Either the tubular cavity enclosed by the gland cells and the membrane proper grow lengthwise simply, and upon ceasing this form of growth may at last roll up into a coil, thus giving us the tubular or cylindrical form of the gland; or (2) the layer of the gland immediately separates, and this being repeated rapidly in both branches an acinous gland is formed, in consequence of the continued ramification and vesicular swelling of the blind closed ends of the numerous gland canals.

In manuals of histology, a sharp distinction is usually made between the tubular and the acinous type of gland; but it must be remembered that there are forms of glands, in regard to which one may be in doubt whether to class them with this or with that kind of gland tissue. I recall only the pyloric glands of the stomach, in which the gland tube ramifies at its end into two short sides, ending in moderate swelling. There are, however, intervening transitions between the two kinds of glands.

To those which are usually represented as the prototype of the tubular glands belong the sudoriferous glands. But it has also been proved beyond doubt, that in rare cases separation of their tubes occurs, so that in their formation, though exceptionally and in a limited degree, processes must have taken place similar to those occurring in the genesis of acinous glands.

The occurrence of two sudoriferous glands uniting in a common duct was proved in the beginning of the fourth decade of this century. In a treatise, excellent for that time, on the skin, in R. Wagner's Manual of Physiology (p. 108, Vol. II, 1884), C. Krause mentions twin sudoriferous glands, which were rather seldom found, scarcely one being observed among sixteen or twenty other sudoriferous glands. But before Krause, according to a memorandum of Kölliker's, R. Wagner and Giraldès had occasionally seen two-sided sudoriferous glands; and they were possibly seen by Bresihet and Rousset de Vauzème also. This latter observer made the strange statement that transverse anastomoses existed between the two tubes of sudoriferous glands, a mistake caused by seeing dichotomous ramifications of the sudoriferous tubes.

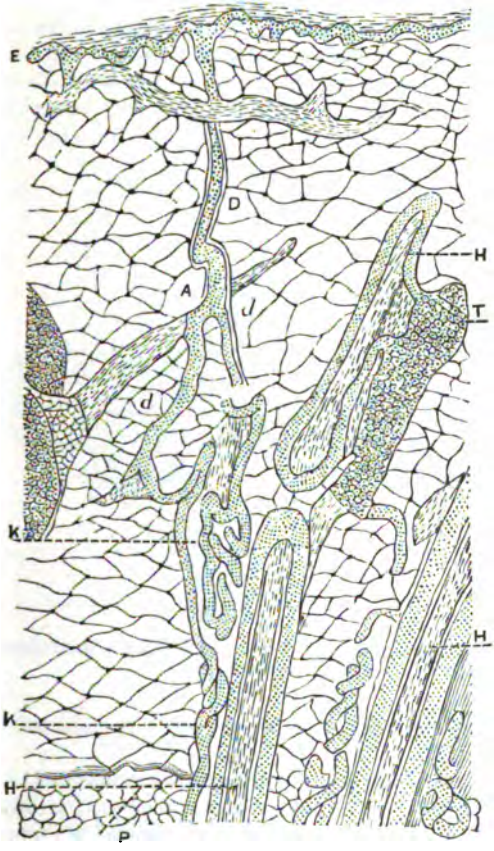
In the later literature, no attention is paid to this rare form of the sudoriferous glands. In manuals of histology and anatomy they are not usually mentioned, or at most there is quoted the foregoing statement of C. Krause, to which our scanty knowledge of this peculiarity is limited. There are also, I believe, no drawings by which the relative positions of the two gland-coils, the approximate length of the common excretory duct, its thickness as compared with the calibre of its branches, and some other interesting points, can be seen. For this reason, some notes on these matters may not be undesirable.

In examining sections from the human scalp, prepared for my microscopical courses, I happened to see a twin sudoriferous gland. This induced me to make a closer examination, and I searched about eighty sections for more glands, with the result of finding a ramification of the excretory duct six times. On an average, each cut contained eight excretory ducts. At this rate there must have been in the eighty cuts about six hundred sweat glands, among which six twin glands were found. The ratio of the

Digitized by Google

latter to the former would thus be about one per cent. But I believe that twin glands occur more frequently than it would seem from this calculation, for the following circumstance was not taken into consideration, and it is one which may have been a source of considerable error. In a series of sections made through the scalp, the excretory duct of the same gland will doubtless occur in several different sections, because of the oblique direction of the sudoriferous canals in passing through the skin. It is also possible that I missed several of the twin glands in my search, however careful I

FIG. 1.



Part of a section through the haired scalp, in which a sudoriferous gland is shown almost to its full extent. K. K. The two gland coils. D. Common excretory duct. d. d. The two branches. A. The dilatation. T. Sebaceous gland. H. Hair. E. Epidermis. P. Paniculus adiposus.

was. For these reasons, I estimate the occurrence of such glands to be about three per cent., and I consider them to be of rarer occurrence than stated by C. Krause, who supposed that the ramifying excretory ducts are about five per cent.

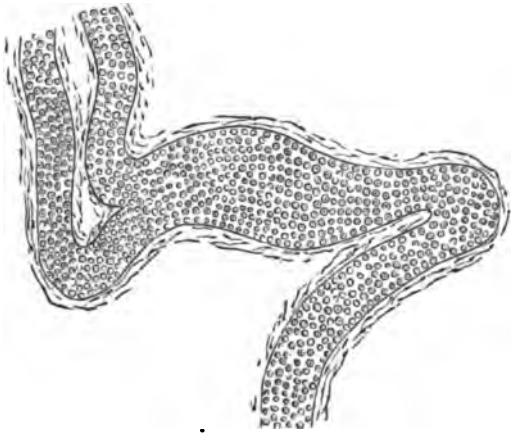
Another circumstance which seems to favor my opinion is, that Heynold, to whom we are indebted for a careful inquiry into the human coil-shaped glands, never noticed the undoubtedly dichotomous ramification of the gland-tubes, either in the ordinary sudoriferous glands of the skin or in the large coil-shaped glands of the armpit, which

Digitized by Google

differ from the former in many respects. It is possible, though, that the twin glands are more numerous in certain parts of the skin than in others, or that they are of particularly frequent occurrence in some individuals. Comparing closely the glands in my preparations, I found, as a constant result, that the ramifications of the excretory duct into two tubes occurred in all cases at the same height. Dissecting the cutis vera into three equal horizontal layers, the plane of ramification always lay in the middle half of the upper third. It was nearly on the same level as the inosculation of the sebaceous gland with the nearest hair follicle. (Fig. 1.)

Closely above the dichotomous division, the lower part of the common excretory duct was swollen into a pear-shaped dilatation, showing a different expansion; in one case only was a swelling entirely missed. This dilatation of the tube may reach to twice the size of the excretory duct. In Fig. 2, I have represented such a case. In this gland the expansion was more distinct in form; it had a diameter of 80 μ and a length of 250 μ. It is remarkable that the dilatation ran here with its longitudinal

FIG. 2.



Place of ramification of the excretory duct of a twin gland, with a transverse dilatation. (*Hartnack System*, VII.)

diameter in a transverse direction; in all other cases it was perpendicular to the surface of the skin.

With regard to the thickness of the common excretory duct and its parting branches, I find that the diameter of the former is always somewhat greater than that of the latter. In the gland the dilatation of which I have copied in Figure 2, the two tubes had a thickness of 30 μ-35 μ, whereas below the ramification the thickness of the common canal was 40 μ. A difference of structure in the two portions of the excretory duct could not be found, but it appeared to me that the common sudorific canal ran in a much straighter course than its two branches. The latter, before entering into the proper gland coil, usually curve still more or make short bends, often even forming several nearly regular spiral turns; but the common excretory duct ascends in an almost straight line to the epidermis. It shows at most only one or an immediately repeated bending immediately above the dilatation, causing, perhaps, a slight stowage of secretion, which itself may again be the cause of a greater expansion of the dilated part.

From what has been said, it will be seen that the common excretory duct must be

Digitized by Google

shorter than the two gland tubes, which extend from the expansion to their entrance into the coiled gland body. But this difference in length is brought about not only by the numerous windings of the crural canals, but also by the fact that the ramification is in the upper half of the cutis vera, and is therefore nearer to the epidermis than to the subcutaneous cellular tissue.

I did not notice any peculiarity with regard to the sudoriferous canals of the twin glands in the epidermis.

In closing, I will remark briefly on the relative positions of the two gland bodies, the excretory ducts of which finally unite. In an examination of the arrector pili, made in my laboratory, Diering demonstrates that in the haired scalp the coils of the sudoriferous glands are always under the sebaceous glands, which they may approach so closely that the muscle arrector pili forms a local septum between the two. From the drawing of Diering, it appears, also, that those gland coils which lie near the hair sac in the lower beds of the cutis, and partly reach into subcutaneous tissue, are usually not round bodies, but are stretched into a longitudinal form. Such oblong gland bodies border upon many hair sacs on both sides. In my preparations I noticed a different arrangement in the position of the two coils of a twin gland. In one of my cuts the one gland body was under the other, both bordering upon one side of a hair sac, as shown in Figure 2. In another case, unfortunately, not all the parts of the gland had fallen into one section. It appeared to me that the two coils were placed opposite each other, at the same height, on both sides of a hair. Finally, it may occur that, under the dilatation, the one branch may descend by the side of the nearest hair, bordering with a gland body upon its sac, while the other branch runs off obliquely or in a transverse direction to a neighboring hair, to form a coil on its side. Therefore, the two gland bodies of a twin gland may assume a local relation to two different but closely situated hairs.

THE DIDACTIC TEACHING OF HUMAN ANATOMY.

L'ENSEIGNEMENT DIDACTIQUE DE L'ANATOMIE HUMAINE.

DER DIDAKTISCHE UNTERRICHT IN DER MENSCHLICHEN ANATOMIE.

BY F. W. LANGDON, M.D.,

Professor of Descriptive and Surgical Anatomy in the Miami Medical College of Cincinnati (Medical Department University of Cincinnati).

Anatomy being, by universal consent, the foundation of all rational medicine, the methods of presenting the subject to students merit the careful consideration, not only of professional teachers, but of physicians in general; for there are few of the latter who are not called upon, at some period of their career, either to act as teachers, or to give advice as to methods of study.

Of the two plans of teaching the subject commonly in vogue, viz: the demonstrative and didactic, the latter only requires consideration here; there being no question as to the utility of, and necessity for, demonstrations on the cadaver.

The writer is not of those who believe or profess to believe that no benefit is to be derived from text-books or didactic lectures on Anatomy; if this be the case in many instances, it is rather, in his opinion, the fault of the text-books or lectures than of the system itself.

Perhaps every didactic teacher has held his own views as to the best method of presenting Anatomy to his classes, and yet there are few, probably, who have not felt, at

Digitized by Google

times, that their efforts were, in a measure at least, a failure, so far as assisting the student's understanding and progress were concerned.

An extended experience as a teacher, in both dissecting room and amphitheatre, as well as to private pupils, has convinced the writer that not only is there room for modification and improvement in existing methods, but that such improvement is quickly appreciated by the average student.

With a desire, therefore, for a comparison of ideas and methods, believing that the importance of the subject merits it, the writer ventures to present his own views as to the objects, scope, and methods most desirable in a didactic course of lectures on anatomy for medical students.

The primary object, of course, of all teaching, is to impart knowledge. Knowledge without understanding, however, is necessarily imperfect, and it will be conceded by all in favor of an advanced standard of medical education, that the student should not only *know*, but *understand*, so far as possible, the human body. That is to say, a mere cramming of encyclopædic facts is not a sufficient education in anatomy to satisfy modern requirements in the graduate of medicine.

Facts are all important as a foundation for *principles*, and it is with this idea in view, that they should be taught. The *objects* of anatomical study, then, may be presented in this order :—

1. Knowledge of facts.
2. Knowledge of principles.
3. Knowledge of *application* of these facts and principles to the promotion of physiological processes, the curé of disease, the repair of injury, and the relief of deformity.

As regards the *scope* of didactic teaching ; what facts, then, should precede the study of anatomical principles? I would reply, first, those pertaining to the gross physical characters of the various tissues ; secondly, the facts of histology, or cellular anatomy —not in the disjointed way often taught, but by the student making, for himself, a series of sections of the normal tissues, and familiarizing himself with their microscopical appearances and actions.

Only when these are mastered, is the student prepared to grapple with the *principles* of the construction of the human body.

I have seen students who could give the most minute ramifications of a given artery, and repeat the nerve supply of group after group of muscles, completely floored when asked to name the tissues entering into the construction of the knee joint?—and yet this is evidently a question involving the practical application of important principles in treatment.

Supposing the groundwork of gross and microscopic histology to be well laid down, the scope of the didactic course may be stated as follows :—

1. A review of the elementary facts of structure, or the study of materials.
2. The principles of structure, or the study of construction.
3. Applied anatomy, for physiological and clinical uses.

Whereas, in the dissecting room and histological laboratory the studies are mainly analytical in nature, the didactic course, being more advanced, may profitably assume a synthetic direction. Both methods of viewing the subject conduce to a more thorough understanding of principles than either method alone.

This brings us to the consideration of *methods*, or details of didactic teaching. As already indicated, the elementary facts of structure are best studied in the dissecting room and histological laboratory, by analytical processes. In the amphitheatre more time should be devoted to the principles of anatomical construction. To put the subject in another light, we have heretofore studied building materials and now advance to the subject of architecture.

Digitized by Google

The architecture of the body, synthetically stated, consists in (1) the union of cells to form tissues. (2) The combination of tissues to form organs. (3) The association of organs to form systems or apparatuses.

The systematic arrangement of these topics, so as to be easily retained and referred to,—*i. e.*, their classification—has been recognized as a necessity by all writers and teachers.

With respect to anatomical classification for teaching purposes, it may be held that the simplest classification is always the best, provided it does no violence to principles of structure. Since the main object of classification is convenience for mental retention and reference, those classifications of tissues, for instance, which, like Bichat's, recognize twenty-one primary types; or with Frey, eighteen, are too cumbersome for convenience. Again that of Gegenbauer, recognizing only *four* primary tissues, while morphologically simple, and therefore commendable, is not applicable to the every-day needs of the student and practitioner of medicine. It is this latter application that should be constantly kept in view, in medical teaching.

For practical purposes I deem it sufficient to recognize seven primary tissues, namely :—

Epithelial, Fluid or nutritive (blood, lymph and chyle), Connective, Cartilaginous, Osseous, Muscular, Nervous.	Constituting the connective tissue group of Gegenbauer.

Each of these is recognizable by the naked eye, and to one or the other any tissue of the body may be referred.

It may be observed that those constituting the connective tissue group of Gegenbauer are so diverse in development, structure and functions, and consequently in pathological relations, as to render their recognition as distinct classes extremely desirable from a practical point of view.

In the classification of *organs*, we likewise find great diversity of views. While one author adopts a topographical classification, a second resorts to nerve supply as a basis; while a third selects similarity of structure as a guide, thus giving us glandular organs, vascular organs, muscular organs, etc.

Now, it would seem to the writer that either of these methods loses sight of the main object of anatomical knowledge, viz. : its application to clinical phenomena :—

When we consider that the diagnosis of disease during life depends mainly on disordered *function*, it certainly seems more rational to study organs with reference to their actions and uses in the economy—in other words, to adopt a physiological classification, which is the one finally resorted to in practice.

Pursuing this system we find the organs falling naturally under eight heads, corresponding with their sequence and uses : viz. : Organs of—

1. The Tegumentary system (or apparatus) comprising the skin and subcutaneous connective tissue, with their appendages—hair and nails.
2. The Locomotory system, comprising all the bones, joints and voluntary muscles.

To the possible objection that some of these, *e. g.*, those of the upper extremity and head, are not properly locomotory organs in man, it is sufficient to refer to their use in swimming, and, in the infant, in creeping; while all know that restriction of the movements of the muscles and joints of the head or neck will produce marked locomotory changes.

Digitized by Google

3. The Nervous system, including the nerve-centres, cerebro-spinal and sympathetic, with their branches, and inter-communications; also the organs of special sense.
4. The Circulatory system, embracing the heart, arteries, veins, capillaries and the lymphatics with their glands, together with the spleen and all ductless glands.
5. The Respiratory system—nasal passages, pharynx, larynx and lungs.
6. The Digestive system—from lips to anus, with its appendages.
7. The Urinary system—kidneys, ureters, bladder, urethra.
8. The Generative system.

That anatomical knowledge classified in this way is eminently applicable to clinical needs may be illustrated by supposing the student or practitioner be asked to make an examination of an unconscious patient with a view to diagnosis.

1st. The skin presents itself, with its variations of texture, temperature, color, moisture, etc., and the presence or absence of œdema in the subcutaneous tissue is noted.

2d. The locomotory apparatus is examined, with reference to fracture or dislocation, contusion, etc.

3d. The nervous system is interrogated, with a view to the presence of paralysis, motor or sensory, reflex irritability, as manifested by the pupils and other phenomena; organs of special sense (*e. g.*, the eyes) may also furnish an index to the condition of the intracranial circulation and pressure.

4th. The condition of the circulation is noted—the pulse, color, presence or absence of cardiac murmur; condition of lymphatic glands, splenic enlargement, indications of plethora or anæmia, and so on.

5th. The respiratory organs are investigated—the action of the alæ nasi, laryngeal sounds, if any, stertor; the rate, depth and rhythm of the respirations, râles, etc.

6th. The digestive system receives attention—from the lips, tongue and teeth, character of matters vomited, if any, presence of hernia, distention or enlargement of abdominal organs, etc.

Finally, the condition of the urinary and generative organs having been ascertained, how much greater is the probability of an accurate diagnosis from some such system of investigation, than from the hap-hazard or irregular investigation of various organs without reference to their physiological relations.

This applicability of anatomical system to clinical work is, in the writer's opinion, a most important matter to keep in view.

The fallacy that anatomical knowledge is applicable chiefly to *surgical* diagnosis and procedures, is one that is largely due to faulty teaching, and should be vigorously combated. To be even a safe "fever doctor," a knowledge of anatomy is essential.

The writer's views respecting the teaching of anatomy may be briefly summarized as follows:—

The anatomical curriculum should embrace three main divisions:—

1. Analytical work in the dissecting room and histological laboratory.
2. The synthesis of the body; a study of materials and principles of construction—the didactic course proper.
   - (*a*) The composition, structure and functions of the cell, considered as the anatomical unit.
   - (*b*) The variations in the primary cell (ovum) and its progeny, from which result the (seven) primary tissues.
   - (*c*) The study of these tissues with respect to their development, structure, destination and uses.

Digitized by Google

(*d*) The formation of organs by combination of tissues, and their classification with reference to function.

(*e*) A course in the elements of comparative anatomy might be added with advantage.

3. Applied Anatomy: a topographical study of the location of organs, with especial reference to the interpretation of symptoms and medical and surgical procedures in general.

In short, facts must precede principles, and principles lead up to practice, as in other branches of knowledge. It cannot be too strongly insisted upon that the meaning of education, as the derivation of the word implies, is "to draw out," *i. e.*, to develope the faculties—not merely to crowd them with facts. Hence, the true teacher cannot be satisfied with a mere recital of facts. His higher office is to point out methods of study, to encourage the observation of facts, and the deduction and application of principles.

The teaching of anatomy in America is largely a labor of love, since in few of our medical colleges is the compensation of the chair at all commensurate with the time and labor required; nor does it bring with it the pecuniary emoluments which accompany chairs of practice.

Hence, the need which exists for special endowments in this department.

ON SOME OF THE PHASES OF EMBRYONIC BONE DEVELOPMENT.

SUR QUELQUES-UNES DES PHASES DU DÊVELOPPEMENT DE L'OS EMBRYONIQUE.

ÜBER EINIGE PHASEN DER EMBRYONALEN KNOCHENENTWICKLUNG.

BY W. XAVIER SUDDUTH, M. D., D. D. S., F. R. M. S.,

Director of the Physiological and Pathological Laboratory, Medico-Chirurgical College of Philadelphia.

In accordance with the title of our paper, we shall confine our remarks simply to the embryonic development of bone.

There exists in the literature of the present day three well-defined processes by which bone is formed, viz.: intra-cartilaginous, intra-membranous and subperiosteal. Bones are also divided into two classes: cartilage and membrane bones. Cartilage bones are the product of both intra-cartilaginous and subperiosteal ossification. In length they increase by intra-cartilaginous, and in girth by subperiosteal, formation. Membrane bones, although beginning as such, complete their ossification finally by subperiosteal deposition. From the foregoing statement it will be seen that intra-cartilaginous and subperiosteal ossification persist throughout the ossific process.

Intra-membranous ossification occupies only a transitory stage in embryonic life, soon passing into subperiosteal. The process of intra-membranous ossification, though belonging to the class of embryonic bone formation, is too well known to require more than a mere notice. I desire to call your attention to-day to another process which precedes intra-membranous ossification in the development of the embryo, and which, to my knowledge, had not been described in literature until I brought it out in an article in the "American System of Dentistry," in 1885.

To the development of bone in the embryonic connective tissue, uninfluenced by either membrane or cartilage, I gave the name of "Interstitial" ossification. Exception has been taken to the term interstitial as applied to ossific products, on the ground

Digitized by Google

that it had previously been used to designate the supposed process by which bones increased in size after birth, especially the inferior maxilla. But since it has been conclusively shown that bones do not grow "interstitially," as the word was then understood, and since the term has fallen into disuse in that connection, I see no impropriety in adopting it in this instance, especially as it so aptly explains the process herein described.

Intra-membranous and interstitial bones play only a temporary rôle in the developmental process, the latter having, perhaps, a shorter duration than the former; yet in the study of embryology they perform as important a part as does cartilage. That they have but little practical bearing upon the general subject of anatomy I am well aware—confined as they are to embryonic and to fœtal life.

For general classification, the two terms membrane and cartilaginous bones are sufficient; but for the student who would enter into the nicer study of fœtal anatomy other classifications are necessary.

Intra-membranous ossification is most typically shown in the development of the embryonic skull cap, and it has been fully described in existing literature. Interstitial ossification is a previous expression of bone formation, and may be said to be provisional in character, resembling cartilage in that respect.

Cartilage is the matrix, or mould, which serves as the antetype of the mature product, and the same can also be said of the interstitial bones. The fœtal jaw, the best defined example of the latter process, is as truly the antetype of the mature jaw as the fœtal femur, preformed in cartilage, is the pattern or matrix that shapes the mature femur. In the fœtal jaw even the form of the alveoli, microscopic in size, can be made out, enclosing a microscopic dental follicle—the mature product in miniature. The malar bones and the clavicle are developed in a manner similar to that of the maxillæ, and for each there forms a matrix, around which the mature bone is developed. It is to the special manner of development of the maxillæ, the clavicle, and the malar bones that I desire to call your attention. The malar, pre-maxillæ, and the superior maxilla are not prefigured in cartilage, and ossification begins in them prior to the formation of membrane or cartilage in other parts of the body. The inferior maxilla and the clavicle are prefigured in cartilage, but the cartilage plays little or no part in the ossification of their bodies, although their articulating ends are developed by intra-cartilaginous ossification.

While studying the development of the jaws, my attention was first directed to the need of an additional classification, and I was surprised, upon looking up the literature on the subject, to find almost no reference made to the manner of development of these bones.

In "Elements of Embryology," by Foster and Balfour (p. 405), the ossification of the human clavicle is dismissed in the following manner: "The clavicle in man is provided with a central axis of cartilage, and its ossification is intermediate between that of true cartilage bone and membrane bone."

Regarding the ossification of the inferior maxilla, there is, so far as I am acquainted with the literature on the subject, only one illustration which has been published with a view of showing simply the earliest stages of maxillary ossification. That illustration is to be found in Klein's "Elements of Histology," and it is taken from an almost mature fœtus. Judging from the paucity of literature and illustration on the subject, one is naturally led to infer that the earliest steps in bone formation have not been so thoroughly worked up as the later forms of ossification.

In describing the process, I shall confine myself to the bones of the face, premising that ossification in the human clavicle is analogous to, and concomitant with, ossification in the inferior maxilla.

The first indication of bone formation in the body is found in the clavicle and the

Digitized by Google

malar bones. In man this occurs about the beginning of the second month, sometimes a little earlier. Ossification in the maxillæ follows shortly afterward, the inferior maxilla slightly preceding the superior. Next in order the membrane bones of the skull develop; then the intra-cartilaginous and the subperiosteal bones develop. The connective tissue of the mesoblastic layer, when ossification in the clavicle and the malar bones first makes an appearance, is still decidedly embryonic in character. There has been no attempt on the part of nature to differentiate periosteum or membranes. My most satisfactory work has been done on fœtal pigs, for which I have constituted the following table of lengths, comparing their development with the development of other embryos which I have studied: A fœtal pig 1 cm. in length compares histologically with a four-weeks human embryo, a twelve-days fœtal rabbit, and a forty-eight-hours chick; a 1½ cm. fœtal pig to a six-weeks human embryo; and a 2½ cm. fœtal pig to a two-months human embryo and a four-days chick. The cells in the mesoblastic layer of a 1 cm. fœtal pig have not assumed a fibrillated character, the somites are only in process of formation, and no true cartilage has as yet been differentiated. Development now progresses very rapidly, and by the time another centimetre has been added to the length, plates of cartilage are seen and ossific points make their appearance in the positions to be occupied by the clavicle and the malar bones, respectively.

The youngest well-preserved human embryo which I have had the pleasure of studying, was probably about two months old. In it I found the clavicle considerably developed and ossification in the inferior maxilla already begun.

I have had younger embryos, but they were not sufficiently well preserved to use for fine histological work. My best work on ossification has been done on fœtal pigs. They more closely resemble human embryos than do rabbits or chicks, upon which I have also done considerable work.

I obtain fœtal pigs fresh, and while they are yet warm, I place them in Müller's fluid, to harden, for several days, after which I finish the hardening process in alcohol. In this way I am enabled to get magnificent preparations. I use Prof. Delafield's hæmatoxylon and eosin, for a double stain, also alum carmine, picro-carmine, alone or in combination with iodine green, and this latter makes a very beautiful stain for developing bone. I find that for general embryological work, hæmatoxylon and eosin make about as good a double stain as is needed. Eosin is especially good for the demonstration of blood corpuscles.

In this article, our study of developing bone is made from sections of fœtal pigs, stained with hæmatoxylon and eosin. The illustrations are photo-lithograph, made from photo-micrographs.

Our description begins with a fœtal pig, 1 cm. in length, which compares histologically, as regards development, with a four-weeks human embryo. At this age no ossific points are to be seen. The epiblast is composed of one or two layers of embryonic cells, the bioplasts of Beale, lying in a bed of protoplasm. They present no histological difference from the mesoblastic cells lying underneath, except that they are arranged in a layer and lie more closely together, giving the layer a denser appearance in stained preparations. This row of cells constitutes the deepest layer of the *rete Malpighii*, or what I have termed the "infant layer." (See Fig. 1.)

The mesoblast is composed of nucleated bioplasmic bodies, oval or round in form, also lying in a bed of protoplasm, with which substances they are constantly bathed, and from which they derive their nourishment.

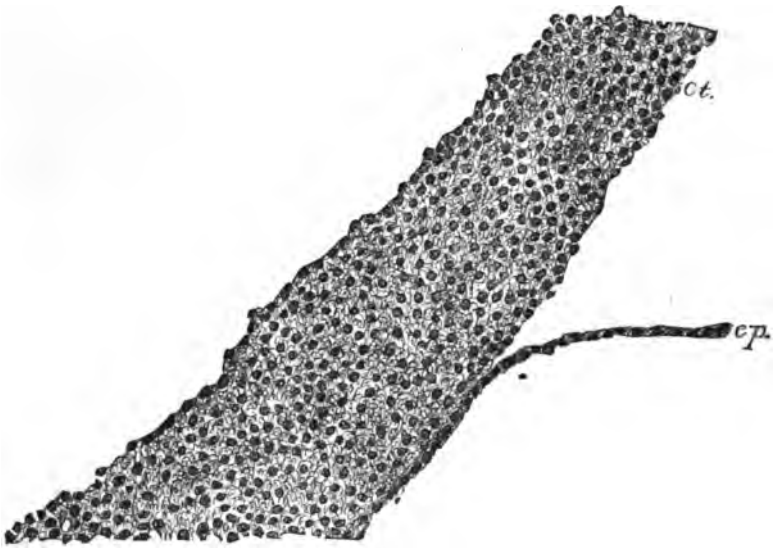
They present no cell body, and hence have no "cell-wall," in the strict sense in which that term is used. They are simply nuclei, and as they advance in age they accumulate around themselves formed material, which constitutes the cell body. They thus assume distinctive and characteristic forms. The connective tissue cells of the

Digitized by Google

mesoblast become fibrillated, their processes gradually thicken and elongate until they are distinctly visible, even with low powers.

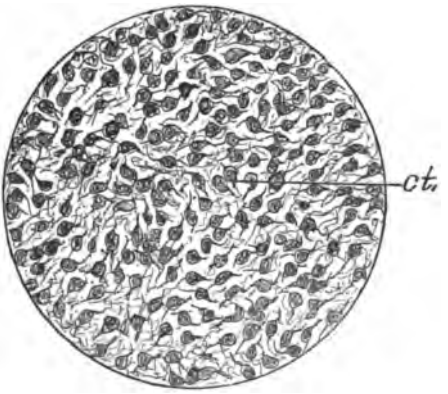
Fibrillation is more or less marked in a pig 2½ cm. in length (see Fig. 2). It is at

FIG. 1.



ct, connective tissue of mesoblast; *ep*, epiblast (single layer of cells). The epiblast is separated from the mesoblast mechanically.

FIG. 2.



Porcine Embryo (2½ cm. × 250): *ct*, embryonic connective tissue of mesoblast.

this age that ossification first makes its appearance in the clavicle and the malar bones of the human embryo, and in the inferior maxilla of the porcine embryo.

At this stage of development the blood vessels are seen as mere channels in the embryonic connective tissue, the corpuscles staining with eosin.

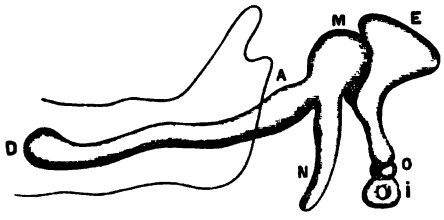
Digitized by Google

The walls of the capillaries are first formed by the more or less fibrillated connective tissue cells; in some instances arranged longitudinally in the direction of the blood current, in others they appear to wind around the current of blood. No muscular tissue has as yet been differentiated.

The inferior maxilla is composed of two lateral halves, united in the median line. It consists of an outer single layer of epiblast covering a solid band of embryonic connective tissue. At the anterior portion the tongue and the inferior maxilla are united into one solid body, and are both covered by the in-dipping epiblastic layer. The mucous membrane of the mouth is derived from the same layer of the blastoderm as the skin, and differs from it, even in adult life, only in the fact that from being constantly bathed in the fluids of the mouth, it consequently does not present the corneous appearance found in the skin—an appearance due simply to desiccation. The central portion of the inferior maxilla, shortly after the union of the two lateral processes, becomes differentiated into a cartilaginous cord, which serves to strengthen the jaw. To this band or cord the name of its discoverer, Meckel, has been given. It is formed in two separate parts (see Fig. 3), arising from the middle of either side. These unite, as do the lateral processes of the jaw, at the symphysis mentis.

The cartilaginous matrices of the bones of the ear become directly ossified, as do also the articular ends of the cartilage of the jaw.

FIG. 3.



Meckel's Cartilage, from Human Embyro of Forty to Forty-two days, and before the appearance of the maxillary bone (magnified ten diameters). A, enlargement of cartilage near its union with the neck of malleus; D, union with cartilage of opposite side; M, head of malleus; N, handle of malleus; E, cartilage of the incus; i, cartilage of the stapes; O, cartilage of the os lenticulare. The outlines of a jaw have been added to the figure, to show the relative position occupied by the cartilage in the jaw.

Surrounding the central cartilage is seen the now more or less fibrillated embryonic connective tissue, deeply stained in well-prepared specimens. The proportion of connective tissue to cartilage is ten to one.

The cartilaginous band does not increase in size, but the connective tissue develops very rapidly.

In the central part of the connective tissue of the inferior maxilla, on the outer side of Meckel's cartilage, the ossific points first make their appearance. These are seen in specimens stained with hæmatoxylon and eosin, as groups of oval or round cells, that are more deeply stained than the remainder of the embryonic connective tissue cells. Except for the darker stain taken and their somewhat larger size, they fail to present any points which under a lower power will differentiate them from the other surrounding cells. Under high powers, however, they do not show the same fibrillated appearance seen in the ordinary connective tissue cells, and in this latter respect they more nearly resemble the plasma cells of older tissue.

It is a well-known fact that those cells which are younger stain more deeply than older cells, and this is also true of the younger portions of the same cell. And, again, cells, when actively functionizing, stain more deeply than do the same cells when qui-

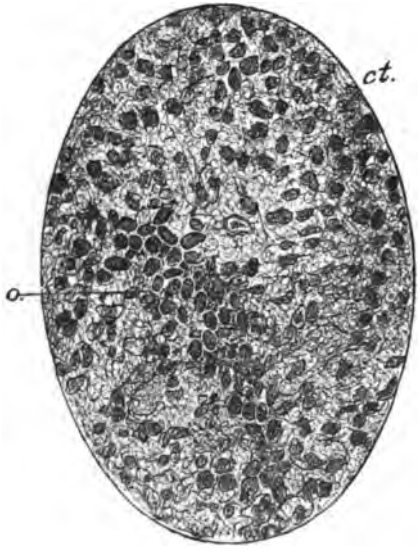
Digitized by Google

escent. Just what property such cells possess at the one time, and not at the other, which causes this difference, we have not been able to determine, but that such a difference exists there can be no doubt.

These groups of cells under consideration, by this peculiar property of selection of stain, differentiate themselves and stand out in strong contrast to the other cells in double-stained specimens. This variation in shade I have reproduced by means of photo-micrographs, and it is well shown in the accompanying figure. (Fig. 4.)

By following out serial lines of studies, in sizes ranging from this stage, 2½ cm. in length to 3 cm., 4 cm. and upward, I have been enabled to constitute a progressive series of steps in the development of bone, and can say that these groups of cells are osteoblasts. They soon form themselves into double layers, and the process of trabeculæ building is begun.

FIG. 4.



Inferior Maxilla, Porcine Embryo (2½ cm. × 240): *o*, osteoblasts grouped together, surrounded by embryonal connective tissue, *ct*.

Now, at this stage of development no bone formation has occurred in the skull cap, neither has intra-cartilaginous ossification begun. The development of provisional bone seems to be concomitant with the formation of the supporting cartilaginous structures, and to play nearly as evanescent a part in the further growth of the embryo as does cartilage. That the osteoblasts found in this character of ossification arise from the ordinary connective tissue, I think there can be no doubt. They are simply modified mesoblastic cells, which are endowed with special functionizing power. Sections through the face of a two-months human embryo show the process of bone formation in the region of the malar bones somewhat more advanced. Here some of the osteoblasts are enclosed in shells of forming bone, which at first have the appearance of being spiculæ, but upon closer observation, it will be seen that each osteoblast lies imbedded in a bony matrix, which it has secreted around itself. (See Fig. 5.)

Digitized by Google

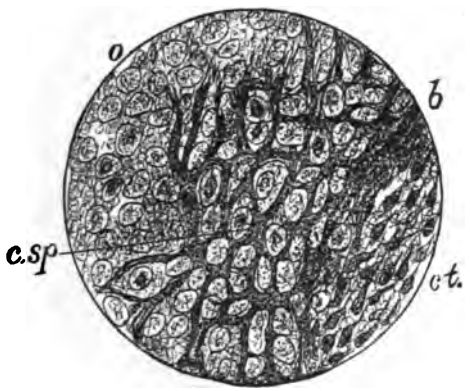
The form that depositing bone assumes depends largely upon the character of the tissue into which the salts of calcium are deposited. In intra-membranous ossification bone is developed in long, narrow spiculæ. In subperiosteal ossification bone is deposited in layers, which, in turn, enclose successive layers of osteoblasts.

In the development of provisional bone, occurring, as it does, in the central portion of the tissue, the secreted lime salts are deposited around the spherical osteoblasts, more or less in the form of spheres.

The apparent spiculæ, if examined closely with a high power, will reveal as many cup-shaped excavations upon their sides as there are osteoblasts in line.

* "Under the superintendency of the osteoblasts, a crescentic layer of true bone is deposited upon the side of the osteoblast in apposition with a similar crescentic layer formed by an osteoblast located on the opposite side of the line. The sides of each crescent join similar crescents formed by fellow-workmen on either side. As deposition progresses, the osteoblast becomes encircled by a shell of lime. As the trabecula widens, by enclosing the osteoblasts which lie upon its sides, new layers of osteoblasts are found

Fig. 5.



Human Fœtus (2 months; × 250): *o*, osteoblasts (the dark lines which come to the edge of the figure at *b* represent bands of forming bone); *c. sp*, calco-spherule surrounding an osteoblast; *ct*, embryonal connective tissue.

arranging themselves on the walls, which, in turn, become enclosed in a layer of bone. Thus, by the accumulation of successive layers of calco-spherules, the broadening of the bands of bone tissue is accomplished. As the osteoblasts build themselves into the wall, their places are taken by fresh recruits. When each osteoblast, by secreting its calco-spherule, completes its life-work as a bone builder, it becomes a *bone cell*, and from that time on occupies the house it has builded. (See Fig. 6.)

"The crescentic nature of the first part of the layer secreted by the oesteoblasts is plainly shown when they (the osteoblasts) are displaced or where they are considerably shrunken. As the process of secretion proceeds, the osteoblast becomes enclosed in a thin spherule of formed material, designated by Mr. Rainey as *calco-globulin*. This shell of bone is pierced here and there by the fibres of the osteoblast which are left as the latter shrinks. The deposition of bone is really in the meshes of the fibres.

* Sudduth, in "American System of Dentistry."

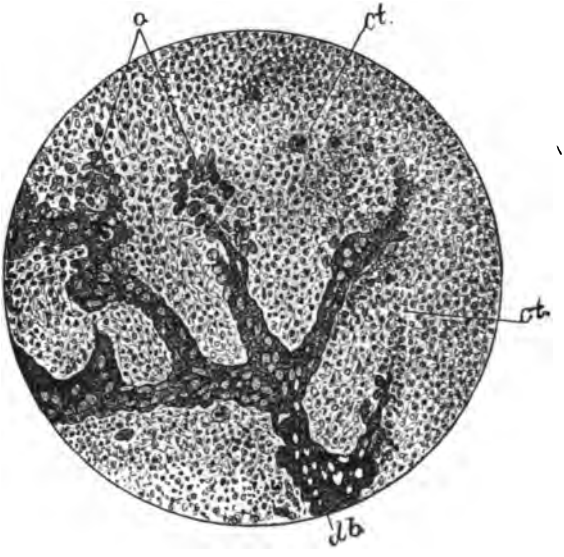
Digitized by Google

"The body of the cell is spheroidal, hence the deposition assumes a spheroidal form; accordingly, we denominate it a *calco-spherule*.

"As the process of secretion goes on depositing from the circumference toward the centre, the fine processes before mentioned continue to be united with the osteoblasts. Their terminal fibrillæ anastomose with those of other osteoblasts, and these again with others; those which lie nearest the capillary vessels connect with them, thereby receiving nourishment, which they, in turn, give to the outer layer of bone cells. The office of these processes, then, is to supply the nutrient matter needed to support life in the bone cells.

"Bone cells are nothing more or less than encapsuled osteoblasts, which are occupying the homes which they have builded themselves. The cavities which they occupy

FIG. 6.



Porcine Embryo (5 cm. long, × 250): *a*, osteoblasts situated at the ends of the lamellæ of bone; *d b*, developing lamella of bone containing bone cells; *ct*, *ct*, embryonal connective tissue.

are the *lacunæ* of the old writers, the canals in which their processes lie are the *canaliculi*, and the capillary vessels the *Haversian system*. These lacunæ and their canaliculi, together with the Haversian canals, are occupied in living bone by the above-described organic element."

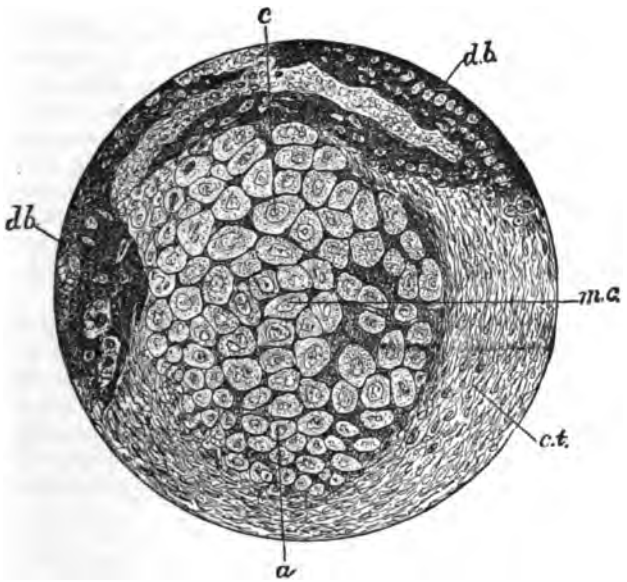
Having thus accounted for the ossification of the inferior maxilla, without the direct aid and influence of Meckel's cartilage, it yet remains to explain the method by which it disappears. The cartilage becomes enclosed in a sheath of bone, which is developed in the surrounding embryonal connective tissue. As the deposition progresses, the cartilage is encroached upon, is broken down under the influence of the osteoblasts, and becomes incorporated into the substance of the maxilla.

The changes are plainly shown in the accompanying cut. (Fig. 7.)

Digitized by Google

There is no proliferation of the cells, as in the head of the femur, nor does Meckel's cartilage increase in size after the process of bone formation begins. The process of ossification differs from that known as *inter-cartilaginous;* in the latter case there is rapid proliferation of the cartilage cells, the cartilaginous head (femur) increasing in size in proportion to the encroachment of the ossification zone. This does not occur in ossification of Meckel's cartilage. There is no increase of cartilage cells, except at the points of articulation, where true inter-cartilaginous ossification occurs. In the body of the jaw the cartilage simply becomes calcified, and afterward ossified and incorporated into the substance of the maxilla, as before stated. It entirely disappears before the fifth month —not by *wasting* away, but by ossification. This change begins, as we have seen, at two and a half months ; at three months it is almost complete, and at four months, in

FIG. 7.



Meckel's Cartilage, from jaw of two-and-a-half-months human fœtus, undergoing ossification. *a*, normal cartilage cells; *c*, enlarged cells containing calcific material; *db*, *db*, developing bone; *ct*, connective tissue (× 250).

nearly every case which I have examined, no trace of the cartilage remains. In the pig it persists much longer, and is unaffected by ossific processes in embryos ten centimetres in length.

Such is a brief description of the process in the inferior maxilla, which also answers for the superior maxilla, the malar bone and the clavicle. With the differentiation of a periosteum, cortical bone formation begins, and this is soon followed by internal absorption of such bone as has been interstitially developed. The space occupied by the first developed bone in the fœtal jaw will constitute the canal of the dental nerves in the mature jaw.

Lengthening of the jaws is by intra-cartilaginous ossification at the condyles.

Digitized by Google

A CURIOUS CONGENITAL DEFORMITY OF THE SPINAL COLUMN PROBABLY DUE TO MATERNAL IMPRESSION ON THE FŒTUS.

UNE DEFFORMITÉ CONGÉNITALE DE LA COLONNE VERTÉBRALE DUE PROBABLEMENT À L'IMPRESSION MATERNELLE SUR LE FŒTUS.

EINE MERKWÜRDIGE ANGEBORENE MISSBILDUNG DER WIRBELSÄULE, WAHRSCHEINLICH DURCH MÜTTERLICHEN EINDRUCK AUF DEN FÖTUS BEDINGT.

BY BENJAMIN LEE, A.M., M.D., PH.D.,

Of Philadelphia.

The question of the influence of maternal impressions on the fœtus has recently been brought prominently before the minds of the profession, in consequence of the reading of an elaborate and learned paper on the subject by Dr. Fordyce Barker, before the American Gynæcological Society, followed by a discussion of great interest, opened by Dr. S. C. Busey, of Washington.

Both of these distinguished gynæcologists expressed the conviction that the conveyance of such impressions is possible, and adduced thoroughly authenticated cases in proof of the theory. Dr. Barker, after a careful résumé of the arguments which have been advanced on both sides of the question, says: "I think the weight of authority must be conceded to be in favor of the doctrine that maternal impressions may affect the development, form and character of the fœtus." Among the names which he adduces as committed to such a belief are those of Montgomery, Rokitansky, Carpenter, Dalton, Flint, Grimaud de Caux, Martin St. Ange, Devay and Hammond. Dr. Busey expresses the "belief that there is some relation of cause and effect between the mental impressions of mothers and fœtal deformities," and supports it by several well considered propositions and a long list of collected cases.

Dr. Goodell stated that while he scouted the extravagant statements made by the laity on this subject, yet he was inclined to believe that "there was more in it than physiologists are willing to concede." In the course of the paper and discussion fifty-one cases were related, more or less at length, corroborative of the truth of the theory. The majority of them are either of the nature of nævi or of arrest of development resulting in deficiency of a portion of a limb or of soft tissues. That which I am about to relate does not partake of those characteristics, but seems more distinctly mimetic, and is, I think, quite unique.

HISTORY.

Daniel Paul Neiler, aged one year and nine months, was brought to me on the 26th of April, 1886. His parents reside at Chestnut Hill, Philadelphia. He was pale and ill-nourished, and had frequent attacks of pain, usually on awakening from his nap during the day. His gait was irregular and tottering. He was wearing a very heavy plaster jacket, which he had had on for a year. When this was removed his attitude was that of lordosis, but a projection of the last dorsal vertebra could be seen on sitting. As a young infant, his health was vigorous, and he was quite free from any unusual pain. The only thing that attracted his mother's notice was that when held on the arm he had a constant tendency to throw himself backward, and that he showed little disposition to walk as he arrived at the age when her other children had done so. About a year ago she observed a little lump on the back, and called the attention of the family physician to it. He pronounced it a case of spinal disease, and ordered the spinal jacket. Two days after the jacket was applied, he walked for the first time. He was then ten months old. On examining the spine I was at once struck with its extraordinary flexibility. Instead of the rigidity which one always expects to find at the seat of disease in spinal caries, especially when it has existed for so long a period as a year, there was unusual mobility at that point, and there was an entire absence of that

Digitized by Google

careful holding of the shoulders, awkward gait and turning in of one or both toes, which are such characteristic symptoms of an acute inflammatory condition in the course of the spinal column. Anchylosis evidently did not exist, nor were there any symptoms of ulcerative action, and yet, when in the sitting posture, there was a distinct protuberant knuckle, just such as one would see in a case of Pott's disease. Over this knuckle of bone there was a well-developed bursa, which increased the apparent deformity. When he stood up, the back arched in, the knuckle disappeared, and the hand, held closely against it during the change of attitude, felt the vertebræ glide over one another, as though there were a free articulation, with cartilages and synovia at that point in the spine.

My conclusion, therefore, was that this was a congenital malformation, that there had never been any inflammatory or ulcerative action, that the irregularities of locomotion proceeded entirely from difficulty of preserving the equilibrium, owing to the abnormal mobility of the spine. I believed that the screaming spells on awakening from sleep were due to the pressure of the jacket upon the spinous process at the seat of deformity, of the severity of which the bursa spoken of was a sufficient proof. The irritation thus produced and the weight of the plaster combined would account for his loss of appetite, languor and emaciation.

Acting on this belief, I removed the jacket, and substituted for it an extremely light spinal corset. The result was an immediate amelioration of all the symptoms. He at once walked with more ease and steadiness, his appetite returned, and his screaming spells ceased. He has continued to improve up to the present time. On inquiry of the mother as to whether she had received any injury during gestation, she said that she had not, but that almost every day during that period she was obliged to see a little girl who was much deformed with spinal curvature, the disease being in the middle of the back. She was full of pity for the unfortunate child, and, as she expressed it, "couldn't keep her eyes off of her" when she was near. This appeared to me to afford a solution of the peculiarities of the case. It was the first one of the kind that I had met in a somewhat extended experience, and I have thought it of sufficient interest to bring it to the notice of the distinguished men composing this Section, and obtain an expression of opinion in regard to it. If my understanding of the case is correct, it simply constitutes the substitution of a diarthrodial for a synarthrodial articulation, under the influence of a powerful and constant maternal impression, producing an imitation of the deformity which aroused the mother's interest and excited her pity.

THE ANATOMY OF STRICTURE.

L'ANATOMIE DU RETRÉCISSEMENT DE L'URETHRE.

DIE ANATOMIE DER HARNRÖHRENSTRIKTUREN.

BY J. NEELY RHOADS, M.D.,

Assistant in the Gynæcological Department of Jefferson Medical College Hospital.

Before entering upon the anatomy of stricture of the urethra, let us glance at the anatomy of the normal urethra.

First, allow me to say that the male urethra is not a canal of uniform diameter, for, if we begin with the meatus urinarius, we have the two labiæ thrown edge to edge vertically, forming a narrow, slit-like opening, which is invariably (in its normal condition) the *smallest* part of the entire urethral tube. Directly behind the meatus the

Digitized by Google

urethra widens out, under the name of the *fossa navicularis*, to twice the size of the meatus, returning again, one and a half inches from the meatus, to a much smaller canal. This portion of the urethra contains the glands of Littré, the openings of which are directed forward, and vary in size from openings that can scarcely be discerned, to large pouch-like openings, as the *lacuna magna*, that renders unskilled instrumentation troublesome.

One and a half inches behind the meatus the urethra contracts into an uniform canal one-quarter of an inch in diameter, and continues so for about four and a half inches, when it again widens in the bulbous portion, to be again reduced, about six inches from the meatus, into the smallest calibre of the tube, except the *meatus urinarius*. This part is called the spongy portion. Continuous with the spongy portion, about three-quarters of an inch, is the membranous; this, as was inferred above, is the smallest part of the entire canal, except the orifice, and terminates at the apex of the prostate gland. This part of the urethra is embraced by the *compressor urethræ* muscle and triangular ligament. Directly following and continuous with the membranous part of the canal is the *prostatic urethra*, the widest and most irregular part of the whole canal. Upon the floor of this portion is a septum-like band or ridge of erectile tissue, called *veru montanum*, which is supposed to prevent the semen from flowing backward into the bladder; on either side of this ridge are the orifices of the prostatic ducts and all the prostatic sinuses. In part of the veru montanum is a sinus called *vesicula prostatica*, which contains the longitudinal openings of the ejaculatory ducts. The prostatic urethra is another stumbling block to the unskilled in instrument passing.

The urethra of the female is an uniform canal about one-quarter of an inch in diameter, except at the meatus, approximating one inch in length. The triangular ligament embraces the female urethra in a similar way as it does the male, and is also embraced by the compressor urethræ muscle in a similar manner.

Both the male and female urethra are made up of three coats: muscular, erectile and mucus. Like the male, the female urinary meatus is always the smallest, and is also always the most sensitive part of the entire canal, causing nearly *all the pain incidental to the passing of bougies, catheters and other urethral instruments.*

Stricture of the urethra is divided by most authors into two forms, namely, spasmodic, or those that are always temporary, and organic, or those that are always permanent.

Spasmodic stricture of the urethra is an occlusion of its calibre, which may be either abrupt or gradual, and may be seated in any part of the urethra, from the neck of the bladder to the meatus; spasmodic stricture is generally of short duration, lasting from a few minutes to six or eight hours, or longer, and is caused by contractions of the muscular coat of the urethra, and is generally reflex, as from adjacent inflammation ; for instance, a man falls astraddle of a fence or log ; a few hours after he may have spasmodic retention of urine, passing off in a few hours ; the same thing may occur after operation upon the perineum or testicles, and may also follow operations upon the penis itself, as the removal of a redundant prepuce. An acute attack of hemorrhoids is likely to be accompanied with the same trouble, as is also an operation upon them. Pressure, in the female, is a common cause, as the passage of a large head in childbirth in a protracted labor, or packing the vagina for gynæcological purposes, and from wearing an improperly constructed or inserted pessary. The passage of a sound or other instrument, the passage of a calculus through the ureters or urethra, an organic stricture in the anterior portion of the canal, acute gonorrhœa, gleet, excessive venery, indulgences in drinks, especially acid wine, and excessive shock, are all reflex sources of spasmodic strictures.

Spasmodic strictures may be annular, and may involve any or all portions of the urethra.

Digitized by Google

Organic stricture is the result of an effusion of lymph into and surrounding the membranous lining of the urethra, generally submucous, and having a great variety of shapes, ranging from the contraction of a single muscular fibre to the hardening of the entire penis, and, when followed by fistulas, even the perineum may become gristly. The most common form of organic stricture is the annular or thread-like, which can be well simulated by tying a string around an intestine. This form of stricture is frequently multiple; sometimes as many as eight, rarely ten, are found in one urethra, and may be associated with all or any of the other forms, and may also have a calibre so small as to be impenetrable, or so large and resilient as to escape detection; the threads or bands forming the annular stricture may sometimes split on one side, thus forming two strictures on one side of the canal, and only one on the other.

Stricture, or contraction of the meatus, is generally described with the annular form, and is very common. Contraction of the orifice, after amputation of portions of the penis, is a rare form that might be classed under the same head.

Longitudinal strictures sometimes exist, running like a septum along nearly the entire canal; this form may be twisted upon itself, forming a spiral stricture, or there may be two or more ridges making the urethra seem to be corrugated, as seen after several internal urethrotomies. Certain strictures have been described by some authors and are very seldom met with, and are generally seated near the meatus, stretching sometimes more than half-way across the urethra, and consisting of a thin membranous fold.

Nodular strictures are also recorded, being really epitheliomas, and vary with the changes of that disease, especially their shape.

Gummatous strictures are also seldom found, are very irregular, and may be found in any part of the urethra.

Sarcomas sometimes seem to be stricture-like constructions, and are rare.

Chancroidal strictures, due to chancres at the meatus or within the urethra, occasionally occur.

Traumatic strictures may be of any imaginable shape, and in any part of the urethra.

Organic strictures are generally made up of scar tissue, hard and gristly, but may be quite soft and flexible (the resilient form).

The most common cause of organic stricture is masturbation; the next, irritating injections, and the next most common, in my judgment, as in the order mentioned below. Injuries, surgical operations, chancre (primary syphilis), passing of calculus, cancer and gumma (secondary syphilis).

I would like to call the attention of the Congress to an invention of mine, in the shape of a bulbous bougie, which can be made extemporaneously by taking a small-sized bougie and melting solder upon it, smoothing it off nicely; and each time it is used either melting a little more upon it or cut a little off with your pocket knife, as the case may require, always smoothing it off nicely with a small piece of emery paper.

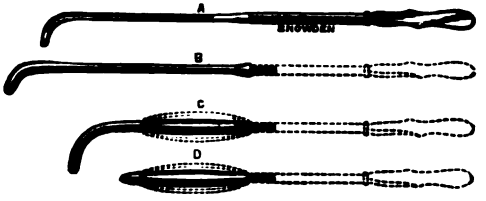
From this idea, I have had constructed, by Mr. Snowden, of Philadelphia, a *pocket bougie*, which is made of steel, nickel-plated, and, as shown in the accompanying cut, consists of ten 2¾-inch bulbs (being only the alternate sizes) and an universal handle. (A) represents a Gouley, or tunneled-sound point; (B) a bladder sound, or stone-searcher point; (C) a curved bougie point, and (D) a straight bougie point. This case, including a few whalebone filiform bougies, contains a complete set of urethral dilating instruments.

This pocket instrument is entirely equivalent to forty separate bougies, for when the curved beak is adjusted, twenty curved bougies can be formed, that will take the place of about six pounds of steel, and can be used for dilating strictures in any portion

Digitized by Google

of the male urethra, and by simply adjusting the straight beak, which weighs just one-quarter of an ounce, twenty straight or meatus bougies are formed, suitable for dilating strictures without disturbing the bladder (in the male), for dilating the female urethra in any part, for stricture, or for dilating the entire female tube, to allow digital cystic exploration for stone or ribbed bladder, and is also an excellent instrument for dilating the *uterus*, either gradually, that is, day unto day, or at once.

Allow me to repeat that this little point, weighing just one-quarter of an ounce, takes the place of twenty bougies, that would weigh several pounds. I have found,



in using my instrument and the ordinary bougie alternately, that patients at once remark the painless, bulbous one. This, I think, is very natural; the meatus, being the most sensitive, and always the smallest part of the urethra, receives the most stretching; by using the ordinary bougie, which increases gradually in size from the beak to the handle, a stricture four inches from the orifice receives less stretching than the meatus, by one or two sizes, while by the use of the bulbous instrument, only the same size passes the meatus, and its passage, being momentary, is nearly painless, and

when the bulb engages the stricture, it may remain in position several minutes without giving much pain, as the small staff near the handle has allowed the meatus to relax. One of the best recommendations of this instrument is its remarkable compactness, as it can readily be carried in the coat pocket and into a patient's house without exciting the curiosity of the neighbors or the rest of the family, and last, but not least, is its cheapness. A full set of regular steel bougies, cased, will cost twenty dollars and take a large case to hold them, and a full set of straight or meatus bougies will cost more than half as much, and need a box more than half as large, making a large, imposing and cumbersome package to carry into a sensitive patient's presence.

Digitized by Google

AN ANOMALOUS MIDDLE THYROID ARTERY, WITH PRESENTATION OF SPECIMEN.

UNE ARTÈRE THYRÖIDE MOYENNE ANORMALE AVEC PRESENTATION DE SPECIMEN.

EINE ABNORME ARTERIA THYREOIDEA MEDIA, MIT DEMONSTRATION DERSELBEN.

BY DR. MAX J. STERN,

Instructor in Laryngology at the Medico-Chirurgical College of Philadelphia.

The preparation that I present for your notice is a dissection, made by myself, at the Philadelphia School of Anatomy, and presented to my friend and teacher, your worthy President, Prof. Pancoast, by whose courtesy I am enabled to bring it before you. The subject was a woman, aged thirty-four, and the cause of her death, according to the certificate, general paralysis.

In the specimen you will please notice the ligature on the left carotid artery; it was cast around it in the injection of the body. Originally, I intended injecting through the aorta, but on reaching the heart, and not finding its situation normal, I cut down on the carotid and injected through it.

The first striking feature that will be noticed is the aorta arising on the left side, 6 cm. from the median line, passing obliquely upward, forward and outward, then arching backward and downward, but at no time touching the median line, the nearest approach being at the commencement of the transverse aorta and it being one and one-half centimetres from it. The left common carotid and subclavian arteries arose normally from the aorta and were normal in their distribution. The slight alteration in the course of the left carotid is due to the handling and cutting of it preparatory to the injection.

The arteria innominata is given off at the commencement of the aorta transversence, passes obliquely upward, then transversely over the trachea, two and a half centimetres above its bifurcation, dividing, immediately after crossing it, into the subclavian and common carotid arteries. The subclavian was throughout normal in its distribution. The common carotid pursued a normal course, bifurcating into the external and internal carotid arteries on a level with the superior border of the thyroid cartilage. The first branch of the external carotid artery, usually the superior thyroid, is the branch to which I wish to call your particular attention. It will be seen that, leaving the external carotid artery immediately above its bifurcation, it pursues first a transverse and then a downward course until reaching the superior border of the thyroid gland, and here dichotomizing into a superior thyroid artery and the anomalous artery, for the demonstration of which the preparation now before you has been made.

The course of this artery is obliquely downward and inward until reaching the crico-thyroidean membrane, then, traversing it, it passes downward and over the isthmus of the thyroid gland, nearing the inferior border of which, it divides and subdivides into minute arteries, which are lost in the substance of the gland. On cutting through the skin, it was found intimately associated with the gland and just peeping between the ribbon muscles directly in the median line.

The significance of such an artery as a complication in the operation of tracheotomy can readily be understood. My friend, Dr. J. Solis Cohen, informed me that, operating in 1865, with the assistance of Dr. D. Hayes Agnew, a frightful hemorrhage was encountered shortly after the skin was incised and while in the deep connective tissue. Neither he nor Dr. Agnew could ever locate the artery that had been divided, although both were positive it was not the artery of Neubauer. Dr. Cohen is strongly of the opinion that it was the one which I have described to you.

Digitized by Google

In examining some sixty or seventy specimens, I have met with three or four like arteries, although, compared with this, they seem simply rudimentary. In my researches into the literature of the subject I can find no mention of such an artery, and believe this the first time it has been described.

SKIN GRAFTING UPON THE CRANIUM.

PEAU GREFFEÉ SUR LE CRÂNE.

HAUTTRANSPLANTATION AUF DEN SCHÄDEL.

BY FREDERICK CHRISTIAN SCHAEFER, M. D.,

Professor of Anatomy of the Chicago Medical College; Member of the American Medical Association.

My object in introducing an old subject is twofold :—

1st. On account of the paucity of statistics of injuries of this nature, to place a case upon record before the profession.

2d. To report results of observations in skin grafting over the bones of the cranium.

Since the subject of skin grafting was first introduced by MM. Reverdin and Frank Hamilton, innumerable and varied trials have been made to close wounds by this procedure, with variable degrees of success, all over the civilized world, but an extensive search of the medical literature gives very meagre information concerning skin grafting upon the cranium.

CASE I.—The patient, a young lady, twenty-four years of age, was at work in a laundry under a revolving shaft, July 9th, 1886. The shaft was between four and five cm. below a heavy beam, and about twenty-five dm. above the floor; it was making 225 revolutions a minute, as computed by the engineer. The girl was said to have reached up for something on the wall, when the wind blew her hair toward the shaft. Instantly she was drawn against the beam and shaft, and fell back to the floor without her scalp. The narrow space between the shaft and beam doubtless saved her life. She then walked upstairs (the accident occurring in a basement), where I found her sitting in a chair, moaning piteously, presenting a most ghastly, heart-rending appearance.

THE WOUND.

The entire skull was laid bare, from the margin of the eyelids to the neck. The nasal bones were uncovered and broken, exposing the superior nasal meatuses. The skin of the eyelids was removed from within three mm. of their edges; the border of the wound was traceable from the lower portion of the left external angular process of the frontal bone and margin of the left upper eyelid, downward and backward below the left ear, which was entirely removed; thence the wound extended across the neck, five cm. below the superior curved line of the occipital bone, and could be traced forward through the lower third of the right auricle to the right external angular process of the frontal bone and margin of the right upper eyelid, across the right lid, nose and the left lid to the point of commencement. The superior border of the left zygomatic process was broken, exposing the cavity within the process. Every vessel and nerve supplying the scalp was torn off, and the pericranium was lost in three places; one spot, corresponding to the centre of the forehead, was oval in contour and measured 5 x 7 cm. The other exposed surfaces of the bones were located on either side of the calvarium, on the corresponding parietal and temporal bones; were elliptical, eight cm. long,

Digitized by Google

six cm. wide. The neck flap of the wound fell away from the muscular structures beneath it, exposing the trapezius muscle almost half-way to the shoulder blade. The trachelo and sterno-mastoid muscles were also uncovered, the latter to the extent of seven cm. The right ear was torn through its lower third, and hung at the side of the

neck by an isthmus of skin less than five mm. in width. The exposed surface of the wound measured 40 cm. in length from before backward, and 34 cm. in width near its central portion. The sutures of the cranium were distinctly seen at several places. There were only a few muscular fibres left upon the head—a portion of the temporal

muscles on either side, and the levator palpebræ muscle in the orbits. Hemorrhage was profuse; the temporal, occipital and posterior auricular arteries had to be tied.

Treatment.—I came to the patient within three-fourths of an hour after the accident occurred. Controlled hemorrhage, examined the scalp, wrapped the head up in car-

Digitized by Google

bolized gauze. The scalp was thoroughly mangled and covered with fragments of hair on its inner surface, which had to be removed. In order to save time, I sent for my friend and neighbor, Dr. J. D. Skeer, to assist in the immediate treatment of the case. Dipped the scalp in warm carbolized water, picked off the broken hairs from its inner surface, and cut off the hair from the outer surface, while Dr. Skeer prepared the

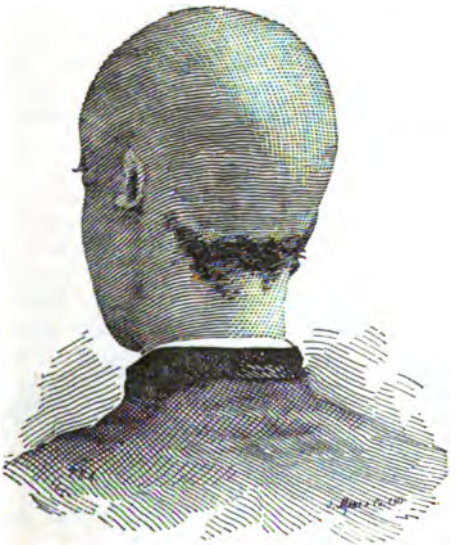
head for the reception of the scalp. Together we freshened the edges of the wound, stitched the scalp back by continuous suture, and sewed the right ear in its position. Applied iodoform over the entire suture, and coiled small, narrow sacks filled with warm sand about the head to keep the scalp warm. After three days, uncovered the

head and cut through the scalp in several places to let out the pus which had accumulated beneath it. On the seventh day removed all dressings. The scalp was now permeated with gases of decomposition, and baked firmly against the calvarium in many places. Removed it. The right ear was warm and joined at the sutures. During the first three weeks we had little hope for the patient's life. Her pulse varied from 90 to

Digitized by Google

130 per minute, while the temperature fluctuated between 100° and 104° F. Was delirious about three weeks, having lucid intervals. Her face was swollen beyond recognition. She was unable to lie down; had to be propped up with pillows. After

the second week her neck became stiff, the muscles were rigid, neck turning to one side. To overcome this tendency, had the bed turned so that it became necessary for



the patient to move her neck in the opposite direction. Also had her exercise the neck by rotating and bending it. The optic nerves were extremely sensitive. Vision weak

Digitized by Google

at $\frac{4}{20}$. She complained of noises in both ears. Hearing distance, right ear, $\frac{4}{40}$; left ear watch C. During the fourth week she left her bed and walked about the room. Dr. Skeer now withdrew from the case, as it was unnecessary for two physicians to

attend her. Six weeks from the day the accident occurred, I concluded that skin grafting might be most promising of satisfactory results. Suppuration was very free at this time, and the patient was too feeble to permit of any treatment requiring extensive

Digitized by Google

operative procedure. I therefore, on the 15th day of August, took thirteen epidermal grafts from the flexor side of her sister's arm and placed them upon the patient's head four mm. apart. By way of experiment, four of them were placed upon the centre of the cranium in direct contact with the granulation tissue; the others near the edge of the wound upon the eyelids where there was the least suppuration. The head at this time was covered with granulations from the pericranium, excepting in places where the periosteum was lost; these were gradually covering in also, from their margins. The grafts were held in place with fish skin, and the bandages were permitted to remain in position forty-eight hours, when the four grafts upon the vertex presented a pink color, and five of the grafts over the eyelids were washed away by the pus. I then attached eighteen grafts, eight upon the top and ten near the margin, as before. After another forty-eight hours all on the top of the head were pink, the four previously planted were spreading. Those near the margin of the wound remained also, but did not look as good. The constant accumulation of pus interfered with the growth of the marginal grafts. I was therefore lured on to place the greater portion of them near the vertex in semicircles, coming forward to meet those which were growing over the lids.

October 12th, my notes say there is a piece of transparent skin, 13 cm. long and 7 cm. wide, extending from the eyelids backward, besides twenty pieces, varying in size from a ten-cent piece to a quarter of a dollar, upon the sides. On the 15th of October noticed that the new skin began to liquefy in spots. October 17th, 10 o'clock, A.M., a copious hemorrhage occurred from one of the venous channels between the tables of the right parietal bone; the blood ran forward and to the left side, beneath the front half of the new skin, which was thus destroyed. A few days later the other half was perforated by many small ulcers. In ten days more it also disappeared. Over two months' work was destroyed in as many weeks. The amount lost would be represented in one piece by an oval measuring 15 cm. by 11 cm., to produce which 915 grafts were required.

Although somewhat discouraged by this disaster, we were not dismayed, but endeavored to retrieve our loss by vigorous grafting. Having noticed that the skin derived from the epidermis, chiefly, was very thin, immovable and short lived, I now resorted to dermal grafting, picking up each time, also, a few fibres of subcutaneous connective tissue. Suppuration having greatly diminished, all of them were placed about the circumference of the wound. About 65 per cent. lived. During this month, November, two irregular pieces of bone, measuring 3 x 2 cm., were removed from the outer table, where the pericranium was gone. December 1st there was an irregular band of skin around the entire head, six cm. wide in front and four cm., wide at the back and sides. The last two series of grafts did not thrive. Up to this time heterodermal grafts only were brought into requisition, from her sister and three brothers. As the patient was now able to walk out of doors, she gained in strength, and the thought occurred to us (Dr. Skeer, who called to see her at this time, and myself) that tissue taken from her own person might do better than the heterodermal material. On the 24th of November commenced taking grafts from her left arm. The rapidity of the growth of her own grafts was simply marvelous. For example: one day thirty were placed in a double row, set three mm. apart. In thirty-six hours they put on a pink color. Three days later they were joined in one piece. The connecting parts were slightly depressed and, of course, quite thin. By the eighteenth day a few hair-like vessels were detected in the new tissue.

Eight times after our first misfortune, numerous ulcers started at different places. At one time there were eighteen of them. They were evidently caused by the contraction of the subcutaneous tissue, whereby the fine vessels were broken.

(Many of the ulcers healed of themselves, but the larger ones were assisted by autoplastic grafts. 4500 grafts were planted, of which the patient supplied 1800.)

Digitized by Google

May 1st (10th month), there remained uncovered four spots, each a trifle larger than a silver dollar. I expected to complete the scalp in four weeks, when suddenly her strength gave out. She took a severe cold, suppurative inflammation of the left middle ear followed, the mastoid process became very tender, and a dozen new ulcers appeared; two large ones, each about twelve cm. long and from one to three cm. wide. The combined area of all might have been represented by a circle sixteen cm. in diameter. The patient was almost disheartened; she slept only three hours of a night for three weeks; her head ached, sight grew dim, and I feared meningitis was setting in. Gave her opiates and tonics. After three weeks' depression she reacted, when grafting was resumed. Occasional discouragements have been met with since, but now she feels good; says the head and eyes seem as strong as before the accident occurred. Her hearing has greatly improved also. There remain uncovered only two spots, each measuring less than a quarter of a dollar. One over the left ear, the other on back of head, which will doubtless heal over in two or three weeks.

NOTES.

From the beginning the entire wound was treated antiseptically with carbolic acid, iodoform, and corrosive sublimate. It was dressed, at the suggestion of my colleague, with strips of carbolized gauze prepared with 2½ per cent. solution. The strips were 6 cm. wide, 30 to 35 cm. long. A layer of them, which came in direct contact with the granulations, was anointed with vaseline, so that it could be readily removed; then a series of strips was superimposed upon each other; next a piece of lint covered on one side with vaseline was placed over these and external to the lint; a roller bandage made of cheese cloth was wrapped about the head. This formed a convenient and serviceable dressing. The lint and strips were easily removed. The interstices of the gauze, by capillary attraction, drew the pus from the wound's surface; the lint prevented too rapid evaporation, whereby the dressings were prevented from baking against the wound. The outer bandage retained the other parts in place. Thinking the lint heated the head, by way of experiment it was left off; but each time the dressings were stuck against the cranium, so that they were removed with difficulty, therefore continued using it.

At first the grafts were held in place by means of a piece of fish skin, which after thirty-six hours began to liquefy and could be removed with a sponge. Repeated experiments caused me to discard this. The fish skin would break down, commingle with the pus and help to macerate the grafts. Next tried McIntosh dressing. This heated the tissue too much, and the grafts were often found floating in the pus beneath it. Having observed that the pus always clung to the gauze dressing; that, as a rule, the thicker the layer of gauze used the less pus remained upon the wound's surface, I concluded to place a pad of oiled carbolized gauze upon the grafts instead of the rubber or fish skin. This worked remarkably well. Six or eight thicknesses of gauze, extending two or three cm. beyond the grafts in all directions, were applied. This had the desired effect of absorbing the pus, reducing the heat to the minimum, while the grafts generally remained in place.

The grafts usually assumed a pinkish appearance in forty-eight hours. Occasionally, a series of the autoplastic grafts, when the patient felt exceptionally good, would be almost pink in twenty-four hours. Many of them, however, gave no evidence of being alive until after seventy-two hours. Several times grafts which hung to the dressings, covered with pus, after forty-eight hours, being replaced, took hold and lived.

The progress of the growth of the new skin was similar to the description of M. Reverdin. A narrow (one mm.) zone of hyaline tissue, somewhat depressed, surrounded the grafts in ninety-six hours; this grew with variable degrees of rapidity until the grafts were joined, then it grew thicker and thicker. After three weeks, sometimes a

Digitized by Google

few days earlier or later, fine vessels were traceable in this new tissue; these increased in size and number until the skin was literally a network of vessels. As they continued developing, it was noticed that the larger ones were directed upward. Later, some of them assumed very nearly the position of the normal arteries, like the temporal, occipital, etc. Now many of them have a bluish color, being evidently veins.

The grafts were derived from her sister, three brothers and herself, aged respectively 17, 19, 27, 30 and 24 years. They varied in size, averaging about 4 x 3 mm., the central portion consisting of dermis with subcutaneous connective tissue; the marginal portion comprised epidermis only. Simply a fold of the skin was lifted with sclerotica forceps and hastily snipped with iris scissors. When removed their under surfaces curved inward. They were then spread out and *laid in direct contact with the granulations*. The peculiar form of the grafts seemed to exert a favorable influence upon them. Their inward curvature caused them to cling closely to the granulations and an immediate suction action took place. The patient said she could feel them draw as soon as they rested upon the head. I have no doubt that the empty vessels in the grafts expedited this process. Many times grafts 6 x 7 cm. were tried without success. Tissue with hair was also applied; but the hair bulbs died. 10 to 100 grafts were planted at a sitting. Most of the time 20 to 30 only were used. Usually two days intervened between the graftings. Frequently, however, several weeks' rest had to be given the patient, on account of her weak condition.

Anatomy.—All of the skin from the margin of the superior eyelids upward presents the characteristics of the normal skin. It is pliable, soft and movable. The lower half can be pinched up in folds. It imparts a velvety sensation to the fingers when touched. Furrows with pores opening into them are plainly seen with the hand glass. Follicles are abundantly present. Her head sweats freely, and much of the new skin feels to the patient like the skin in other portions of her body. The eyelids are somewhat retracted, less, however, than they were four months ago. The younger portion of the new tissue appears quite red; myriads of small vessels can be seen in it. As the skin grows older the smaller vessels gradually disappear from view, others appear larger; the integument assumes a more normal appearance as to color and consistency. It also becomes more movable, owing to the development of subcutaneous connective tissue. One cannot see where the new skin begins upon the eyelids. The lids close perfectly, so that the lady sleeps with closed eyes.

STATISTICS.

Our works on Surgery give no statistics, although at different times such accidents have occurred in the manufacturing States of our country. After having written many letters to physicians for information concerning cases which came under their observation or treatment, I received responses from two only. I have imperfect reports to give of six cases:—

CASE I.—A girl, nineteen years of age, lost her scalp, fourteen years ago, in the Elgin Watch Factory, Elgin, Ill. Dr. E. Powell, of Chicago, informed me that the wound extended across the forehead above the eyebrows, and that her ears were not touched. Skin grafting was tried repeatedly in this case, with no results. Was told by a surgeon who saw her more recently that one of her eyes was lost since by exposure, occasioned by retraction of the eyelids.

CASE II.—Gross' "Surgery," last edition, makes mention of a young lady who lost her scalp in a factory in Philadelphia, in 1869. He simply says the wound cicatrized over.

CASE III.—Reported in the "Surgical History of the War," Part I, p. 315. A conductor on the Union Pacific Railroad was scalped by the Sioux Indians, near Cheyenne, in 1869. Wound, as indicated by the illustration given, was elliptical; measured about

Digitized by Google

10 x 8 cm. A portion of the outer table of the cranium was removed. The wound healed over.

CASE IV.—Reported by her employer, Mr. E. F. Smith. Miss Hattie Thomas, at Union City, Conn., March 29th, 1871, in a button factory, placed her head under a swift-running shaft to pick up a button, when her hair was caught by the shaft and her scalp taken off from the nape of her neck *to her eyebrows*. Her physician was Dr. S. C. Bartlett, now deceased. He cleaned the scalp and placed it upon the head about two hours after it was torn off, but it did not stay. Then commenced skin planting. Had the head almost covered twice. Each time it was lost. A third effort proved successful in healing the wound, so that she was able to work after two years.

CASE V.—Miss Neuman, Bristol, Conn. Had her scalp torn off by her hair catching in the machinery in a mill where she was at work during the past winter. The entire scalp was gone, an eyelid and an ear slightly torn. She was treated by Drs. Wilson and W. E. Way, of Bristol. Result of treatment not given. The *Hartford Times*, of last July, contained an advertisement for volunteers to furnish skin for grafting.

CASE VI.—Reported by Dr. W. W. Horton, of Unionville, Conn. "June 12th, 1885, Rosa Flynn had her entire scalp torn off by her hair catching in the shafting of a paper mill, in Unionville, Conn. The scalp was torn from the back of the neck around, just above each ear to the nose, taking the eyebrows. There was also a flap torn down the right side of the face nearly to the mouth. . . . I treated her for one month, then sent her to the Hartford Hospital, where skin grafting was tried. Both human and rabbit skin was used. It was a failure. Lids are retracted. The wound is still open. Hemorrhage occurs from the capillaries when her head gets on a lower level than her body. Expect the wound will cicatrize over in another year."

It will be noticed that in none of these cases was the wound as extensive as in the one under consideration.

DEDUCTIONS FROM EXPERIENCE IN THIS CASE.

I. When grafts are set within five mm. of each other, a stronger skin is produced than when they are placed further apart.

II. A graft will grow to many times its own dimensions, so that a circular graft three mm. in diameter will form a piece of skin eighteen mm. wide, but the skin will not be as strong or of as good quality as when the grafts are closer together.

III. Dermal grafts with a few fibres of connective tissue form a smooth, soft, pliable, movable skin, presenting the characteristics of normal integument.

IV. Epidermis placed upon the cranium produces only a thin film, which is immovable, and ulcerates readily.

V. Autodermal grafts are more reliable than heterodermal grafts, providing the subject is healthy.

VI. Grafts from a person near the patient's own age do better than from one much older or younger.

VII. With dermal grafts the follicles are preserved, whereby the patient is made more comfortable, the skin produced is more natural, hence more durable.

VIII. The most reliable skin is formed by grafting about the circumference of the wound, say one or two rows, producing a narrow strip one cm. wide, and waiting until the blood vessels are well developed in it, then plant another series close to the last ones. By this plan, contraction in the marginal tissue takes place in advance of the subsequent graftings, diminishing the liability for the ulcerative process to occur by reason of such contraction.

IX. The new skin is fed first by the tissue upon which it rests, probably by osmosis, but depends for its permanent sustenance upon the vessels growing into it from the

Digitized by Google

circumferential tissue. This fact is thoroughly established in my mind, as it was frequently demonstrated by the death of patches of new skin removed from the marginal tissue. Time and material can therefore be saved by constantly grafting from the circumference in *large wounds.*

X. All things considered, grafts will grow better in a new wound than in an old one, and the more recent the wound the more certain are they to thrive. In the present case, during the last three months, notwithstanding the improved physical condition of the patient, only twenty per cent. of the autodermal grafts took, while six months ago eighty-five per cent. could be depended upon.

THE RETRO-CALCANEAN BURSÆ IN REFERENCE TO AMPUTATIONS THROUGH THE ANKLE JOINT.

LES BOURSES SÉTRO CALCANÉENNES PAR RAPPORT À L'AMPUTATION DANS L'ARTICULATION TIBIO TARSIENNE.

DIE BURSÄ RETRO-CALCANEÄ MIT RÜCKSICHT AUF AMPUTATION DURCH DAS FUSSGELENK.

BY JOSEPH N. DICKSON, A. M., M. D.,
Pittsburgh, Pa.

MR. PRESIDENT AND GENTLEMEN :—Various methods have been proposed in performing this very important amputation; viz., the circular incision; the long dorsal flap (Baudin's); the lateral flaps; the single lateral flap (Roux, Seiddillote, Mackenzie); the inferior posterior or heel flap (Syme, 1843); and various modifications of Prof. Syme's method.

I propose to set before you the plan of operation as detailed by Dr. W. R. Hamilton, of Pittsburgh, Pa., in 1881, in a paper entitled "A New Method of Performing the Amputation of the Ankle-joint." He describes his method thus :—

Amputation at the ankle-joint, as heretofore performed, has been difficult in execution and frequently followed by unfavorable results. It is an operation frequently required here (Pittsburgh), on account of injury, a larger proportion of our population being engaged in railroad service, mining and mechanical occupations dangerous to life and limb, than in any other part of this country.

A new method of performing this operation is given as applicable to cases of injury, admitting the ordinary regular operation, but which may be varied as cases of injury or disease may require.

It is as follows: The foot being held at right angles to the leg, the point of a large scalpel should be introduced immediately below the tip of either malleolus, and an incision made through the soft parts directly across the sole and terminating at the top of the opposite malleolus. The ends of this incision should be joined by another directly across the front of the ankle joint. All these incisions should be made down to the bones by the first application of the knife. The ankle joint should now be opened by dividing the ligaments and the dissection of the flap commenced from above downward. The thinness, softness and flexibility of the tissues at this part facilitate the operation. The dissection at the sides of the os calcis should be kept in advance of that at the posterior part, which enables the attachment of the tendo-Achillis to be easily divided. The separation of the flap from the plantar surface of the os calcis then becomes the least troublesome part of the operation. The dissection should be

Digitized by Google

carried so close to the osseous surface as to include the periosteum. No vessel should be divided after the first incisions, except the nutrient vessels of the bone, as every touch of the knife can be plainly seen by the operator. The soft tissues, together with the periosteum, should be separated from the outer side of each malleolus to about one-third of an inch beyond its base, and the malleoli then sawn off obliquely. The saw should be applied on the articular surface of the bone and the direction of the cut be upward and outward. The articular surface of the tibia should be left uninjured. The anterior tibial artery may now be seen and easily secured by torsion. The Esmarch bandage should be suddenly removed and the limb elevated. The external plantar artery, which may be found near the middle of the anterior border of the flap, may require attention, and as it is desirable to avoid the use of ligatures in this operation, the artery may be caught and twisted, but as this is difficult on account of the tissues which surround it, a needle, carrying a silver wire, should be passed from the outside of the flap through it, alongside of the artery and returned on its opposite side, thus including it in a loop which may be tightened. This loop may be removed in twenty-four hours.

If the lines of incision have been made as directed, when the flap is pressed firmly against the bones it will be found to fit admirably, and the hard integument of the flap will be pressed against the cut edge of the skin on the front of the leg, which will be found drawn above the plane of the articular surface of the tibia, and the end of the stump will be covered by the firm cushion of skin and other tissues from over the os calcis.

While the flap is held firmly in position, silver wire sutures should be inserted about three-quarters of an inch apart, after which a strip of adhesive plaster half an inch wide and sixteen inches long should be applied transversely between each suture, and its free ends so attached to the leg above as to press the flaps firmly against the bones. The skin of the flap and leg should be placed in close apposition, and as the integument of the flap is much thicker than that of the leg, the sutures should be introduced deeper in the latter than in the former, so as to approximate the edges of the true skin in each. A compress, secured by a bandage, applied firmly over the end of the stump, so as to compress the flap against the end of the bones, and leave no cavity for the accumulation of blood, etc., completes the dressing.

At the end of five or six days the sutures can be withdrawn without disturbing the adhesive plasters, if everything is progressing favorably.

In favorable cases, absolute "healing by immediate union" may be expected in the greater part of the wound, and "healing by adhesive union" in the remainder, without suppuration within the flap.

In the preceding part of this article the term "flap" has not been applied to the tissues above the line of incision in front of the ankle, which is usually drawn up above the plane of the articulation of the tibia. The term "flap" has only been applied to the tissues dissected from the foot. When the incision is carried across the sole of the foot, as directed by Prof. Syme, the dissection of the flap is exceedingly difficult. Owing to the thickness and unyielding character of the tissues, it cannot be pushed back without considerable force and wrenching, and owing to the os calcis, the plantar surface of which rises rapidly toward the anterior extremity, the blade of the scalpel cannot be kept parallel with the osseous surface, and, in the language of Lister, "It becomes a most troublesome task to turn back the integument over the prominence of the heel, the knife being thrust the operator knows not where. The subcutaneous tissue on which the skin depends for its nourishment is punctured and scored, and perhaps the point of the instrument appears occasionally through the skin itself, while the flap is subject to violent wrenching in the effort to draw it back over the bony projection."

It is entirely foreign to the purpose of the author to detract any credit from Prof.

Digitized by Google

Syme for his original operation, or to lessen the credit due any of the many surgeons who have offered modifications of this operation. Nor do we claim priority for this method in all its details. The method of dissection was first pointed out by Dr. W. R. Hamilton, in an article read before the Pennsylvania State Medical Society, in 1875. The manner of dressing has been developed since that date.

Others have recommended the removal of the malleoli obliquely without disturbing the articular surface. Should the articular surface of the tibia be damaged, either by disease or injury, it should be removed, although this has the objection that the end of the stump, after removal of the articular surface, resembles the large end of the frustrum of a cone, which renders the stump tender until it is rounded off by absorption.

William Stokes, in "Heath's Dictionary of Practical Surgery," recommends the use of Von Langenbeck's periosteal raspatory for detaching the periosteum and other tissues from the os calcis. Some objections have been raised against this method, on account of the possibility of spicula of bone being developed from the periosteum in the flap, but this has never occurred in any case we have observed. Macleod, of Glasgow, and J. Bell, of Edinburgh, preserve the periosteal covering whenever it is possible to do so.

It will be seen that the operation as described above differs in many important details from that of the original operation as described by Prof. Syme. The malleoli are removed obliquely, but the articular surface is left intact, forming a concave surface, which is filled up by the tissues of the flap and the retro-calcanean bursæ, which will be described later on. The advantages of sawing off the malleoli obliquely are obvious, as it permits of the adaptation of a lighter and better artificial foot. We are informed by J. W. Thompson, of the Artificial Limb Manufacturing Company, of Pittsburgh, Pa., that the stumps after this operation are the very best that his firm have any knowledge of, as all the patients are able to walk without apparent lameness. Some are even able to dance. He also told us of a man who works at his trade as a stone mason, in a shop where his fellow-workmen do not know that he has an artificial foot.

The lines of incision in this amputation also differ from Syme's, as the plantar incision is carried further forward, making a longer plantar flap, while the dorsal incision is carried further upward, so that when the stump has healed the cicatrix is higher in the limb, and, consequently, less liable to irritation than in Syme's operation.

As regards the circulation in this long plantar flap, J. A. Wyeth, of New York, has proved, by a large number of dissections, that the principal blood supply is derived from the calcanean branches of the external plantar artery. Hence, the propriety of a long plantar flap.

On the other hand, Erichsen, Lister and Spruce do not consider the division of the posterior tibial artery an accident of serious moment. In one case operated upon in the Western Pennsylvania Hospital, with the assistance of Dr. W. R. Hamilton, we found the posterior tibial artery badly damaged, as well as the soft tissues surrounding it. This case did equally as well as others.

Perhaps the most important step of all is the preservation of the retro-calcanean bursa. This bursa is thus decribed by Nancrede: "Its anatomy differs somewhat from that of other bursæ, in that cartilage forms part of two of its walls, viz.: the anterior and posterior, for the vertical portion of the os calcis beneath the tendo-Achillis, and the portion of the tendon that faces that part of the bone are both coated with a thin layer of cartilage. This bursa is small and heart-shaped, with its apex upward, thus forming two pouches, one on either side of the tendon. These lateral pouches have a few bridles of connective tissue traversing them, although not of enough bulk to form even partial partitions. A vascular and fatty fold is usually found in each pouch."

The preservation of this bursa is an important consideration, for several reasons: it fills up the concavity of the articular surface of the tibia, thus rounding the stump; it

Digitized by Google

also acts as a cushion, deadening shock and permitting mobility of the integument over the stump. By preserving this bursa intact, the danger of bursitis with consequent inflammatory exudation and suppuration is obviated.

The ankle-joint amputation with the bursa retained is analogous to amputation at the knee joint, where the bursa is also retained.

The method of dressing the stump is very important. The pressure on the heel portion should be continued all the time the sutures are being placed and the dressing and bandages applied.

As regards the statistics of this operation, fifty-two cases have been collected from the practice of various Pittsburgh surgeons; of these there were—

	15	cases operated on by Dr. McCann.
	17	" " " Dr. Hamilton.
	1	" " " Dr. Lemoyne.
	4	" " " Dr. Murdock.
	6	" " " Dr. King.
	1	" " " Dr. Haworth.
	1	" " " Dr. Foster.
	1	" " " Dr. Scott.
	1	" " " Dr. Buchanan.
	5	" " " Dr. Dickson.
Total,	52	

Of these fifty-two cases only three died, two from internal injuries and the third from gangrene.

Ten of the above cases were double amputations of both lower extremities, and two were double amputations where an upper and lower limb were amputated.

The results that follow this amputation have been more satisfactory than any amputation in any other part of the body.

THE DISPOSAL OF DISSECTING-ROOM OFFAL.

LA DISPOSITION DES ABATS DE LA CHAMBRE DE DISSECTION.

FORTSCHAFFUNG DER ABFÄLLE AUS DEM SEKTIONSSAALE.

BY H. C. BOENNING,

Of Philadelphia, Pa.

A question of some importance to those concerned in practical anatomical work is, What shall be done with the refuse and offal of our dissecting rooms? As the result of some inquiry, I have ascertained that our medical colleges and other institutions where the cadaver is in use dispose of their offal in different ways, and generally one of the following, and in this order of sequence; by burial, by storage in vaults or wells, by cremation and by alkalies; but in addition other methods are employed by some; thus, in one answer to a letter of inquiry I read, "We mince our waste and throw it into the sewer." Another writes: "We get away with the stuff," meaning the refuse, "by putting it where the buzzards will clean the bones;" and one charitable institution informs me that they "box the offal and send it to Rahjamundy," which place I have learned is a mission station in India. Such suggestions are original, somewhat startling, perhaps, but on the whole not sufficiently practical. Buzzards, for instance,

Digitized by Google

seldom pass Mason and Dixon's line, and the expressage to India is not a romance. Burial seems to be the favorite plan. But the cost of burial, except in rare instances, is considerable; thus, to bury the waste at the Philadelphia School of Anatomy for the session of '85 and '86, it cost close upon two hundred dollars. But aside from the element of cost, burial should be discountenanced, because, to avoid the expense of frequent handling and hauling the foul and offensive offal, it is allowed to accumulate for a week, a month, six months, and in some instances, a year or longer. Such storage means decomposition, fermentation and the evolution of gases and vapors favorable to the culture and development of all manner of disease germs. Where, however, as in the case of some institutions, it is possible to send away the dissecting-room offal several times a week, *in coffins*, at the expense of the city, I think the plan is most excellent. As to vaults and wells: Twelve or thirteen years ago, when the old University of Pennsylvania buildings were torn down, workmen discovered a number of wells into which the waste from the dissecting-room had been thrown. Some of the wells had been bricked over, and altogether they held the refuse accumulated in that institution for perhaps fifty years. As soon as the laborers broke into the wells and discovered the remains of possibly their ancestors, a howl went up that reached all over the country, and the University men were dubbed ghouls, whereas they are all clever gentlemen, and all of them friends of mine. Vaults and wells are largely used, but they are so expensive, if properly constructed, and so offensive if poorly built, that I am surprised to learn that so many institutions have provided them for the deposit of their dissecting and surgical room refuse. As to alkalies: At least three institutions in this country destroy their offal by alkalies. One uses lime, the others potash and a refuse material or by-product, the result of the manufacture of the cyanide of potassium. These alkalies, I am informed, are intimately mixed with the waste and allowed to stand, when, in one instance (I now quote from a letter), "it stands until liquid, or nearly so, when it is poured into the river," alluding to one of the great water highways of the west. This plan is undoubtedly cheap, but again involves storage and frequent handling, as well as the formation of noxious gases. Cremation stands third upon the list, and is undoubtedly and beyond all controversy the best way to destroy the mouldy and maggoty remains of our rooms. But to build a crematory means the outlay of a large sum of money; it means the accumulation and storage of a large quantity of refuse; it means the outlay of a number of dollars, and twelve hours to heat the furnaces; it means untold annoyances, for cracks will occur, the gases formed during cremation will escape and appeal to the olfactories of the people in the vicinity, and, in general, it means a period of unhappiness, that is dreaded alike by the faculty, the college and its neighbors. Such, at least, is the common experience of our institutions provided with huge and expensive brick-built crematories.

Among the correspondence I received is a letter from a western college, in which the writer states that their refuse is stored in a well or vault until spring or summer, when it is gradually burned in a large stove. Let me narrate my experience in this line at the Philadelphia School of Anatomy:—

I was located at the corner of Tenth and Arch streets, the very centre of the city, and in the course of time every one within a radius of half a mile knew where the dissecting room was located, and when Mr. Jones, a quarter of a mile away, one day, burned an old pair of gum boots, every family residing within the circuit stated sent a representative to my room, with orders to investigate our doings. This was of common occurrence, and I may say that every mal-odorous condition of the atmosphere in that vicinity brought the people to my doors. I cannot deny that occasionally my janitor did feed the furnaces with sundry scraps, always, of course, provoking complaints and the visits of the Board of Health, but latterly the complaints became decidedly less frequent, although more refuse was burned, and I can only account for the falling off of the "complaints" on

Digitized by Google

the assumption that noses, like palates, are susceptible to a vast deal of cultivation, and I believe, gentlemen, that if I had remained in that neighborhood long enough rents would have gone up—*sure*. About six months ago a member of the Franklin Institute called my attention to a patent furnace which, to use his vigorous language, would "burn up the very devil himself," and upon investigation, I found this furnace was constructed, theoretically, in such a manner as to secure absolute and perfect combustion, and I reasoned that a furnace that would burn wet sawdust and feathers without evolving smoke or odor, as was claimed, would burn the waste anatomical material which collected in my rooms, and without offence to any one. I wrote to the inventor, Mr. Gregory, and at my suggestion he built a furnace of convenient size, which in the course of time was placed in the lower dissecting room. I had about 800 pounds of refuse, mouldy, stinking, wet stuff, and after a fire was started Mr. Gregory had the furnace packed with the worst smelling and foulest of the offal, which was placed in strata alternating with sawdust and breeze, which is a waste dust or dirt from the coke works. As the dripping mass became heated a destructive distillation began, and gradually the consumption proceeded, shortly increasing at such a rate that I calculated the capacity of the furnace at not less than 500 pounds of offal a day. During all this time the magazine of the furnace was filled with dense smoke, but on top of the house, close to the chimney, neither smoke nor odor was perceived, and although we burned all the offal at my rooms, and sent to the Jefferson College for 300 pounds more, so as to fully and thoroughly test the furnace, no one in the neighborhood perceived the slightest odor or smoke, and we ourselves, conscious of the process in progress, could not perceive the faintest odor suggestive of the burning of putrid flesh. So much in proof of the practical application of the Gregory furnace to the uses of the dissecting room.

One of my many visitors, during the experiments, said to me: "The only objection to this furnace that I can imagine is the cost of the fuel it must take." "Yes," I answered, "fuel *is* a costly article, but in this furnace, so far, we have used none, nothing but coke-yard dirt and sawdust," the cost of which for cremating eleven hundred pounds of offal, including wood and kindling, was just sixty cents. Now, let me describe this furnace and the principles of its construction, but premise what I have further to state by saying that I am in nowise interested in this furnace, except in the cause of science, and that I am not paid in any manner for bringing the invention to your notice.

The great merit of this furnace is the only reason why I speak of it, and I predict that in a few years it will be used largely, not only to cremate dissecting-room offal, but also garbage, and, indeed, in a thousand different ways, in the arts.

The furnace consists of two cases, one inside the other, and an air space between. Just as when we take two boxes, the one smaller than the other, and place the smaller within the larger, we have between them an air space. The fire is built in the inside box, and heats the air in the air space to an intense degree. At the back part of the furnace the inside box has a horizontal slot running the entire width of the furnace, so that the heated air can escape from the air space and mingle freely with the smoke and gases as they are about to pass out of the furnace. As this intensely heated air comes in contact with the smoke, etc., it at once combines with the unconsumed carbon, giving an absolutely colorless flame, free from odor, proving, practically, that perfect combustion has occurred. In the experiments at the Philadelphia School of Anatomy, Mr. Gregory effected an arrangement by which the interior of the furnace could be seen, and the process of cremation watched, and what, therefore, I have described is not fancy, but fact. Of course, I have not given you the details of this furnace. These you can ascertain by sending to the Patent Office for specification No. 228,061, dated May 25th, 1880. The entire principle of the furnace may be stated to be the supply of

Digitized by Google

superheated air to the partially consumed products or distillations formed during the combustion. The heat developed is prodigious—sufficient to melt ordinary fire brick, which, I believe liquefies at about 5000° F. A word concerning the ashes left in the pit of the furnace: They were carefully collected, and did not quite fill an ordinary wooden bucket. I am certain that if the draft of the furnace had been opened and the fire pushed, even less ashes would have remained. As the furnace can be readily used for heating purposes, the waste, as it is formed, day by day, can be gradually consumed, and from a strictly economic point of view, serves the purposes of a fuel. These are the advantages, gentlemen, I experienced in the use of this furnace: The destruction of my offal as soon as it was formed, and without annoyance to any one; avoidance of expense of storage, handling and hauling; hence, an absence of gases and exhalations, which ordinarily are formed, and which menace the public health; and, lastly, the economy of this plan, which need not be further enlarged upon; and yet, gentlemen, with all these advantages—all these, I say—the successful management of a school of anatomy is not a poem.

FROZEN SECTIONS OF MALE PELVIS, SHOWING THE RELATIONS OF THE PERITONEUM TO THE ABDOMINAL WALL, BLADDER AND RECTUM, WITH SPECIAL REFERENCE TO SUPRA-PUBIC CYSTOTOMY.

SECTIONS GELÉES DU BASSIN MÂLE MONTRANT LES RAPPORTS DU PERITOINE A LA PAROI ABDOMINALE, A LA VESSIE, ET AU RECTUM, AVEC UNE ALLUSION SPECIALE A LA CYSTOTOMIE SUS-PUBIENNE.

GEFRORENE DURCHSCHNITTE DES MÄNNLICHEN BECKENS, DIE BEZIEHUNGEN DES BAUCHFELLS ZU BAUCHWAND, BLASE UND MASTDARM DARLEGEND, MIT SPECIELLER HINSICHT AUF DEN HOHEN BLASENSCHNITT.

BY ALBERT B. STRONG A.M., M.D.,

Demonstrater of Anatomy, Rush Medical College, Chicago, Ill.

Mr. President and Gentlemen:—In performing supra-pubic cystotomy, to cut into the bladder without endangering the peritoneum or doing undue violence to the connective tissue back of the pubes, is the important part of the operation. Shall this be done with the bladder empty, or partially filled, either alone or in connection with a distended rectum, are questions upon which all are not yet fully agreed.

For the purpose of investigating these mooted points, I have undertaken, with the assistance of medical student John S. Perckhan a series of experiments upon a number of cadavers.

The results are shown in these drawings, which are made from photographs of frozen sections of the adult male.

They represent the parts as enlarged to three times their natural size.

Plate No. I represents a subject forty years of age, weighing 100 pounds. It shows the normal relation of the parts, the bladder and rectum being empty. Here we see the anterior peritoneal reflection is 1½ inches below the crest of the symphysis pubis.

Plate No. II represents a section of a subject forty years of age, weighing 140 pounds. It shows the bladder as empty and the rectum distended with 15 fluid ounces of plaster-of-Paris solution. Here we see the anterior peritoneal reflection is one inch below the crest of the symphysis pubis.

Digitized by Google

PLATE I.—Peritoneal Reflection.

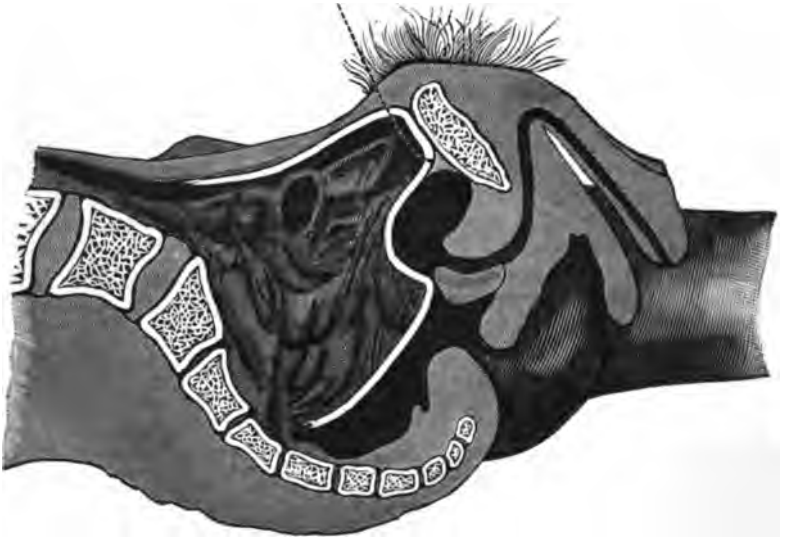
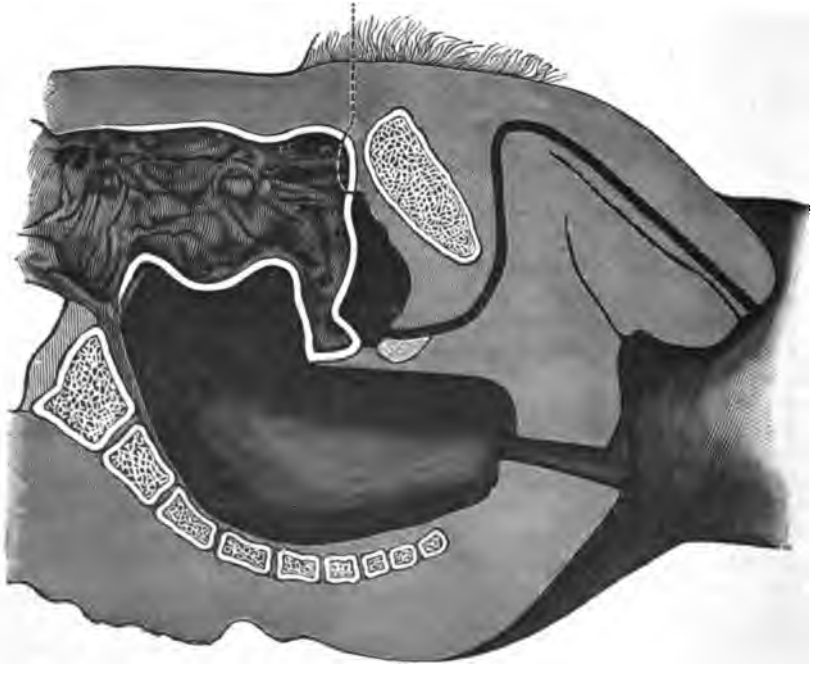


PLATE II.—Peritoneal Reflection.



Digitized by Google

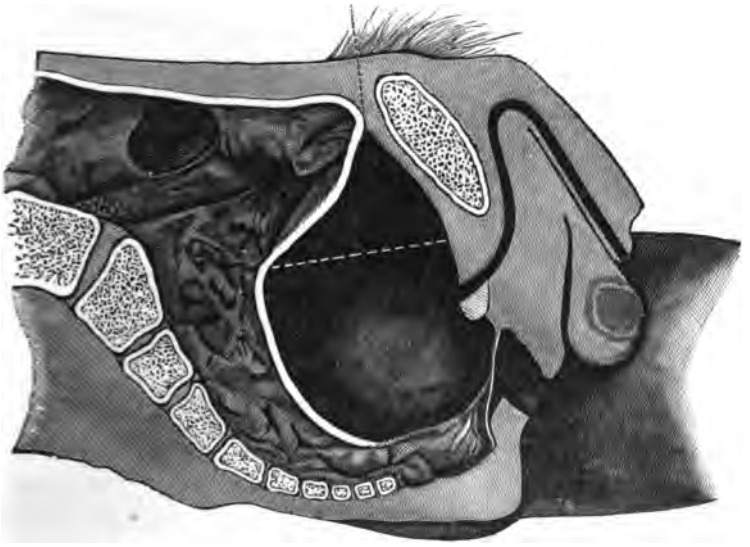
Plate No. III represents a section of a subject thirty-five years of age, weighing 110 pounds. It shows the rectum as empty and the bladder distended with 10 fluid ounces of plaster-of-Paris solution. Here we see the anterior peritoneal reflection is one-fourth of an inch below the crest of the symphysis pubis.

Plate No. IV represents a section of a subject forty years of age, weighing 140 pounds. It shows the rectum as containing 15 and the bladder 10 fluid ounces of plaster-of-Paris solution. Here we see the anterior peritoneal reflection is seven-eighths of an inch above the crest of the symphysis pubis.

You observe here this white line running across the bladder, in plates III and IV. This indicates the height to which the plaster-of-Paris solution of 10 ounces came. The space above contained air, which I think must have been there before the bladders were injected.

Looking at these plates with reference to the relation the anterior peritoneal reflec-

PLATE III.—Peritoneal Reflection.



tion bears to the crest of the symphysis pubis, we observe in the normal condition of the parts, when the bladder and rectum are empty, it is 1½ inches below; when the bladder is empty and the rectum distended with 15 ounces, it is one inch below; when the rectum is empty and the bladder distended with 10 ounces, it is one-fourth of an inch below; when the rectum is distended with 15 and the bladder with 10 ounces, it is seven-eighths of an inch above.

Numerous experiments made upon many other subjects, treated without freezing, as is seen in each of these drawings, gives the same general results.

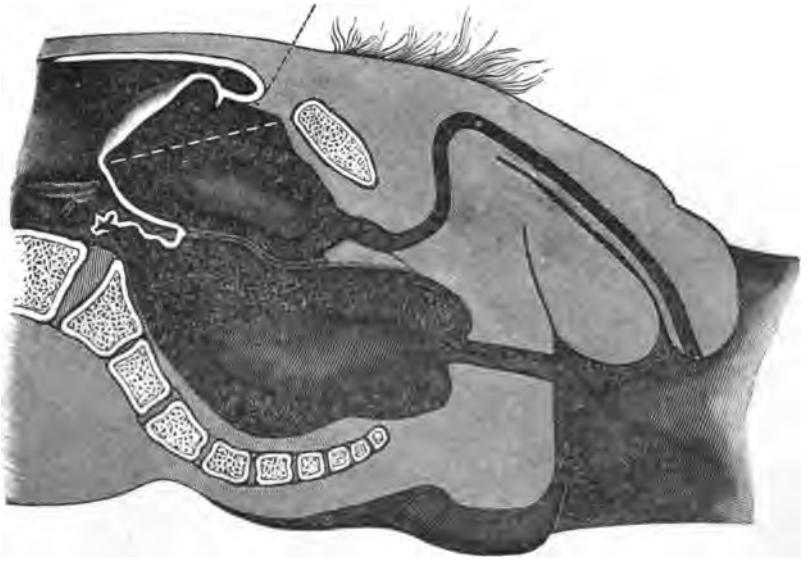
After preparing each cadaver, two lines of investigations were followed. In the first, a small opening was made into the abdominal cavity and the parts examined by touch and sight. In the second, supra-pubic cystotomy was first made and the parts explored by abdominal section afterward.

To give in detail one case under the first head will be sufficient to show in what manner our investigations were conducted.

Digitized by Google

CASE No. IV.—Male, fifty years of age, weighing 150 pounds, dead 24 hours; bladder empty, rectum washed out, rubber bag placed in the bowel and gradually distended with 12 fluid ounces of water. Made a cut into the abdominal cavity in the median line below the navel, just large enough to admit the index finger. The anterior peritoneal reflection was found to be at the crest of the symphysis pubis. The rectum was only moderately distended, filling up the floor and the posterior wall of the pelvis to within an inch of the promontory of the sacrum. Next, 12 fluid ounces of water was injected into the bladder, which caused it to rise upward nearly to the base of the sacrum, and forward against the abdominal wall, dragging the peritoneum with it to a height of three-quarters of an inch above the crest of the symphysis pubis.

PLATE IV.—Peritoneal Reflection.



Next the water was let out of the bladder, which immediately settled down until the peritoneum was even with the crest.

Then the water in the rectal bag was increased from 12 to 18, then to 24 fluid ounces. In neither case was the anterior peritoneal fold elevated.

All the fluid was contained in the rectum proper. It did not ascend to a higher point than one inch below the promontory of the sacrum.

Next enlarged the abdominal opening and inspected the parts. Rectum very much distended; no rupture of its fibres.

The following table gives a brief summary of this series of cases :—

Digitized by Google

TABLE NO. 1.

Subject.	Sex.	Weight.	No. of Fluid Ounces in Rectal Bag.	No. of Fluid Ounces in Bladder.	Relation of Anterior Peritoneal Reflection to the Crest of Symphysis Pubis, in inches or fractions thereof.	Relation of Upper Dilated End of Rectum to the Promontory of the Sacrum, by inch measurement.
1	F.	112	12	10	¼ below.	
"			22	10	Even.	2 below.
"			22	22	¼ above.	2 below.
2	M.	160	12	10	Even.	
"			12	14	2 above.	
"			0	Air.	3 above.	
3	M.	220	15	10	4 above.	
"			15	16	6 above.	Even.
"			18	16	6 above.	2 above.
4	M.	150	12	0	Even.	1 below.
"			12	12	¾ above.	1 below.
"			18	0	Even.	1 below.
"			24	0	Even.	1 below.
5	M.	150	6	0	½ below.	
"			12	0	½ below.	Even.
"			18	0	½ below.	2 above.
6	M.	150	0	12	¼ below.	
"			6	12	Even.	
"			12	12	Even.	1 below.
"			16	12	¼ above	Even.
"			16	15	½ above.	
"			16	21	2 above.	
"			0	Air.	2¼ above.	
"			10	10		1 above.
"			10	6	¾ above.	
"			10	10	1¾ above.	
7	F.	190	0	Air.	1¼ above.	
8	F.	130	12	12	1¼ above.	½ below.
"			0	Air.	2¼ above.	
"			0	12	1½ above.	
"			0	18		
21	M.	125	12	0		Even.
"			15	0		2 above.
"			15	12	2½ above.	
"			23	0		3 above; burst rectum.

In the second series of cases, supra-pubic cystotomy was first made and the parts examined afterward.

The following case is offered in illustration: August 9th; subject No. 13; male, of good muscular development, forty years of age, weighing 160 pounds; bladder empty, rectum washed out. Injected 12 fluid ounces of water into the rectal bag and ten ounces into the bladder; made supra-pubic cystotomy without seeing or injuring the peritoneum, also without disturbing the space between the bladder and pubes.

The manner of operating was as follows: Carried an incision from the crest of the pubes upward in the median line three inches; exposed the linea alba; cut between the pyramidales muscles; exposed the transversalis fascia; made a small nick into this opposite to the crest of the pubes, enlarged it upward an inch, put the end of the index finger into the wound and hooked up the underlying fatty tissue; a little scraping below the finger revealed the bluish appearing bladder. The apex of the bladder could be felt two and a half inches above the crest; the bladder was only moderately distended; easily picked it up with common dissecting forceps, made an incision, an inch in length, into the anterior surface; there escaped about one ounce of fluid; left index finger hooked into the bladder and felt its apex one inch above the upper end of the cut into it.

The following table is a brief summary of the cases operated upon without seeing or injuring the peritoneum :—

Digitized by Google

TABLE NO. 2.

Subject.	Sex.	Age.	Weight.	Fluid Ounces in Rectal Bag.	Fluid Ounces in Bladder.	Relation of the Apex of the Bladder to the Crest of the Pubes, in inches and fractions thereof.
9	M.	45	160	0	10	½ below.
10	M.	28	150	0	0	½ below.
11	M.	50	175	0	Air; 10ʒ.	2½ above.
12	F.	30	110	14	8	1½ above.
13	M.	40	160	12	10	2½ above.
14	M.	25	160	0	Air; 10ʒ.	2 above.
15	M.	35	125	12	6	2½ above.
22	M.	40	150	12	8	1 above.
23	F.	50	110	15	12	3 above.
24	M.	40	120	12	8	3 above.

In the last table we give the relation of the apex of the bladder to the crest of the pubes, in the first the relation of the anterior peritoneal reflection to the same point. It must be remembered that the apex of the bladder and the peritoneal reflection are not always on the same level. In moderate distention of the bladder, the peritoneum passes down on its anterior surface a variable distance, depending upon the degree of distention and the firmness with which the bladder is pressed against the abdominal wall.

When the apex of the bladder is two inches above the crest and the bladder pressed firmly against the abdominal wall, the peritoneal reflection is not more than three-quarters of an inch above the same point.

With this degree of distention, however, the peritoneum is so loosely attached that it and its subjacent fatty layer can easily be slipped up without doing violence to either.

Rectal Bag.

The rectal bag used and which you see here, is made of pure rubber with rather thin walls. When empty it is six inches in length by two and a half inches in width. One end is somewhat rounded; to the other is attached, as part of it, a rubber tube twelve inches long, for convenience in filling.

To introduce the bag, it is first oiled, then bunched together and folded over the index finger, when it is easily introduced just within the internal sphincter. A measured quantity of water is gradually and slowly injected with a bulb syringe.

As the result of these investigations, the following conclusions are submitted:—

I. In the normal condition, the bladder and rectum being empty, the apex of the bladder and the peritoneal reflection are a little above the arch of the pubes.

II. In moderate distention of the bladder, the anterior peritoneal reflection is below the apex; with the same degree of distention, and the bladder pressed against the abdominal wall, the peritoneum ascends higher.

III. Supra-pubic cystotomy can be most easily and safely performed when the bladder is lifted from the pelvic floor and moderately distended against the abdominal wall.

Digitized by Google

IV. Distention of the rectum alone elevates the base of the empty bladder, but does not raise materially the vesico-abdominal fold of peritoneum.

V. Distention of the bladder alone, in thin subjects particularly, requires relatively a greater amount of fluid to elevate the peritoneal reflection. The bladder is not crowded against the abdominal wall, but rather falls away from it.

VI. Moderate distention of both rectum and bladder meets the indication the best; from ten to fifteen fluid ounces in the rectal bag, and eight to ten in the bladder is generally enough, and seems to be a safe quantity to use.

VII. Dilatation of the rectum first and the bladder afterward lifts the peritoneal reflection the highest.

VIII. The object of the rectal bag is to elevate the distended bladder and press it against the abdominal wall, and so crowd up the peritoneum.

IX. To meet this indication, the gut should be dilated from the anus to near the promontory of the sacrum.

X. The rectal bag should be sausage-shaped, of thin rubber, rather than pyriform and thick. For the thinness enables it to follow up the gut, and the shape makes uniform pressure throughout the entire length.

XI. In a very fleshy subject with a flabby and pendulous belly, the bladder is relatively freely movable. In such a case it will easily rise out of the pelvis against the abdominal wall, when alone distended with a moderate quantity of water; the rectal bag may be safely dispensed with.

XII. Air injected into the bladder of a subject lifts the bladder and its peritoneal reflection out of the pelvis better than water. (See Cases II, VII, XI and XIV.)

XIII. In one case the gut was ruptured opposite the promontory of the sacrum, with twenty-three ounces in the rectal bag (Case XXI); in no case was the bladder ruptured.

XIV. In the cases reported, twenty-four in number, an average of fourteen ounces in the rectal bag and twelve in the bladder elevated the anterior peritoneal reflection on an average of one and a half inches above the crest of the pubes; the apex of the bladder was one inch higher.

In twenty-five cases reported by Dr. Helmuth in his monograph on supra-pubic lithotomy, when the bladder alone was injected, the average quantity of water in each was twenty-seven ounces; the peritoneum was lifted two, and the apex of the bladder four inches above the crest of the pubes.

In five of these cases rupture of the bladder occurred with an average of 59 fluid ounces.

EXPERIMENTS.

The first thing done with each subject was to empty the bladder and wash out the rectum. Water was used to distend the gut, by means of the rectal bag; the bladder was directly injected; a bulb syringe was used.

The relation of the anterior peritoneal reflection to the crest of the symphysis pubis is given by inches; also the relations of the upper end of the distended rectum to the promontory of the sacrum is given by the same measurement.

The following are the cases operated on:—

SUBJECT No. I.—June 29th. Female, in good flesh, thirty years of age, weighing 112 pounds.

Experiment No. 1.—First injected 12 ounces into the rectal bag, then 10 ounces into the bladder. The peritoneal reflection was one-quarter of an inch below the crest; bladder only moderately distended.

Experiment No. 2.—The amount of fluid in the bladder remaining the same, *i.e.*, 10 ounces, the fluid in rectal bag was increased from 12 to 22 ounces; the peritoneal reflection was even with the crest; the upper end of the distended rectum was two inches below the promontory of the sacrum.

Digitized by Google

Experiment No. 3.—The amount of fluid in rectal bag remaining the same, *i.e.*, 22 ounces, the fluid in the bladder was increased from 10 to 22 ounces; the peritoneal reflection was one-quarter of an inch above the crest; both bladder and rectum seemed to be distended to their utmost capacity. Neither viscus was ruptured.

Notwithstanding the unsafe quantity of water in the rectal bag and bladder, 22 ounces, the peritoneum was lifted only one-quarter of an inch. I think this is explained when we see that the rectum was not dilated upward beyond a point two inches below the promontory of the sacrum. The bladder bulged backward over the upper end of the rectal bag.

The rectum was not dilated high enough to throw the bladder against the abdominal wall. It is not so much the lifting of the base of the bladder that is required, as throwing the whole bladder forward; a high dilatation of the rectum does this best.

SUBJECT NO. II.—June 29th. Male, negro, well-developed, weighing 160 pounds; dead 20 hours, from typhoid fever; abdomen considerably bloated and tympanitic; not due, however, to post-mortem change.

Experiment No. 1.—First injected ten ounces into the bladder, then twelve ounces into the rectal bag. The peritoneal reflection was even with the crest.

Experiment No. 2.—The fluid in the rectal bag remaining the same, *i. e.*, twelve ounces, the fluid in the bladder was increased from 10 to 14 ounces; peritoneum elevated two inches.

Experiment No. 3.—The bladder and rectum were both emptied, the rectum remaining empty; the bladder was distended moderately with air. Peritoneum elevated three inches.

SUBJECT NO. III.—June 30th. Male, large, very fat, pendulous abdomen, 25 years of age, weighing 220 pounds; dead 24 hours.

Experiment No. 1.—First injected 15 ounces into the rectal bag, then 10 ounces into the bladder. Peritoneum elevated four inches, bladder only slightly distended.

Experiment No. 2.—The quantity of water in the rectal bag remaining the same, *i. e.*, 15 ounces, water in the bladder increased from 10 to 16 ounces. Peritoneum elevated to the naval, bladder only moderately distended, rectum distended to the promontory of the sacrum.

Experiment No. 3.—The water in the bladder remaining the same, *i. e.*, 16 ounces, the water in the rectal bag was increased from 15 to 18 ounces. Rectum dilated to a point two inches above the promontory of the sacrum. Rectum not over distended ; no fibres ruptured.

SUBJECT NO. IV.—July 12th. Male, 50 years of age, weighing 150 pounds ; dead 24 hours.

Experiment No. 1.—Injected 12 ounces into the rectal bag. Peritoneum even with the crest. Rectum moderately distended to one inch below the promontory of the sacrum.

Experiment No. 2.—The rectal bag holding 12 ounces, 12 ounces were injected into the bladder. Peritoneum elevated three-quarters of an inch.

Experiment No. 3.—Bladder emptied; water in rectal bag increased from 12 to 18, then to 24 ounces; rectum very much distended up to a point one inch below the promontory of the sacrum.

SUBJECT NO. V.—July 14th. Male, well developed, forty years of age, weighing 150 pounds ; dead 24 hours.

Experiment No. 1.—Injected first six, then 12, then 18 ounces into rectal bag; peritoneum not materially changed in either case; when 12 ounces were in rectal bag the rectum was dilated to the promonotory of the sacrum; when 18 ounces were in it was dilated to a point two inches beyond.

No experiments were made on the bladder, owing to a stricture of the urethra.

Digitized by Google

SUBJECT No. VI.—July 21st. Male, forty years of age, weighing 150 pounds; dead 36 hours.

Experiment No. 1.—Injected 12 ounces into the bladder; moderately distended; peritoneal reflection one-quarter of an inch below crest.

Experiment No. 2.—12 ounces remaining in the bladder; six ounces were injected into the rectal bag. Peritoneum even with the crest; six ounces more injected into the rectal bag does not materially change the peritoneum; rectum distended to within one inch of the promontory of the sacrum.

Experiment No. 3.—12 ounces remaining in the bladder; the water in the rectal bag was increased from 12 to 16 ounces; rectum distended to the promontory of the sacrum. Peritoneum elevated one-quarter of an inch above crest.

Experiment No. 4.—16 ounces remaining in the rectal bag; water in bladder increased from 12 to 15 ounces. Peritoneum half an inch above crest.

Experiment No. 5.—16 ounces remaining in the rectal bag; water in bladder increased from 15 to 21 ounces. Peritoneum elevated two inches.

Experiment No. 6.—Water let out of the rectal bag; bladder with its 21 ounces sinks into the pelvic cavity. Peritoneum three-quarters of an inch above crest.

Experiment No. 7.—Both bladder and rectum being empty, bladder was moderately distended with air. Peritoneum elevated two and one-quarter inches above crest.

Experiment No. 8.—Rectum and bladder being empty, ten ounces were injected into rectal bag, which distended it to a point one inch below promontory of sacrum. No effect on the peritoneal reflection.

Experiment No. 9.—Ten ounces remaining in the rectum, six ounces were injected into the bladder. Peritoneum elevated three-quarters of an inch above crest; water in bladder increased to ten ounces. Peritoneum elevated one and one-quarter inches.

SUBJECT No. VII.—July 21st. Female, very fleshy and flabby, with pendulous abdomen, fifty years of age, weighing 190 pounds; dead 40 hours.

Experiment No. 1.—Rectum empty; bladder moderately distended with air. Peritoneum one and one-half inches above the crest; on attempting to squeeze the air out of the bladder through a small opening in the abdominal walls, the bladder was ruptured. It had not been over-distended with air.

SUBJECT No. VIII.—July 22d. Female, twenty years of age, 130 pounds weight, in good flesh; accidentally killed 40 hours before.

Experiment No. 1.—Injected 12 ounces into the rectal bag, also same amount into the bladder. Peritoneum elevated one and one-quarter inches above crest. Rectum distended to a point half an inch below promontory of sacrum.

Experiment No. 2.—Bladder and rectum emptied; bladder distended with air till quite resistance was offered to the bulb of a Davidson's syringe. Apex of bladder within two inches of navel. Peritoneum lifted two and one-quarter inches above crest. Bladder not ruptured.

Experiment No. 3.—Bladder and rectum emptied; injected 12 ounces into the bladder. Peritoneum lifted one and one-half inches above crest.

Experiment No. 4.—Rectum empty; 18 ounces injected into the bladder; elevates apex of bladder half way to navel; 30 ounces brings it to within one inch of navel. Bladder very tight, not ruptured at 36 ounces.

SUBJECT No. IX.—July 29th. Male, forty-five years of age, weight 160 pounds; dead 36 hours.

Experiment No. 1.—Rectum empty; bladder distended with ten ounces; made suprapubic cystotomy without seeing or injuring peritoneum. Apex of bladder half an inch below crest; picked up apex of bladder with forceps; when incised there escaped not more than two drachms of fluid. Bladder very loosely distended.

Digitized by Google

SUBJECT No. X.—Aug. 5th. Male, twenty-eight years of age, weighing 150 pounds; 20 hours dead.

Experiment No. 1.—Bladder and rectum being empty, made supra-pubic cystotomy without seeing or injuring peritoneum; apex of bladder half an inch below crest. In operating the cellular tissue in front of the bladder was much more disturbed than if the bladder had been distended.

SUBJECT No. XI.—Aug. 8th. Male, fifty years of age; general anasarca; moderate ascites; weighing 175 pounds; fifteen hours dead.

Experiment No. 1.—Injected into the bladder eight bulbs of air from a Davidson's syringe; moderate distention, equivalent to ten ounces of water; made supra-pubic cystotomy without seeing or injuring the peritoneum; apex of bladder two and a half inches above crest; peritoneum at least one inch above crest. Bladder very loosely distended.

SUBJECT No. XII.—Aug. 9th. Female, thirty years of age, 110 pounds weight; dead 36 hours.

Experiment No. 1.—Injected 14 ounces into rectal bag, also eight ounces into the bladder; made supra-pubic cystotomy without seeing or injuring the peritoneum; apex of the bladder one and a half inches above pubes and against the abdominal wall.

SUBJECT No. XIII.—Aug. 9th.—Male, of good muscular development, forty years of age, weighing 160 pounds.

Experiment No. 1.—Injected 12 ounces into the rectal bag, also ten into the bladder; supra-pubic cystotomy in the usual way, without injuring the parts or seeing the peritoneum; apex of bladder two and a half inches above crest and against the abdominal wall; rectum distended to within one inch of promontory of sacrum.

SUBJECT No. XIV.—Aug. 15th. Male, twenty-five years of age, good muscular development, weighing 160 pounds.

Experiment No. 1.—Eight bulbs of air from a Davidson's syringe distends the rectal bag as much as ten ounces of water. Rectum empty; bladder distended with eight bulbs of air; apex of bladder rose two inches above the crest; made supra-pubic cystotomy without seeing or injuring the peritoneum. The bladder was only moderately distended, and opening it disturbed considerably the cellular tissue. Bladder not held against the abdominal wall.

SUBJECT No. XV.—Aug. 18th. Male, thirty-five years of age, rather small, but well-developed, weighing 125 pounds.

Experiment No. 1.—Injected 12 ounces into rectal bag, and six into the bladder. Made supra-pubic cystotomy without seeing or injuring the peritoneum; apex of bladder two and a half inches above crest and against the abdominal wall; loosely distended; not more than half an ounce of water escaped when it was opened. No disturbance of the cellular tissue back of the pubes.

SUBJECT No. XVI.—July 2d. (See plate No. 1.) Male, thirty-five years of age, weighing 100 pounds; bladder emptied; rectum washed out; put in the refrigerator of Rush Medical College.

July 9th. Removed to the dissecting room, and made a vertical section with a saw; anterior peritoneal reflection one and a half inches below crest.

SUBJECT No. XVII.—July 8th. (See plate No. 3.) Male, thirty-five years of age, 110 pounds; rectum washed out; bladder emptied; injected into the bladder ten fluid ounces of plaster-of-Paris solution and put in the refrigerator.

July 25th. Made section; anterior peritoneal reflection half an inch below crest. Bladder not more than four-fifths full; air chamber above; air must have been there at time of injection.

Digitized by Google

SUBJECT No. XVIII.—July 8th. (See plate No. 4.) Male, forty years of age, 140 pounds weight; bladder emptied; rectum washed out; injected into the rectal bag 15 and into the bladder 10 fluid ounces of plaster-of-Paris solution; put in refrigerator.

July 25th. Made section; anterior peritoneal reflection seven-eighths of an inch above crest of pubes. Air space above level of plaster.

SUBJECT No. XIX.—July 11th. (See plate No. 2). Male, forty years of age, 140 pounds weight. Bladder emptied; rectum washed out; injected 15 ounces of plaster-of-Paris solution into rectal bag; put in refrigerator.

July 25th. Made section; anterior peritoneal reflection one inch below crest.

SUBJECT No. XX.—July 2d. Child, five years of age, 30 pounds weight. Bladder emptied; rectum washed out; put in a refrigerator.

July 25th. Made section; anterior peritoneal reflection one-fourth of an inch below crest of symphysis pubis.

SUBJECT No. XXI.—Aug. 22d. Male, forty years of age, 125 pounds weight.

Experiment No. 1.—Twelve ounces injected into the rectal bag; distended the rectum to promontory of sacrum; water increased to 15 ounces; rectum distended to a point two inches beyond the promontory of the sacrum.

Experiment No. 2.—Fifteen ounces remaining in the rectal bag; six ounces were injected into the bladder; peritoneum even with the crest; water in the bladder increased to 12 ounces; peritoneum elevated two and a half inches.

Experiment No. 3.—Water in bladder let out; water in rectal bag increased from 15 to 23 ounces; rectum distended to a point three inches beyond the promontory of the sacrum; muscular fibres gave way in a longitudinal direction on each side of the medium line of the gut and opposite to the promontory of the sacrum.

SUBJECT No. XXII.—August 24th. Male, forty years of age, 160 pounds weight. Injected 12 ounces into the rectal bag and eight into the bladder; made a cut into the bladder without seeing or injuring the peritoneum. Apex of bladder one inch above crest; rectum distended to the promontory of the sacrum.

In this case had considerable trouble to keep the bag in the rectum, on account of the lax and distended anus. When 6 ounces of water were injected, the bag began to bulge from the anus, and came out altogether when eight ounces were in. Reintroduced the bag and applied a compress to the perineum while twelve ounces were injected, then injected eight ounces into the bladder; when the bladder was exposed it was not distended, was very lax and fell away from the abdominal wall. It was opened with difficulty and considerable more laceration of the connective tissue back of the pubes than was necessary, had the bladder been comfortably filled and pressed against the abdominal wall. In the latter condition, as I have frequently observed, the bladder and abdominal wall are practically inseparable; the bladder lies at the bottom of the wound and bulges into it slightly; when the transversalis fascia is opened absolutely no separation of the connective tissue about the bladder is made in such a case.

Let out the water from the bladder; increased the water in the rectal bag to twenty ounces, when the bag burst; the rent took place close to where the injecting pipe entered the bag; a round piece as large as the little finger nail was completely torn out; was holding a compress against the anus at the time, and there was considerable pressure felt in the syringe. I was trying to burst the gut; with one hand in the abdomen felt the large bowel distending two inches beyond the promontory of the sacrum. The gut was not ruptured.

SUBJECT No. XXIII. August 25th.—Female, fifty years of age, 110 pounds weight; dead five hours; body yet warm.

Injected fifteen ounces into the rectal bag and twelve ounces into the bladder; cut into the bladder without seeing or injuring the peritoneum; apex of the bladder three

Digitized by Google

inches above crest; bladder only moderately distended; lays close against the abdominal wall; drew bladder into wound and opened it; there escaped not an ounce of water, showing the bladder was not much distended or pressed upon by the dilated rectum. Rectum distended to promontory of sacrum and against the arch of the pubes tight enough to prevent the water in the bladder from escaping at the urethra. When the water was let out of the bladder its base was easily explored with the index finger. It was not more than two inches below the cutaneous surface.

Experiment No. 2.—Put the bag in the vagina and injected six ounces into it; this dilated the vagina completely, but would have had no effect on the distended bladder, I think, for the distention did not follow up the hollow of the sacrum; the distention did not pass behind the bladder.

SUBJECT No. XXIV.—August 25th. Male, forty years of age, 120 pounds weight.

Experiment No. 1.—Injected twelve ounces into the rectal bag and eight into the bladder; this caused a slight bulging of the bag from the anus, and the bladder stood up well defined; there was considerable resistance felt in the syringe on filling the bag, but little or none on distending the bladder. Cut into the bladder; its apex was three inches above the crest; bladder lay close to abdominal wall and seemed to be fully distended; on opening the transversalis fascia the bladder bulged into the wound; four ounces gushed out when the bladder was opened, showing the bladder had been tightly compressed; rectum distended to promontory of sacrum.

ANATOMICAL AND SURGICAL CONSIDERATIONS OF THE SITE OF A URINARY FISTULE.

CONSIDÉRATIONS ANATOMIQUES ET CHIRURGIQUES DE L'EMPLACEMENT D'UNE FISTULE URINAIRE.

ANATOMISCH-CHIRURGISCHE BETRACHTUNGEN DES SITZES DER HARNFISTEL.

BY WM. C. WILE, M.D.,

Danbury, Conn.

In presenting the following paper, I lay myself open to unavoidable criticism; in one sense, it should have been brought within the province of surgery, yet it is not the surgeon's but the anatomist's knowledge I seek. These two, that is anatomy and surgery, are so closely allied that one is but the other applied; and when the surgeon's skill fails to bring us to the desired goal, we seek the anatomist, and look for enlightenment at the fountain head. So now, where so many differ, let us see if we can glean a thought to guide us into the proper path, from those who are so familiar with nature's construction. Knowing from experience what these requests portend, I have purposely curtailed my article to its utmost limits, hoping the time taken therefrom will be devoted to the discussion which may ensue, and sincerely hope that this exchange of thought may elaborate lasting results.

The subject that I seek discussion on is the best site for the construction of an artificial urethra when the natural passage way has become impervious through the senile changes wrought in the prostatic surrounding of the canal, or occlusion superinduced by malignant growths. I will illustrate the necessity of such a procedure by relating a characteristic case.

Digitized by Google

In the latter part of 1885 I was called to a man, seventy-seven years old, suffering from retention of urine. He had been an invalid for a long time, his condition being brought about by an enlarged prostate. Despite regular and systematic bougieing, the canal had continued slowly and progressively decreasing in calibre until the night before I saw him, when he could no longer use his catheter. I finally succeeded in introducing my smallest instrument, a number one, and, to my consternation, but the slightest amount of fluid escaped. I palpated the abdomen and found a tumor extending almost to the umbilicus, which unquestionably was the bladder. A few stringy clots could be removed from the urethra, and I was loth to recognize the bladder being filled with coagulated blood. After considerable manipulation, which was negative in result in so far as draining any of the contents was concerned, I resorted to injections of hot water strongly impregnated with pepsin. This succeeded in rendering it soluble, and the bladder was finally emptied. It was the last time an instrument could be passed through the urethra, which eight or ten hours afterward, despite all medicinal treatment, had become absolutely impervious.

The question now presented itself, What shall I do for the best interests of my patient? There was an immediate need to be alleviated, and a futurity to be borne in mind; suffering to be relieved and permanent benefit to be derived. A horsehair, even, could no longer be passed. The viscus was again filled, and suffering was intense; so great was the latter that I feared the patient might fulfill his threats of seeking immunity in death. After much thought, I decided, finally, on perineal section, deeming it best for the patient, despite his almost hopeless condition, and the gravity of the operation in one of such an advanced age, in whom all the signs of advanced senility were already so well marked, and who, among all the other unfavorable symptoms, presented that grave one of atheromatously degenerated arteries. But before me shone the motto: He who risks nothing, will not gain.

On the following day, with the assistance of my friend, Dr. J. J. Berry, of Portsmouth, New Hampshire, I made a perineal section, without the use of a guide, at the left lateral lithotomy site. We cut down until reaching the vesical sphincter; then introducing a dilator, we forcibly stretched the muscle to its utmost limits, hoping thus to gain a temporary paralysis, similar to that attained in stretching of the anal sphincter, allowing the urine to dribble away, and ensuring the greatest amount of rest to the inflamed organ, and hoping that the pressure might exert a benign influence over the hypertrophied gland.

I then made a transverse incision through the urethra, one inch above the superior angle of the wound, and dissected the canal out, from the division to its vesical attachment, dividing the gland and stitching the free end of the divided urethra to the sides of the wound. No untoward results were developed after the operation, which lasted nearly two hours. The patient fully recovered, the healing of the wound lasting about three weeks. The urine was retained in the bladder, and could only be emptied on the introduction of a silver female catheter; the voluntary evacuation of the vesicle had passed entirely from will control. The man died just about one year subsequent to the operation, from pneumonia.

The interference we practiced has caused me much thought. Many deem the operation inexpedient, owing to its gravity. But is the operation not less grave than the disease? Can suffering be more intense than that superinduced by such an overdistended organ and its concomitant inflammatory state? Puncture or aspiration promise no lasting help. Were the procedure many times more dangerous than it is, I venture to say there would not be a single one of these poor sufferers who would not eagerly grasp after the doubtful hope of ulterior relief from their intense agony. When we reflect upon the enormous number who have, and will have, enlarged prostates, we can gain some idea of the relative importance of radical procedures. Yet,

Digitized by Google

prevalent as the affection is, so divided are opinions as to its relief. Now, cannot the anatomist come to our aid, and say, This shall be the way, for it is the nearest approach we can attain to the normal condition?

I cannot but believe that we can yet arrive at a mode of operating in which the patient could obtain voluntary control over the contents of the bladder. The futility of this, if a section should be made in the normal state, can, of course, be understood; but where hypertrophy of the gland has for a long time existed, we know what an enormous development the muscular coat attains. Now, cannot this be turned to account?

Pettigrew, in his exquisite dissections of the muscular fibres of the bladder and prostate, has shown how all the fibres are arranged in a figure-of-8 manner, in a long longitudinal axis. They usually occupy seven strata and are normally imperfect. They become fused with each other, with corresponding or homogeneous fibres, and with fibres which are either superimposed or underlie them. The aggregation of these fibres toward the apex forms the sphincter vesicæ. Some of them are continued into the prostatic urethra, which may be regarded, therefore, as an anterior continuation of the bladder. The fibres of the cervix are traceable into the veru montanum, which he believes to act as a valve to the urethra, falling into the tube and obstructing it when the muscle of the bladder is not acting, and raised up so as to stand erect in the middle line, and allow the flow of the urine when the fibres contract.

Is not this arrangement of the muscular element of the organ analogous to that of the uterus? We all know that in the gravid state of the latter, when the exciting cause is at hand—that is, fœtal maturity—an expulsive force is developed in the organ, which, to not a little extent, can be increased through the agency of the mother's will. Now, in the bladder, where hypertrophy and hyperplasia of the muscular element sometimes exists to the thickness of an inch, cannot the urethra be transplanted, or a new canal formed in such a manner that the control of the vesical contents be under the power of the will, which shall make this increased muscularity subservient to it? Unhappily, it was not attained in my operation, and, so far as I can ascertain, in no other; but I firmly believe that it can yet be accomplished; and I ask of you any aid that you can give me in arriving at this result.

ON THE SURGICAL COLLATERAL BRANCHES OF THE MAIN ARTERIES.

SUR LES BRANCHES COLLATÉRALES CHIRURGIQUES DES PRINCIPALES ARTÈRES.

ÜBER DIE CHIRURGISCHEN COLLATERALEN ÄSTE DER HAUPTARTERIEN

BY EDMOND SOUCHON, M. D.,

Professor of Anatomy and Clinical Surgery, Tulane University of Louisiana, New Orleans, La.

It is a very remarkable anatomical fact that all the main arteries of the human body give off usually but a large collateral branch, which very often presents such importance in operative surgery or in surgical pathology, that I will call it the *surgical branch* of that artery. It is most commonly single, but in some few instances we find a *principal branch* and a smaller *accessory branch*.

The type of such surgical collateral branch is represented by the deep femoral (Profunda).

Digitized by Google

The branch, as a rule, originates from the deep portion of the artery, and is distributed to the deeper parts.

The surgical importance of this collateral branch is not so much because of its size when cut, but because it is mostly the great anastomotic medium by which the blood is carried to the parts beyond the main trunk when this has been ligated or when the circulation has been interrupted through it from any cause. It is, in fact, the *safety branch* of the circulation of the region beyond; it is, indeed, a remarkable provision of nature, which thus insures the nutrition of the distant parts against accidents to the main channel.

A thorough knowledge of these branches is most important in the following instances: 1. To properly manage primary and secondary arterial hemorrhages. 2. In treating primary and recurrent aneurisms by compression and ligature. A lack of the proper knowledge of this surgical branch may be the cause that a ligature for primary or secondary hemorrhages, and for primary or recurrent aneurisms, may be placed improperly and thus fail to cure the trouble; it may also be the cause of serious operations being performed for recurrent aneurisms, especially such as ligature of a still larger artery, or the extirpation of the sac, whereas the ligature of the surgical collateral branch would have effected a cure; worse than that, death has been caused by secondary hemorrhage because the surgeon was ignorant of the important rôle which this surgical collateral branch plays. The files of the journals show many such cases. 3. Also in treating wounds of the large surgical veins, where it sometimes becomes necessary, after ligating the wounded vein, to *ligate also* the main artery *below* the largest collateral, when the limb becomes *swollen and blue* to an alarming extent, thereby diminishing the supply of arterial blood, and equalizing the circulation of the limb. In those cases, if the point of ligation is not properly selected, gangrene may follow as the result of the arterial ligation itself. 4. The same remarks apply to cases where, from some cause or other, a venous hemorrhage cannot be arrested by the ordinary means and we must ligate the corresponding artery *to arrest the bleeding.*

The most important practical points connected with this branch are the following:—

1. The gravity of the *ligation* of the *main artery just above the point of origin of this collateral branch*, since the most important anastomotic channel is shut off also, hence, the danger of gangrene.

2. The great importance of always *placing the ligature* on the *distal side* of this *collateral branch*, unless it is decidedly impossible.

3. The necessity in some cases *to put a ligature around the collateral branch itself*, to arrest secondary hemorrhage, or to overcome the return of pulsations in recurrent aneurisms affecting the main artery.

4. The surgeon should always bear in mind when operating on the main artery of a region or on the surgical collateral branch of this artery, that the *ligature is not safe unless applied at least over one-third of an inch from the point of origin of the surgical collateral branch.* If the ligature is placed on the main artery immediately *above*, near the origin of the collateral artery, the blood passing through the collateral circulation into the surgical branch, and thence into the main trunk, will prevent the formation of a clot in the main artery between the ligature and the origin of the surgical branch, and the result will be secondary hemorrhage. The same result will follow if the ligature is placed on the main artery, beyond the point of origin of the surgical branch, but close to it, since the blood from the main artery into the surgical branch will also prevent the formation of clots. Again we will run great risk of secondary hemorrhage if we *ligate the surgical branch too close to its point of origin*, since the blood passing in a continuous stream will wash out the clots as fast as formed.

We will now review the main arteries of the body and point out the surgical collateral branch connected with each and every one of them. In many cases the point

Digitized by Google

will be seen and recognized at once; in others it may at first seem a little strained, but upon second thought it will be found that the law holds good all over.

(A) ARCH OF AORTA AND DIVISIONS.

1. Truly, the surgical collateral branch of the *arch of the aorta* is the *innominate artery*. It is more frequently the seat of aneurism than either the first portions of the left carotid or the left subclavian; it is by far more accessible than the first portion of the left subclavian; it has been twice ligated *successfully* for aneurism of the second or third portions of the subclavian, whereas, the ligations of the first portion of the left subclavian have all failed.

The accessory surgical branch here is the *right carotid*, and because of this it has to be ligated also in all cases of ligation of the innominate artery, to prevent the return of the blood through the circle of Willis.

2. *The common carotid and the internal carotid* form really but one artery, and its surgical collateral branch is the *external carotid*. All the remarks made above apply with force to this artery. It is of the greatest importance whether the common carotid is ligated or the internal or the external. Any ligation applied to cut off the circulation of the external carotid will not accomplish its object unless one of two important points is attended to most carefully. We may apply the ligature at the bifurcation, but this unnecessarily deprives the brain of needed blood. It is therefore best to ligate the external carotid, but taking care to ligate at the same time the inferior thyroid, the lingual and also the facial if it is near by; otherwise we will have secondary hemorrhage, either because to avoid tying their branches the ligature should be placed too close to the common carotid, or, because if the ligature is applied on the external carotid at the requisite distance, the inferior thyroid or the lingual will be permeated by the current and will prevent the formation of clots. The cases of Dr. Wyeth, of New York show this conclusively.

3. The surgical collateral branch of the *external carotid itself* is the *occipital artery*, on account of its *descending cervical* branch which anastomoses with the ascending cervical branch of the superior intercostal, from the subclavian. This anastomose is second in importance to the vertebral in bringing the blood back into an aneurismal sac of the third portion of the subclavian artery.

4. The surgical collateral branches of the *internal carotid* are the *anterior* and *posterior communicating branches*; although they are beyond the reach of the surgeon they play such an important part in the surgical pathology of primary and secondary hemorrhages and in the return of pulsations in aneurisms, that we must count them among the most important collateral surgical branches of the body.

5. The true surgical collateral branch of the *subclavian artery* is unquestionably the *vertebral artery*, because of its size and because of its free anastomoses through the circle of Willis, with its fellow, by means of the basilar artery, and with the internal carotid through the posterior cerebral and the posterior communicating arteries.

Clinical experience teaches this most beautifully in the now historical case of the man Banks. Secondary hemorrhage having occurred it was arrested and the patient practically cured only after the ligation of the vertebral artery. Surgeons are almost all so fully impressed with this now that ligation of the vertebral should be performed at the same sitting as that of the innominate and of the common carotid if we wish to surely cure an aneurism of the third portion of the subclavian.

Banks was operated on here in 1861, by Dr. A. W. Smith, of this city. This case is so instructive that although well known, I will recall its main features. The aneurism affected the third portion of the right subclavian; it was traumatic; it was of the size of a duck's egg. The innominate and the right carotid were ligated with some effect, but a few days afterward secondary hemorrhage from the distal end occurred, neces-

Digitized by Google

sitating the ligation of the vertebral artery. This practically cured the aneurism, though there still remained there a pulsating spot no larger than a pigeon's egg. The patient remained in this condition for ten years, when having taken to house painting, he used his right arm altogether; this started the tumor to grow again, and he came back to Dr. Smyth at the Charity Hospital. The internal mammary was ligated without any effect. The sac became inflamed, and as it threatened to rupture, it was opened and the cavity plugged tightly; the patient died a few days afterward, of exhaustion from loss of blood. The body was dissected by myself, and my report to Dr. Smyth published in the *New Orleans Medical and Surgical Journal.* It was found that the innominate, the carotid and the vertebral and mammary were reduced to membranous cords; none of the other branches of the subclavian were perceptibly larger, but the perforating branches of the aortic intercostals and the branches of the subscapular were much enlarged.

The internal mammary plays no important part in the surgical pathology of aneurism of the subclavian artery. It was ligated by Dr. Smyth in the case of Banks, and it had no decidedly perceptible effect on the aneurism. I would say as much of the *superior intercostal*, in spite of its ascending branch anastomosing freely with the occipital, as it was not found enlarged upon dissecting the body of Banks.

The same remarks apply to the other branches of the subclavian artery.

6. The surgical collateral branch of the *axillary artery* is the *subscapular.* It was through the anastomoses of this artery with the perforating intercostals from the aorta that the blood was carried back into the sac in the ever-so-instructive case of Banks. These anastomoses were numerous and large. Dr. Smyth and myself had, of course, thought of them as being the channels through which the right arm was nourished, but we had never thought that upon striking the axillary artery the blood would divide and send a *retrograde current back toward the heart* into the aneurism.

Had we ligated the axillary artery *above* the origin of the subscapular artery, between that origin and the sac, it is my firm belief that Banks would have been saved a third time and cured permanently. It is important to remember that it would not be proper in such cases to ligate the *subscapular itself*, nor to ligate the axillary *below* the origin of the subscapular, since either procedure would cut off the most important source of supply to the arm, and would very likely be followed by gangrene.

The posterior circumflex may be considered here as the accessory surgical branch of the axillary.

7. The surgical collateral branch of the *brachial artery* is the *superior deep brachial* (Profunda). Its analogy with the deep femoral is striking.

The accessory branch is the *great anastomotic* which insures the collateral circulation on the inside of the elbow by anastomosing with the recurrent ulnar and interosseous, as the deep brachial insures it on the outside by anastomosing with the recurrent radial.

8. *The surgical collateral branch of the radial artery* is the *recurrent radial.* Its accessory surgical branch is the *volar branch* to the superficial palmar arch.

9. The surgical collateral branch of the *ulnar artery* is the *interosseous*, because of its anastomosis above through its recurrent branch with the great anastomotor, and below with the anterior and posterior carpal arteries from the radial and ulna; it is through this last anastomose mainly that the blood reaches the hand when both the radial and the ulnar have been ligated in their course.

10. The accessory surgical branch of the ulnar artery is the *deep anastomotic* branch to the deep palmar arch.

(B) THORACIC AORTA.

11. The surgical branches of the thoracic aorta are the *intercostal arteries*, because of the important part their anastomoses with the subscapular artery play in the surgical pathology of subclavian aneurisms, as explained above.

Digitized by Google

(C) ABDOMINAL AORTA.

12. Truly the surgical branch of the abdominal aorta is the *cœliac axis*, not so much on account of its size, since the renal arteries are larger, but because of its anastomoses with the superior mesenteric artery.

13. The only other branches which might play a rôle are the *middle sacral* and the *lumbar arteries.*

(D) THE ILIACS AND DIVISIONS.

The arteries of the lower extremities present points which are the analogues of those of the upper extremities.

14. The *common iliac and the external iliac* form really but one artery, as the common and internal carotids. And just as we find that the external carotid is the true surgical collateral branch of the carotids, here we find that the true surgical collateral branch of the *common and external iliacs* is the *internal iliac.* And again, just as we find the occipital and its descending cervical branch to be the accessory surgical branch of the external iliac, we find that the accessory branch is represented by the *ischiatic artery* because of its anastomoses with the branches of the deep femoral.

15. The surgical collateral branch of the *femoral artery* is the *deep femoral or profunda*, on account of its anastomoses above with the ischiatic from the internal iliac, and below with the articular arteries from the popliteal. It is the typical surgical collateral branch. It corresponds to the deep brachial in the upper extremity. The femoral artery possesses an *accessory surgical collateral* branch, which is the *great anastomotic.*

16. *The surgical collateral branch* of the *popliteal artery* is the *anterior tibial*, because of its anastomoses above, through its recurrent branch, with the articular arteries and great anastomotic, and below, through the perforating artery, with the external plantar and posterior tibial and the anterior terminal division of the peroneal. It corresponds to the interosseous artery with its recurrent branch of the forearm, and plays just as important a part in the surgical pathology of the region.

The following case, reported in the *New Orleans Medical and Surgical Journal* (1885), by Dr. David Jamison, then Assistant House Surgeon of the Charity Hospital, demonstrates this conclusively :—

"G. B. Narcisse, age twenty-eight years, was admitted into ward 2, August 28th, 1884. His trouble was a large pulsating tumor on the inner aspect of the right leg, about three inches from the knee joint. He received a gunshot wound in the leg two years before admission. The tumor made its appearance eighteen months after the injury. After careful examination, the diagnosis of 'traumatic aneurism of the anterior or posterior tibial artery' was recorded. On September 6th the man was brought under the influence of chloroform and a ligature placed around the popliteal artery. Pulsation in the tumor immediately ceased ; the wound healed kindly and was completely closed by October 1st. Pulsation, however, returned in the tumor in two weeks after the ligation of the popliteal, and the tumor commenced again enlarging. As the patient suffered a good deal of pain, another effort was made to relieve him. Concluding that the collateral circulation had been established through the posterior tibial, a ligature was placed on this artery beyond the aneurism. No difference was noticed in the tumor. It pulsated as before, and by actual measurement was larger and firmer. One week later his condition had not improved. His pains were sharp and laminating. The lymphatics in the groin were enlarged and the feet œdematous. Large anastomotic vessels were found running into the tumor, which continued slowly to increase. The tumor was now examined by Dr. A. W. Smyth, who said that it was being supplied through the anterior tibial, and on his suggestion this artery was ligated on the distal side. The pulsation in the tumor immediately ceased. For a long time

Digitized by Google

it grew no smaller, but gradually the patient's condition improved, and six months later he was discharged from the hospital entirely cured.

"The instructing feature in this case is the ligation of an artery carrying a retrograde or collateral current of blood to a recurrent aneurism, after the ligation of the artery on the proximal or cardiac side had failed to effect a cure. We believe this is the only instance in which ligature of the branch carrying the collateral circulation to a recurrent aneurism has been successfully practiced."

17. The surgical collateral branch of the posterior tibial is the *peroneal artery*, because of the anastomoses through its anterior terminal branch with the tarsal arteries from the dorsal artery and anterior tibial. It was evidently through this anastomose that the blood found its way into the aneurismal sac in the case of the man Narcisse.

ANATOMICAL POINTS OF VALUE IN DIAGNOSIS AND TREATMENT OF SOME JOINT AFFECTIONS.

POINTS ANATOMIQUES DE VALEUR DANS LE DIAGNOSTIQUE ET LE TRAITEMENT DE QUELQUES AFFECTIONS ARTICULAIRES.

WERTHVOLLE ANATOMISCHE PUNKTE IN DER DIAGNOSE UND BEHANDLUNG EINIGER GELENKKRANKHEITEN.

BY M. STAMM, M. D.,

Fremont, Ohio.

In preparing this paper it was not my object to present anything new, but simply to collect some scattered facts and arrange them in a manner as they may bear upon the diagnosis and treatment of some joint affections. The improvement of late years in our methods of treating chronic inflammatory affections of the joints, is of sufficient interest, on such a momentous occasion, where the medical profession is represented by its best minds, and where the light of their experience may be brought to a focus, to call out a discussion and receive the various opinions as to our best measures. I consider it of value to speak of the most prominent anatomical points which contribute to our diagnosis, and naturally serve as a valuable guide in our treatment. I trust I will not meet with too violent an opposition before this enlightened audience, if I designate them as chronic forms, which by some authors are described as "fungus articuli," by the term "tuberculosis" of the joints. Recent investigations, at least, have accumulated valuable evidence which will tend to strengthen this view; and among modern surgeons there seems to be a growing disposition to adopt this name. I might further state, that I am not ignorant of the fact that there are some other forms of acute and chronic joint affections, as a result of infectious diseases, *i. e.*, measles, scarlatina, rheumatism or syphilis. But, as their history generally enables us to make a correct diagnosis, and as their treatment differs very little from the one to be described in this paper, I did not deem it of sufficient importance to speak of them in a special way here. As each joint presents some anatomical features of its own, which naturally influence our views in regard to diagnosis and treatment, it may be best to treat of each one separately. It was my intention to speak of all the joints of the upper and lower extremities, but as this would exceed the allotted space, I have only chosen the hip, knee and ankle joints.

Digitized by Google

COXITIS, OR TUBERCULOSIS OF THE HIP JOINT.

It is of some practical value to divide this affection into three stages. We notice first a certain fixity of the joint, and in the erect position of the patient the suspected limb has a tendency to slight abduction and flexion at the knee and hip joint, but the feet stand parallel with each other. The gluteal region becomes flattened and the gluteo-femoral crease is lower and not as deep as on the healthy side. We should not forget to test the rigidity or mobility of the joint, at least of some muscles, *i. e.*, the psoas magnus, iliacus internus or the adductors. For this purpose the patient should be placed on the table and laid flat upon his back, care being taken that the whole spine is brought upon the plane, which can be done by lifting both thighs in placing your arm under them. To make sure that the pelvis and trunk are at right angles with each other, we may draw a line from the centre of the sternum over the umbilicus to the centre of the pubis, this is then crossed at a right angle by a line drawn from one anterior superior spinous process of the ilium to the one on the other side. In health the limb can be brought down so that the popliteal region will touch the plane without lifting the spinous process or upsetting the relation of the lines. We are also able to flex the limb to such an extent as to touch the chest with the knee without raising the pelvis; any impediment in this direction should, therefore, excite some suspicion in our mind. In disease we find adduction also very much limited, so that the patient is not able to cross the affected limb over the sound one without moving the pelvis. There is a little more scope in abduction if the limb is slightly flexed and rotated outward. Pain is occasionally felt at the outside of the ilium, sometimes at a spot in front, or the inner aspect of the thigh, a little below the apex of Scarpa's triangle. Oftener, however, we have a pain near the inner condyle of the knee, or over the whole joint. Various explanations as to the cause of this pain have been furnished, of which only the one ascribing it to irritation of a small branch of the obturator nerve in the vicinity of the ligamentum teres, can give us some sound anatomical reasons. A few years ago an explanation was offered by Fick, which, if for no other purpose, deserves notice, at least for its ingenuity and the originality of his observations. He made the deduction that the pain exists in the hip joint, but was erroneously projected to the knee. In asking a number of persons, who had no knowledge of anatomy, as to the joint that was mainly used in walking, he was almost invariably referred to the knee joint. In this way, he thinks, some ground is furnished for locating the pain in this joint. We should not forget, however, that this pain of the knee may also be felt in other affections besides that of the hip joint, as *i. e.*, caries of the lumbar vertebra, inflammation of the sacro-iliac joint, abscess within the pelvis or Scarpa's triangle. It, therefore, has value only in combination with other symptoms. The severe pain, which will startle the patient from his sleep, is undoubtedly due to the sudden contraction of the muscles surrounding the joint, pressing the articular surfaces together.

Second Stage.—At this time we have apparent lengthening of the limb, the foot is everted in the erect position, the knee is bent and held a little in front of the sound leg. We find the lumbar side curved toward the affected side; the pelvis is also lowered on the same side, and this is what simulates the apparent lengthening. If you tell the patient to keep the knees straight and the feet together, he will either bend the knee, or get on the toes of the sound limb, throwing back the diseased side of the pelvis and protruding the buttock. Abduction, flexion and rotation outward of the limb, which are generally a well-marked sign of the second stage, are at times also found in inflammatory processes outside the joint, *i. e.*, in front of the joint, in swelling of the glands or some other periarticular abscess, also abscesses in the region of the psoas or iliac muscles. But we generally have no great difficulty in making a correct diagnosis between these conditions and coxitis, except in cases of psoas or iliac abscess. Abduction and rotation outward in these cases, it is true, do not reach a very high degree, but

Digitized by Google

we also meet cases of coxitis where these abnormal positions are not very pronounced. We may, therefore, be somewhat in doubt at the beginning, especially as long as we have no other signs of vertebral affection. We know of only one means under such circumstances which will assist us in excluding coxitis. In psoitis an insurmountable resistance is only felt in the attempt at straightening the limb, and the movement of the corresponding extremity is only limited in the abnormal position. If you bend the limb further you will find flexion, and within this position of stronger flexion nearly always rotation and lateral movement comparatively free. This examination should, of course, be made gently. Pain is often felt on pressure in front or behind the head of the femur or upon the trochanter. We should also mark the swelling, especially at the front part of the joint, neck of the femur, or at the trochanter. Tumefaction below the great trochanter, to the outside, is not well defined; as its consistency is not different from the rest of the limb, it appears as a simple increase in size. It indicates extensive disease of the neck near the epiphysis. The iliac fossa should also enter into our field of examination. It is best to place patient on his back, or slightly turn him to the sound side, with the knees drawn up and the shoulders supported by a pillow. Standing on the affected side, place your flat hand and fingers inside the iliac spines; press the bowels gently over toward the middle line; after your hand has pushed on deep enough toward the sacrum its palm should be turned outward, and you will get an opportunity to examine the iliac fossa and brim of the pelvis. Examination through the rectum will often detect swelling where the acetabulum is affected. Wasting of the muscles is also well marked, the gluteal muscles are flabby and the fold of the nates is found obliterated.

The Third Stage is the one of adduction or shortening; it is also combined with rotation inward and flexion. Swelling (para-articular) is considerable, especially at the contour of the joint as far as the trochanter, so that its point is not distinctly felt. We find the trochanter, in osteomyelitis or circumscribed necrosis, at times enlarged. If suppuration perforates the iliac bursa, we will have fluctuating swelling at the anterior surface. Not unfrequently we may find perforation above the trochanter minor, at the lower edge of the obturator externus, and matter will sink along the adductor muscles. Fistulæ below the trochanteric region will indicate an osteal process in the trochanter or femoral neck. Perforation may in rare cases occur posteriorly upon the edge of the acetabulum. We there detect a hardness and thickened condition of the ilium, evidence of inflammatory irritation of the periosteum produced by disease of the acetabulum. Of a very grave nature are abscesses caused by perforation of the acetabulum into the pelvis. They may burrow into the small pelvis toward the perineum or posterior part of the thigh down to the knee, at times toward the rectum. Abscesses of the rectum may also have their origin from the lower posterior part of the acetabulum. It is quite difficult to recognize an osteal focus before it breaks through into the joint. A very grateful task it generally is to remove a depot from the head or neck of the femur which will perforate near the great trochanter. We there find an abscess or only a soft spot, or a groove upon the bone; in such a case it is well to follow up the tract with chisel or scoop. Occasionally we may succeed in tracing a focus to the acetabulum and in removing it from there. As an example, circumscribed abscesses may present at the posterior rim of the acetabulum, or a focus in the region of the inferior spinous process may produce an abscess at the outside of the thigh, below Poupart's ligament, with slight coxitic symptoms. Although such centres are not of frequent occurrence, they have repeatedly been removed and the integrity of the joint saved. At such a stage we may have spontaneous luxation of the joint, mostly produced by the filling up of the acetabulum with granulations, and by destruction of the ligamentum teres. Where this real luxation exists, we may suspect an osteal process of the acetabulum. Nélaton's line, which is a line drawn from the anterior superior iliac

Digitized by Google

spine to the most prominent part of the tuber ischii, will enable us to estimate the amount of dislocation. In the normal position the trochanter just touches this line, whereas in dislocation it will either be found above or below it. Bryant's method of examination, which consists in drawing lines horizontally outward from the iliac spines, and from which you then may measure the vertical distances to the trochanters on both sides, may also be employed. The femoral head may be partly or totally absorbed; the acetabulum also enlarged by absorption, so that the trochanter travels upward; in this way we may also have the appearance of dislocation. A very instructive case, reported by Volkmann some years ago, should find mention in this connection, because of its difficult diagnosis. In this case the femur had been affected from the neck down to the knee, and it was bent near the hip in the shape of an ?, so that the ligamentum teres was about two and a third inches below the summit of the trochanter. The limb was considerably shortened, adducted and rotated inward. The joint, under anæsthesia, was slightly movable, with no crepitation, and post-mortem examination found it intact No doubt the femur was in a softened condition at the time when the osteomyelitis had reached its acutest stage, from which resulted its twisted shape. If diagnosis had been possible in time, amputation probably might have saved this patient.

Treatment.—The first thing besides rest is certainly correction of any abnormal position, and in this regard the anatomical points mentioned above should be our special guide. Plaster-of-Paris bandage or extension with weight and pulley are at first our best measures. After a while Taylor's or Thomas' splint, also Hutchinson's plan (high-soled shoe on the sound limb with a pair of crutches) may be used to great advantage. In a few of my cases they have given quite satisfactory results, and other surgeons seem to be equally well pleased. With Hutchinson's method, however, I have found it of advantage to use extension at night, to prevent any return of muscular contraction. Small abscesses may, in the milder forms of coxitis, be treated expectantly; if they, however, get larger, their opening should be advised. An undoubted case of pelvic abscess will necessitate resection to give free exit to the matter, and besides that an incision near the anterior superior spinous process should be added, to clean out the cavity. Multiple abscesses, with fluctuation in front and behind the joint, demand resection, as well as fistulæ, which have no tendency to heal after the abscess has been opened. The experience of some surgeons leads to the opinion that in about half the cases the acetabulum is the starting point of tuberculosis. Abscess or fistulæ of the trochanter should, as already mentioned, be laid open and cleaned out with chisel or spoon; some surgeons praise thermo-cautery very highly. This treatment is also applicable in case of abscess or fistulæ at the edge of the acetabulum. Some localized focus may be found at the upper part of the acetabulum near the anterior inferior spine; it may reveal itself as a cheesy tumor or abscess below Poupart's ligament, a little to the outside. Operations of a similar kind have been performed in this locality and a cure accomplished without interfering with the integrity of the joint. These localized processes are, however, not so frequently found alone, since in most cases they are running along with a deeper destruction of the joint, so that resection of the femur will have to be resorted to. A probable diagnosis of this condition is gained by the pain resulting from movement of the joint, or a slight push or blow upon the straightened extremity, especially where symptoms of coxitis have existed before. Spontaneous luxation leaves very little doubt of osteal coxitis, principally of the cotyloid region. In the majority of cases we have the iliac form of luxation, or then the head is found in front of the acetabulum; even if in these instances no abscess exist, resection will generally be called for. We not unfrequently see neglected cases with severe contracture; as long as they are without abscess or fistula an attempt should be made to reduce it, either with weight or under anæsthetics. Should these means not succeed

Digitized by Google

and unmistakable signs of destruction of the joint exist, resection with the chisel should be given preference to sub-trochanteric osteotomy. It need not be stated that resection is more urgent where fistulæ and abscesses accompany the former condition.

Langenbeck's incision seems to answer best in most of the cases. This is made in a line from the superior anterior spine to the middle of the trochanter, in one stroke down to the bone. In this way the capsule is opened and we get directly down to the head and neck of the femur. After having nipped the cotyloid ligament (limbus cartilagineus), divide the muscles in front of the trochanter with the knife, which is rendered easier by pushing the index finger under them and rotating the thigh outward. For the muscles behind the trochanter, repeat the same act with rotation inward. After some traction and slight adduction of the leg, cut the ligamentum teres and then remove the head and neck below the trochanter. Sayre's method has also a number of advocates. He enters the knife about midway between the anterior superior spinous process and the point of the great trochanter right down to the bone. Keeping it there firmly, he draws it in a curved line to the top of the trochanter between its centre and posterior border, thence forward and inward. Ollier makes a somewhat similar incision; not curved, however, but forming an obtuse angle, which begins four finger-breadths below the crest of the ilium, and the same distance behind the anterior superior spinous process, running down to the summit of the trochanter and from there directly down to the shaft. Neuber makes a curved incision down to the bone, forming a semicircle around the great trochanter. The summit of the trochanter is sawed off obliquely. This, together with the flap, is separated from the femoral neck and the edge of the acetabulum and turned upward. The projecting edge of the acetabulum is then chiseled off, the neck luxated and removed with the saw. After this the femur is placed firmly into the acetabulum, the trochanter fixed with nails to its former point of attachment, and the upper part of the wound is united with sutures, leaving the lower part open, which affords natural drainage. The advantage claimed for this operation seems to be that the diseased parts can be readily inspected and removed with ease. So far not much is known about the functional result. A method described by Koenig a few years ago, and tried in a number of cases, is gaining very much favor with some surgeons. Koenig, after having made an incision similar to Langenbeck's, does not separate the muscles from the trochanter with the knife, but he breaks off with the chisel the anterior and also the posterior part of the trochanter, leaving these parts and also the muscles loosely in contact with the shaft. This procedure will leave a small zone of the trochanter, which is now obliquely chiseled off. This gives better access to the neck and the joint, and we can now get in with a fine saw and separate the neck. Some difficulty may be found in removing the head. This can, however, be facilitated by a scoop-shaped elevator designed by one of Koenig's assistants. Should we suspect mischief in the acetabulum, it will be well to remove a piece of its posterior aspect, so that we can fully oversee the ground and remove whatever is diseased. After the affected bony parts have been removed, the diseased synovialis should share the same fate. A pair of strong scissors, of some length, will be very convenient in removing most of its parts, except, perhaps, a pocket near the small trochanter, which will yield to the sharp spoon. Roser's incision at the anterior side of the joint begins close to the outside of the crural nerve and divides the rectus femoris, sartorius, tensor fascia lata, etc. Luecke and Schede have modified this by making a vertical instead of transverse incision outside the crural nerve, beginning a little below and to the inner side of the anterior superior spine of the ilium and running directly downward. Volkmann considers it a practical way for enlarging fistulæ situated in front of the joint, for the purpose of scooping out the joint with the sharp spoon, or for the extraction of the separated head of the femur, etc. For the purpose of resection it seems to be a less expedient method.

Digitized by Google

KNEE JOINT.

According to some authors the synovialis of this joint seems to be oftener affected than the bony structures. In some rare cases we have what is now called "hydrops tuberculosis," which seems to have almost its exclusive seat at the knee joint. Anatomically, there is no diagnostic difference between this form and a serous synovitis. The natural depressions at the sides of the patella and its ligament are filled in by the exudation in a similar manner as in the latter instance. The swelling will reach about three fingers wide above the patella, and even the bursa supra-genualis, situated under the rectus femoris, becomes filled up. The patella will float as upon a water bed, and on pressure will dip upon the femoral condyles. If there is only a small quantity of fluid present we may compress the joint from above, so as to get the fluid under the patella; on pressure, then, we get the same sign of ballottement. In many cases we have the granulating form of synovitis without suppuration as long as the knee is protected from severe injury. This granulation is more pronounced at the folds of the capsule, near its insertion to the sides of the condyles and patella. Fluctuation is seldom present, and if so, very slight in comparison to the swelling. The contour of the patella, as well as of the condyles, becomes obliterated; the anterior aspect of the joint is globular. The swelling is somewhat characteristic, at least it distinguishes itself from the one caused by serous exudation into the joint. In the latter form the synovial sac is more distended near the upper recessus, whereas in the granulating synovitis the swelling is more in the region of the condyles, at the joint line on both sides of the ligamentum patellæ. There the granulations, as they increase, cause the sub-synovial fat to bulge outward, and, as the swelling gradually lessens toward the upper and lower region of the joint, the knee assumes a spindle shape. Abscesses are at times localized near the upper recessus, and, when found at the anterior margin of the tibia or at the lateral surfaces of the condyles, they point to an osteal focus. Contractures are found not only in a position remaining within physiological limits, but also in the most abnormal attitude, which only destruction of the joint would admit. Complete luxations are very seldom found, more often, however, sub-luxations. Genu valgum is the principal deformity, probably owing to constant pressure of the bony parts upon each other, caused by muscular contractions. If inflammatory signs have appeared before, and the knee is left in a flexed position, it is fair to assume some destruction near the external condyle. Genu varum is very seldom seen, and in case it should occur it may be the result of an osteal affection of the inner condyle. Genu recurvatum may follow an awkward attempt to straighten the diseased joint. Sub-luxation of the tibia backward may take place where the ligaments or articular surfaces, or both together, have suffered some change or disintegration. If a case has not been observed from the beginning, it will be very difficult to say whether we have an osteal or synovial affection before us. A focus under the joint surface of the tibia is not easy of recognition. A focus at the condyles or the anterior surface of the tibia, however, may at times admit of an accurate diagnosis if pain and a doughy swelling at a definite spot of the bone have been felt before symptoms of synovitis have set in. It is somewhat peculiar that ostitis and periostitis of the lower end of the femur occur with such great regularity at the posterior surface between the long flexors of the leg. As to the tibia, a focus is frequently found to burrow into the joint at the anterior portion of the meniscs and the anterior border of the insertion of the capsule.

Treatment.—Immobilization and rest is here, as well as in all other joints, the first indication. Plaster-of-Paris bandage serves, in cases where contractures are absent, or present only in a slight degree, its best purpose. Extension may be employed for a short time where contractures require it; it will change the points of contact and thereby relieve the ulcerated portions of the joint from pressure. Hueter ascribed to it also an antiphlogistic effect, as the tension of the skin and muscles produces a slight

Digitized by Google

compression with some increase of intra-articular pressure. Some authors recommend Thomas' knee splint and crutches in patients old enough to use them. In this appliance the weight is transmitted through the tuber ischii and the foot does not reach the ground. Where the disease is active, it might be best not to correct the deformity from the first, but simply mould the bandage to the present position; the muscular spasms will gradually subside and the limb admit of more and more extension. Should the knee be fixed in a position of flexion with slight displacement backward, extension will be the best method to reduce it. It should be used in the longitudinal axis, but may also be aided by a pulley in a perpendicular line to lift up the displaced head of the tibia. Forcible redressment is only admissible in cases with severe pain or spastic muscular contractions. Cicatrices in the popliteal region with extreme emaciation of the leg may lead us to think that the popliteal artery is embedded in the cicatricial tissue; they should, therefore, serve as a warning that no great force be used. In such cases appliances for extension are preferred by some. Where the displacement backward and outward is considerable, it will be difficult to correct it, on account of the rigidity of the ligamentous structures at the back of the joint, even after the hamstring muscles have been divided. The knife in such cases will have to be resorted to. These measures may, in quite a number of cases, be sufficient to effect a cure. But in spite of all our endeavors, we will find cases which need some more radical mode of procedure. Typical resections in children are generally always followed by bad results, as shortening, and contractures will occur in spite of plaster-of-Paris bandage or other splints. The contractures seem to be due to the peculiar mode of growth, since more bone is deposited in the front portion of the epiphysis than in the popliteal region. In view of this fact it was, therefore, no surprise to see it condemned by a number of surgeons, and it was merely a logical consequence that some would contrive measures to get at the seat of affection without mutilating the joint to any high degree. We may in this connection state with some satisfaction that, in regard to this joint, they have during the last few years accomplished surprising results. The antiseptic period had also in this field of surgery its influence full of blessing. Some authors, as for instance Ollier, speak very highly of the effects of actual cautery, others again of injection of carbolic acid. I am, however, inclined to think that the results recently obtained by partial resections will encourage most all surgeons to imitate this plan of treatment. In children with open circumscribed abscesses we may make an attempt to remove the granulations with the sharp spoon. But in cases where the fistulæ have no tendency to close in a short time, where suppuration will spread or where soft tuberculosis rapidly produces a lax or dangling joint, we should not hesitate to open it by a large incision and remove the diseased synovial sac and osteal foci. In adult patients we should resort even with less reserve to such measures and remove grave fungi with soft consistency thoroughly, at an early time. Volkmann has given this operation the name of arthrectomy, whereas he calls the simple incision into joint arthrotomy. Arthrectomy again is divided by him, according to the amount and anatomical character of the structures removed, into—

1. Partial and total synovial arthrectomy.
2. Partial and total osteal arthrectomy.
3. Osteal and synovial arthrectomy.

To judge by the results so far reported, arthrectomy seems to supersede typical resection in a great majority of cases, and in not a few instances almost an ideal result has been obtained. As the synovial form of tuberculosis primarily predominates in adults, the total extirpation of the capsule seems to meet all the indications. And again in children who are more predisposed to the osteal form, the primary foci are so small that they can be removed without any further destruction of the joint surface. In the graver cases it may be well to follow Volkmann's advice to prepare them for such an operation. Contractures should be removed either by extension or manual

Digitized by Google

redressment and the limb kept in a fixed bandage. A large abscess should be opened and scooped out, afterward closed again and well drained; where septic symptoms are present, disinfection should by no means be neglected. We should then wait for the proper time for arthrectomy, or else the fungous proliferations in the wound, fistulæ or abscesses will repeat themselves. As to the mode of performing this operation, every surgeon seems to have his own favorite plan, and too great importance should not be attached to this, as long as the object of removing all the diseased portions of the joint is fully attained.

Volkmann prefers a transverse cut into the joint; this at once enables him to make a digital examination and to decide whether total extirpation of the capsule, or simple drainage, or scooping out of the affected parts will bring about the wished for result. Should you have decided upon removal of the capsule, you enlarge the incision and divide the patella transversely with the saw. Should the fungous masses reach up to the bursa extensorum and produce thickening of its walls, it might be of advantage to make a flap with its apex reaching above the bursa and its basis a little below the joint line. Esmarch's bandage is used only in exceptional cases, as the acute angle of flexion, and also the hand of the assistant, encircling the thigh firmly above the knee, control the bleeding very nicely. With a number of hooks the soft parts, with the separated halves of the patella, are pulled widely apart, so that the capsule is bulging outward. The entire capsule and also the ligaments are then dissected out from the healthy tissue with knife and scissors. The whole bursa extensorum is also extirpated in that region, a few small incisions made for drainage openings. The apophysis of the femur is cleanly dissected off to the width of three or four fingers, and also the head of the tibia freed from any portion of capsule or semilunar cartilage, so that at the end of the operation only muscles or healthy tissue are visible. Any focus visible at the epiphysis may be scooped out, as it will not interfere with primary union. After all the tissues are removed, the limb should be brought into slight flexion with careful coaptation of the epiphyses. The patella, if it has been divided, should be united with catgut sutures, or the tendon of the quadriceps femoris stitched to the patella, in case a flap operation has been selected. Catgut sutures or steel nails are used where any displacement of the epiphyses is expected.

Some others follow Koenig's plan with great satisfaction, which consists in making a slightly curved incision at the inner side with its convexity backward. Beginning at the tibia near the insertion of the inner margin of the ligamentum patellæ, you follow the convex line with your knife, pushing the internal lateral ligament back; after having reached the upper part of the joint turn toward the middle line, where you readily find the distended synovial sac. This incision is everywhere carried deep into the joint, and vertically it also separates the vastus internus muscle along the course of its fibres. Another shorter incision at the outside of the joint, in front of the external lateral ligament, will assist very much in getting free access to the sac in that region. If circumstances should require, you may nip the ligamentum patellæ at the inside, and even divide the internal as well as the crucial ligaments. Another very ingenious method, which will undoubtedly gain some followers, has been lately reported by Israel, having obtained in two cases, so to say, an ideal result. His plan consists in making a large flap with its convexity downward; then he saws off the tuberosity of the tibia in an oblique direction, so that it remains with the patellar ligament attached to the flap. This gives him ample room to remove the entire capsule in connection with the crucial ligaments and semilunar cartilage. After having inserted the drainage tubes he nails the tuberosity to the tibia. Primary union followed in his cases and patients were early induced to use passive movement of the joint. One patient, about eleven years old, can bend the leg at an angle of 100 degrees, has full strength in its use, without any shortening and with a natural shape of the joint.

Digitized by Google

Helferich has in one case also treated the lateral ligaments in a similar manner; they were chiseled off from the femoral condyles anteriorly and from below, and posteriorly left in contact with the periosteum. Riedinger has a method which is also favorably spoken of by some surgeons. He makes a longitudinal incision over the middle of the patella, including at the same time the lower portion of the quadriceps and the tendon of the patella. After the tissues are divided down to the bone with one stroke, he, in a corresponding line, separates the patella into two symmetrical halves with the saw. By pulling them apart the joint can be inspected with ease, and the extirpation of the capsule and removal of the joint-ends present very little difficulty. Anatomically this method certainly presents some advantages, as the coaptation of the parts after the operation is simple, and immobilization during the process of repair is also readily secured. Moreover, the way the structures are divided, the functional result will not be interfered with in case movement of the joint is an object to be obtained. We may, in not a few cases, encounter some difficulty in removing the affected synovialis from the popliteal space, at least we may experience some uneasy sensation in working in such close proximity to the popliteal artery. A few hints thrown out by Zesas in this respect may, therefore, serve us in good stead. He advises to carefully remove the superficial granulations and then go down directly to the blood vessels and dissect them off all along the granulations. In some very grave cases with extensive granulations we may be obliged to get at the blood vessels from behind, in a manner similar to their ligation.

Typical resections may be carried out by incisions similar to those referred to when we spoke of arthrectomy. The transverse incision with division of the patella is quite in favor with some surgeons. Flaps, with their convexity downward upon the tibia, or upward above the patella, have also their advocates. My experience would lead me to adopt Hahn's method, as in my hands it presented all the advantages pointed out by this surgeon. An incision is made at the inner side, starting from a point as far back as possible at the line of the joint, cutting the tendon of the quadriceps close to the patella and ending at the outside at a corresponding point. The advantages of this method seem to be that the wound heals faster, as it nowhere is in immediate contact with the bone. It also affords good drainage and free access to all the parts of the joint. The patella also is easily kept in its place and hemorrhage is not to be dreaded, as very few blood vessels are cut

ANKLE JOINT.

In fungous inflammation of the ankle joint, and especially where there is a large quantity of exudation, we at first notice in front a soft swelling of the synovialis along the extensor muscles, at the outside between the external malleolus and the extensor digitorum, at the inner side between the internal malleolus and the tendon of the tibialis anticus. Posteriorly, we notice it on both sides of the tendo-Achillis, and it is often well marked here when swelling elsewhere is very slight. It presents a somewhat diffuse appearance as it is kept down by the deeper muscles, and we have here an elastic touch, while in front distinct signs of fluctuation are exhibited. These different prominences will serve us to decide whether the exudation is in the tibio-crural joint, or simply under the tendons, as in the former case pressure in front will increase tension behind. We have still another sign enabling us to determine this question, which is also caused by the exudation distending the tibio-fibular joint. By its internal tension the fibula is pushed outward and gives the ankle a broader appearance. The astragalus can now be easily moved from one side to the other, and in striking it against the malleoli we hear a knocking sound, "bruit du choc malléolaire." (Aubry.)

If swelling is only present at the posterior part of the joint, we may infer trouble in the talo-calcaneal joint. But, as a rule, the swelling of the capsule presents rather a

Digitized by Google

doughy than fluctuating feeling, and gradually diffuses into the peri-synovial tissue, giving the joint a spindle-shaped appearance. In affection of the calcaneo-tarsal joint we have swelling on both sides of the calcaneum, behind and below the malleoli, and the heel looks very broad. In case Chopart's joint is involved, the principal swelling is on both sides of the extensors; at the inner side about the tuberosity of the scaphoid bone, at the outer side over the anterior process of the os calcis. The fungous masses will proliferate toward the plantar side and flatten the foot; at the dorsal side they will spread under the tendons in front of the talo-crural joint. Somewhat characteristic is the form of tuberculosis sequestrum of the tibia in smaller children; there we have thickening of the tibia, with fistula and consecutive affection of the joint. A really typical affection has its origin in the body and neck of the astragalus. Perforations here most frequently appear near the insertion of the synovialis at the talo-crural joint; but they may also penetrate through the cartilage of the astragalus into the tibio-tarsal, talo-calcaneal or scaphoid joint, singly or together. The bones which communicate with the astragalus have also a certain importance in regard to affection of the ankle joint. The scaphoid bone seems to form an exception, as it frequently has been extirpated alone and no tendency to involve the neighboring bones was found. The close connection of the three joints makes it easy for these pathological processes to travel from one joint into the other. We see this especially in tuberculosis of the ankle joint, which easily spreads into the talo-calcaneal joint, and vice versa. Less often it happens that an osteal focus of the astragalus simultaneously involves the upper and lower joint. Should the tibio-tarsal joint be affected at the same time with the talo-navicular, we may, as a rule, assume that the primary tuberculosis has its seat in the astragalus. Abscesses are rather frequent, and they develop at the anterior surface of the joint and spread along the extensor muscles of the leg. In other cases they may burrow under the tendo-Achillis or the flexors behind the malleoli, especially behind the inner one. A large abscess at the anterior surface of the foot, breaking open in the region of the neck of the astragalus, almost invariably points to an osteal focus of the astragalus. The same is true with abscesses over the malleoli; they generally indicate a process located there. We might think this recognition of the exact seat and extent of the mischief of very slight practical importance; in fact, in the earlier stages of the disease the treatment is very little affected by it. But where operative interference comes into consideration we feel its necessity very keenly. It is then important to know whether the tibia, astragalus or the tarsal bones, or the synovialis are involved. In most cases we will be enabled to establish, even before fistulæ exist, whether certain bones are affected or not, as the diseased tarsal bones are generally sensitive to indirect pressure. If you wish to know whether one of the cuneiform or scaphoid bones are affected, you steady the ankle joint with one hand, and with the other you exercise an equable pressure in the direction of the longitudinal axis of the foot successively upon each head of the first three metatarsal bones. If you suspect the cuboid bone, practice a similar test upon the two outer metatarsal bones while you steady the os calcis. In a similar manner you may recognize affection of the astragalus by pressure upon the plantar surface of the calcaneum, steadying, at the same time, the leg. Somewhat after this method you may discover the seat of primary affection of the synovialis in the different joints of the foot by testing their function separately. You must not forget that flexion of the foot takes place in the tibio-tarsal or ankle joint; pronation and supination mainly takes place between the astragalus on one side and the calcaneum and scaphoid bone on the other. Affection of the meso-tarsal or Chopart's joint may be detected by strong flexion of the foot in this joint. Passive movements in synovitis are generally very painful, and an extensive destruction of the ligaments is revealed by abnormal lateral mobility of the joint; in exceptional cases also by crepitation. Of course this method of examination will

Digitized by Google

only be of value with patients who give expression to pain in a reliable way; children generally baffle our efforts very much in this respect. Should swelling exist and no pain be elicited in this manner, we may suspect periostitis.

Treatment.—With children a conservative treatment is generally indicated. But where extensive suppuration exists, and where unmistakable signs of an osteal process of the tibia, astragalus or malleolus, are manifest, operative interference can be recommended equally well; it will at any event shorten the whole process. As to grown persons there is, in severe cases, less agreement among surgeons in regard to treatment in this locality than in any other joint. Some advise evidement of the fistulæ, and, in case nothing is accomplished by it, immediate resort to amputation; others again recommend amputation from the start. A number of others, again, have their own special method of typical resection, by which they gain access to the affected portions and remove them with the sharp spoon, saw or chisel. Hueter recommended a transverse cut at the anterior aspect of the joint, right through all the tendons and nerves, but so far, and probably with good reason, his method has found no imitators. Busch tried to get at the parts from below by sawing through the calcaneum from the plantar side, and Vogt would immediately begin with extirpation of the astragalus to get a full view and access to the rest of the portions necessitating removal. These methods may answer in a few cases, but there is no reason to think that they ever will be generally adopted. If possible, we should avoid total extirpation of the astragalus, at least, it should never be done for the simple purpose of getting better access to the parts. Aside from the shortening of the foot, it is not always easy to adjust a proper shoe, as the heel becomes very short. We should therefore only restrict it to cases where the bone itself is affected, or the surrounding joints, *i. e.*, the talo-navicular and the talo-calcaneal joint. The observation, that in the majority of cases osteal processes start from the anterior region of the tibia and astragalus, induced Koenig to make the incision more in front. He makes two lateral incisions in front of both malleoli, which really furnish all the room necessary to inspect the inside of the joint. The inner incision begins upon the tibia, about 1 to 1½ inches above the joint surface, between the inner side of the extensors and anterior margin of the tibia, opening the joint and taking its course downward. It is extended over the body and neck of the astragalus and ends at the inner side of the foot, near the prominence of the scaphoid bone. This incision does not injure any important structures, and admits of a very good inspection to nearly all the parts of the joint. But a fuller view is gained by a second incision running along the anterior margin of the fibula and ending at the height of the talo-navicular joint. If we have a tuberculosis of the synovialis, it will bulge forward and can be removed with some ease. By elevating the soft parts you can now examine the anterior and a great part of the lateral aspect of the astragalus. By making traction upon the foot you can oversee the largest part of the joint surface, and are already enabled to remove some osteal foci as well as the diseased synovialis. Should the astragalus be affected, the question will confront us whether to remove only a part or the entire bone. A focus of the body of the astragalus near the confines of its neck undoubtedly necessitates total extirpation, as it otherwise might break through into the surrounding joints. With the incisions already indicated, we have no great difficulty in accomplishing this, and also in removing the posterior portion of the capsule. Any diseased portion of the tibia may then be removed by means of the spoon or chisel. But should total resection be decided upon, the incisions mentioned above will be very convenient. Koenig, in order to give lateral support to the joint, removes the outer shells of the malleoli with the chisel, leaving them simply in contact with the periosteum above and with the ligaments below. With a broad chisel he removes whatever is affected from the tibia, astragalus, and eventually some parts of the calcaneum. I have practiced this method in two cases, and am somewhat surprised that it is not more generally

Digitized by Google

adopted by the profession, as the claims made for it by Koenig have been fully substantiated in my experience. I did, however, not think it essential to chisel off a shell of bone from the malleoli, but simply preserved the periosteum. In one case I removed about one inch of the astragalus, one-half inch of the tibia and left the fibula intact; the result has been very satisfactory, and the shape of the foot suffered very little. Some other methods have of late been published by Zesas and Kocher, which in their hands furnished very nice results. The method published by Zesas was first suggested by Girard and tried in several cases. The foot rests upon the inner malleolus; an incision of about two inches begins in front, above the line of the ankle joint, at the point of connection between the tibia and fibula, and runs obliquely down to the point of the external malleolus. Keeping the foot at right angles, a horizontal incision, beginning right at the external margin of the tendo-Achillis and passing closely to the point of the malleolus externus, then ending immediately before the tendon of the peroneus tertius, will connect with the first incision. The peroneal tendons are then exposed and each one transfixed by a thread in two different places, and thereupon separated with the knife between the points of transfixion. After the soft parts are dissected off, you can readily see the astragalus and the inner surface of the joint. The capsule of the joint is split in its full length, the ligaments are separated at their insertion, and, while the foot is kept in supination, the astragalus is extirpated with ease; after this the foot is luxated inward. The cavity of the joint is now fully accessible, and any suspicious portion can be removed. The claims for this method are, that it affords a better view and access to all the parts than any other method so far known. Quite similar to this is Kocher's method; it, in fact, seems to differ only in the external incision. Keeping the foot at right angles, he begins at the tendo-Achillis and makes a slightly curved incision over the point of the external malleolus, ending at the extensors. He also divides the peronei tendons, and afterward unites them again. Reverdin also has operated in a similar manner, but, besides, he divided the tendo-Achillis, and did not unite the peroneal tendons. In cases where the astragalus and calcaneum, and even the rest of the tarsal bones are affected, and where partial resection might not promise a satisfactory result, we may resort to an operative method known by the name "Wladimiroff," Miculicz's osteoplastic resection. This method seems to have given great satisfaction in about thirteen cases. Kümmel has in one case even removed parts of the tibia and fibula, all the tarsal and part of the bases of the metatarsal bones, with very good functional results. Miculicz begins the incision a little in front of the tubercle of the scaphoid bone, and carries it across the plantar surface to a point behind the base of the fifth metatarsal bone. Schattauer made the cut about one-fourth to three-fourths of an inch anterior to this, running his incision over the cuneiform and cuboid bones. Then from these points incisions are made on the tibial and fibular side, upward and backward to the malleoli, and these again are united by a horizontal incision, dividing the tendo-Achillis. Fenger has modified this method somewhat in order to preserve some branches of the posterior tibial artery, as the fear of gangrene of the foot is not altogether unfounded, seeing a case reported by Sordina. He does not carry his incision entirely across the sole of the foot, but begins about three-fourths of an inch outside the tibial surface of the os calcis, below the sustentaculum tali and to a point one inch back of the sustentaculum; then it is continued upward along the tibial border of the tendo-Achillis to the posterior side of the ankle joint. It is a little more difficult in this way to get at the ankle and Chopart's joint than by Miculicz's method. But this difficulty can be overcome to some extent by separating the soft parts subperiosteally from the calcaneum and astragalus, and by raising the fibular incision more upward toward the dorsum of the foot, so it will terminate about one-fourth of an inch in front of the external malleolus. It is best to begin the disarticulation of the ankle joint first, as this gives more room for the separation of the dorsal flap, with the extensor tendons

Digitized by Google

and anterior tibial artery, from the neck of the astragalus. Chopart's joint is also more readily disarticulated in this manner than from below. Separation of the dorsal flap from the astragalus should be the next step, and this should be done sub-periosteally; any injury to the anterior tibial artery is thereby excluded. The preservation of the extensors secures also more active mobility of the toes, rendering thereby the gait more elastic. After this the ligaments of the calcaneo-cuboid and talo-scaphoid joint are cut from above, and the heel is removed. The malleoli, the articular surfaces of the tibia, cuboid and scaphoid bones are then taken off with a saw, and if we find any further affection of the bony parts we may use the sharp scoop. Should the tuberculosis extend along the sheaths of the tendons, we may remove it with the spoon or with scissors and forceps.

FUNDAMENTAL ANATOMICO-MECHANICAL CONSIDERATIONS UNDERLYING THE SUCCESSFUL TREATMENT OF DEFORMITIES, DISEASES AND WEAKNESSES OF THE SPINE.

CONSIDERATIONS ANATOMICO-MECANIQUES FUNDAMENTALES BASES DU TRAITEMENT AUREUX DES DIFFORMITES, MALADIES ET FAIBLESSES DE L'EPINE DU DOS.

FUNDAMENTALE ANATOMISCH-MECHANISCHE ERWÄGUNGEN, DIE DER ERFOLGREICHEN BEHANDLUNG VON DEFORMITÄTEN, KRANKHEITEN UND SCHWÄCHEN DES RÜCKGRATS ZU GRUNDE LIEGEN.

BY MILTON JOSIAH ROBERTS, M. D.,

Professor of Orthopedic Surgery and Mechanical Therapeutics in the New York Post-Graduate Medical School and Hospital; Visiting Orthopedic Surgeon to the New York City Hospitals on Randall's Island; Consulting Orthopedic Surgeon to the Woman's Hospital, Brooklyn, etc.

The present paper will be eminently of a practical nature. No measure is herein advocated which has not been thoroughly tested and shown to be of practical value.

Method.—Believing that specialization is the surest road to success, no matter what branch of knowledge it is sought to advance, I have for several years devoted a large part of my time to the study and treatment of spinal diseases and deformities. It will, therefore, be understood that whatever I have accomplished in the way of increasing our therapeutic resources for the cure or amelioration of spinal affections, has not been the result of so-called "happy thoughts," but, on the contrary, has been the outcome of persistent and laborious endeavor in this special direction, extending over a period of several years.

Aim and Motive.—I have aimed at the acquisition of that kind of knowledge which is available for the cure of spinal disease, the relief of human suffering and the prevention of unsightly deformity. I have endeavored all along to maintain that frame of mind so essential to the avoidance of self-deception, viz., that of being absolutely indifferent as to which one of two or more ideas are true; my only concern being to ultimately arrive at accurate and trustworthy conclusions based upon facts which all trained observers can verify for themselves.

The Elaboration of Means to an End.—In the practical execution of the mechanico-therapeutic ideas arrived at I have spared no expense, time or labor necessary to produce the most effective, comfortable and elegant apparatus. (In adopting this course

Digitized by Google

I have received much encouragement from the afflicted, for in my professional capacity the one question which I have been most frequently and earnestly besought to answer by anxious parents bringing children for treatment of spinal disease or deformity is, What is the best that can be done in the way of relief and cure of my child? What is the most serviceable, comfortable and least conspicuous form of support for him to wear? The prime question has not been, as so many physicians seem to think, "How much will treatment cost, and what is the cheapest apparatus that can be used?")

Though I have received much encouragement from this source, it is scarcely possible for any one to adequately appreciate the ground gone over, who is not practically familiar with the difficulties of procuring competent workmen to carry out novel mechanical ideas, who is a stranger to the frequent encounter of what at first appear to be insurmountable obstacles, and who has never shouldered the burden of the enormous number of perplexing details which continually present themselves while conducting to a successful issue a series of mechanical experiments.

Result.—The invention of the spinal corset herein described was made over four years ago. Desiring above all things to be absolutely accurate in my statements regarding it, the corset has thus far been used without being brought to the attention of the medical profession. Since its invention I have been engaged in elaborating it, and in making a most thorough and extensive test of its practical value in the treatment of all forms of spinal affections.

The uniformity of success which has attended the use of the apparatus herein described in arresting the progress of that terrible malady—spinal caries (Pott's disease)—and in preventing the great suffering as well as the development of that hideous deformity which often results from the ravages of this disease; its wonderful adaptability to the treatment of all forms of lateral curvature and weaknesses of the spine, and finally, the universal expression of satisfaction on the part of patients wearing it as to its comfort, inconspicuousness, and elegance, all combine to encourage me in now bringing it to the notice of the profession. This I do with the fullest confidence that those whose minds are unbiased and who will give it a thorough practical test in the light of therapy and hygiene will be as enthusiastic in declaring its superiority over all other forms of portable supports as are the members of the limited circle of physicians and patients who now know its worth.

Distinctive Features.—The distinctive features of my corset are, that it is woven out of wire in the form of thread, that the weaving is done over an exact* form or cast of the body, and that the meshes, while affording the most perfect ventilation to the body, are formed in such a manner as to combine permanency of contour with elasticity of support and accuracy of fit.

Personal Qualifications and Management.—The fullest success obtainable in the treatment of spinal affections implies a high degree of manipulative dexterity on the part of the surgeon. He must also possess a sufficient knowledge of mechanics to readily overcome the numerous little obstacles that present themselves in the course of treatment, which are dependent, for the most part, on the peculiarities of the bodily contour and physical condition of individual patients, and, therefore, cannot be satisfactorily considered in detail in books or articles on treatment. When anything goes wrong with a support which a patient is wearing, the physician in charge of the case should immediately endeavor to overcome the difficulty complained of. He should neither ignore its importance nor postpone giving attention to it. It is requisite also that the physician should be sufficiently familiar with human nature and the art of giving

* Sometimes the form is modeled for therapeutic or æsthetic purposes prior to the weaving of the corset. In this way a corrective action may be exerted by the corset, or the bust may be given any desired form.

Digitized by Google

directions, to secure the hearty coöperation of those interested in the carrying out of any plan of treatment laid down for individual patients.

Technology.—Success in the scientific treatment of spinal diseases, deformities and weaknesses implies a knowledge of, and personal familiarity with, a very considerable number of technical details. These are, for the most part, matters of general applicability, and, therefore, will be considered first.

*Fundamental Ideas Regarding the Support of the Body Proper for the Cure of Disease, the Relief of Suffering or the Arrest of Deformity.**—If mechanical support is to be utilized as a therapeutic agent, a definite idea should be entertained by the physician as to the *modus operandi* of the production of its beneficial effects. Otherwise, he who uses such means may some day wake up to find that he has been the credulous supporter of authority or the blind follower of the teachings of tradition, rather than an intelligent institutor of remedial measures the *rationale* of which he thoroughly comprehends.

The Normal Spinal Column.—Viewing the normal spinal column, stripped of its appendages, we see that it presents a graceful curvilinear outline, that the vertebral bodies, with their respective processes and articular facets, alternately rest upon and are supported by intervertebral substances.

Spinal Caries or Pott's Disease.—When caries of the spine has progressed to the stage of deformity, the anatomical relationship of the different parts of the vertebral column to each other becomes changed. A good idea of the disastrous nature and extent of the changes which take place when the disease is not properly treated from the first, can be gained by a description of the vertebral column of a child, aged four years, who was brought to me for treatment. The bodies of the tenth, eleventh and twelfth dorsal vertebræ were the seat of disease. The necrosed remnants of the bodies of the eleventh and twelfth dorsal vertebræ lay loosely in a capacious abscess sac anterior to the spinal cord. There was marked thickening of the periosteum anteriorly, together with the usual inflammatory fusion of the periosteum and cord membranes, forming the posterior wall of the abscess cavity.

These, then, are the terrible ravages of disease which the surgeon is obliged to face, and which he seeks, through the agency of mechanical and other remedial measures, to aid nature in repairing. Owing to the inaccessibleness of the diseased areas of bone, they being buried deeply beneath the soft parts, it would, at first sight, seem as if the surgeon were utterly powerless to aid nature in such a manner as to bring about a cure. But let us see if we cannot elaborate a rational basis for mechanical support in diseases of the spine.

In ostitis, or caries of the vertebral bodies, the establishment and consummation, of reparative processes depend upon the material lessening and final arrest of irritation at the seat of the disease. Now, it is perfectly feasible, in my judgment, to more or less effectually interrupt or prevent the transmission of all mechanical irritation to the seat of disease. The sources of such irritation are :—

1. That which is caused by undue pressure at the inflammatory focus, from superimposed bodily weight.

2. That which results from jars or vibratory impulses due to the impact of the feet against the ground in walking, and which are imparted to and conducted up along the bony framework to the seat of disease.†

---

* See leading article on this subject, by author, in the *New York Medical Record* for July 16th, 1887.

† See paper, by author, entitled, "The Fundamental Principles of Mechanico-Therapy in Hip Disease, based on a consideration of Clinical, Pathological and Physico-Physiological Data, with a description of New Forms of Elastic Tension Hip Splints." Read before the Medical Society of the State of New York, February 6th, 1884. The *New York Medical Journal*, March 15th,

Digitized by Google

3. Jars which result from riding in vehicles of any kind, and which are transmitted along the bony framework to the seat of disease.

4. Direct blows or concussion resulting from falls.

The relief of irritation from the first-named cause, viz.: superimposed weight, involves the use of an apparatus which will either transfer the weight of the parts of the body above the seat of disease to healthy parts below, or to some source of support other than the body of the patient. The relief of irritation from the second and third named sources, resulting in jars or vibratory impulses, involves, in addition to the thorough vertical sustentation of the patient, the use of an apparatus which will, at the same time, deflect or interfere with the transmission of jars or vibratory impulses along the bony framework to the seat of disease. This can be accomplished only by the use of an elastic or resilient apparatus. To guard against irritation from the last-named sources necessitates the use of an apparatus which is, in itself, a coat of mail, and is, at the same time, a resilient support.

But, how is relief from irritation to aid repair? The answer is this: In all inflammatory lesions of bone we have the formation of new or embryonic cells. If the irritation at the seat of disease be excessive, these cells will break down and form waste products. If, however, the irritation, be not too great, they become developed into healthy bone tissue.*

Thus, it will be seen that *for the cure of caries or Pott's disease of the spine*, we have a clear indication for the relief of mechanical irritation by means of mechanical appliances, and just in proportion to the thoroughness with which we are practically able to carry out this indication will reparative processes be established.

Furthermore, experience has taught us that, *for the relief of suffering*, the complete and permanent removal of undue bodily weight above the seat of disease will be found all that is necessary to be done in order to overcome the pain resulting from pressure upon sensory nerves. Finally, *for the prevention of deformity* in Pott's disease, we have learned that no success will attend treatment unless the vertebral bodies are kept from falling together by the removal of superimposed bodily weight.

Again, when there is no longer any inflammation, when the morbid process has been transformed into physiological action, or the vertebræ more or less thoroughly anchylosed, there may yet remain the indication for support. This is always true in those cases where the disease has progressed so far as to seriously deform the spine, and thus by interfering with its normal linear continuity, impair its supporting power.

Lateral Curvature.—In lateral curvature the overtaxing of the spine by superimposed bodily weight is a potent factor in the production of the deformity and the consequent pressure upon nerves. In support of this statement, it is only necessary to recall the familiar experiment of making linear traction upon the spine for the temporary reduction or obliteration of deformity, and in aggravated cases, for the relief of pain, and in this way to relieve the spine of its undue burden. The use of any mechanical appliance in which it is sought to overcome the spinal deformity by means of force applied through levers, will result in disappointment.

Linear traction on the spine is the most efficacious method known of reducing all forms of lateral curvature. Such traction can only be applied with full efficiency through gymnastic exercises. The tendency of all supports, if worn constantly, is to lessen the power of the spinal muscles, and especially is this the case in true rotatory

---

May 3d, and May 24th, 1884. The *Proceedings* of the Medical Society of the State of New York, 1885.

* Vide "Manual Pathological Histology," by Cornil & Ranvier, translated by A. M. Hart, Vol. I, p. 116. "Whatever may be the origin of the embryonic tissue, it has a tendency to reproduce the tissue of the region in which it is situated." London, 1882.

Digitized by Google

lateral curvature. In lateral curvature there is no new formation of bone taking place which is to ultimately act as a prop to strengthen the spine, as in the case of Pott's disease. It thus follows that spinal supports must be used with the greatest caution in this class of cases. Otherwise, through the weakening effect which they have upon the muscles, and by the neglect of gymnastic exercises, the patient will soon be rendered more helpless, when the support is temporarily removed, than at the inception of treatment, before any support was applied.

Then again, as in Pott's disease, so in examples of true rotatory lateral curvature, when the deformity has become so marked that all hope of diminishing it to any considerable extent must be given up, there yet remains the indication for lessening the burden of the spine by providing a suitable support; for while the weight of the body remains about the same, the normal spinal curves have been so seriously distorted that the column can no longer perform the function of supporting the body, and the result is great discomfort to the unfortunate victim.

In all these cases the patient will always be made more comfortable by the wearing of a suitable spinal support. These patients should, however, never be led into the belief that they will, after a more or less prolonged course of treatment, be able to go about unsupported with as much comfort as while wearing a suitable support.

Traumatic Spinal Curvature.—There is a form of lateral curvature which occurs most frequently in male adults, and is directly traceable to injury of some of the soft parts outlying the spinal column proper. Cure is, as a rule, rapidly brought about in all such cases by wearing an efficient and properly adjusted support for a short time, thereby restoring the body to its normal position and relieving the injured tissues of undue irritation during the period of repair.

Spina Bifida.—In examples of spina bifida which do not prove fatal early in life, artificial spinal support may be demanded, or at least found of great practical utility.

Spinal Weakness.—In simple weakness of the spine, unaccompanied by deformity, the patient continually assumes faulty attitudes, both while standing and sitting, thereby cramping the chest and abdominal organs and giving rise to more or less discomfort. All such patients are greatly benefited by using a stay which precludes the possibility of their assuming faulty attitudes. At the same time it is of the greatest importance to supplement such treatment by suitable gymnastic exercises and attention to the general physical condition of the patient.

These, then, are the considerations which lead us to make use of mechanical supports in the treatment of spinal affections. The treatment, therefore, resolves itself into two questions: first, how and where to get hold of the body in order to support it in such a manner as to relieve irritation; and secondly, the determination of the best means or therapeutic agents to employ in order to effect this desirable end.

How and Where to Get Hold of the Body.—In considering how and where to get hold of the body in order to support it, the problem should be viewed from a thoroughly mechanical standpoint. We have the skeleton and overlying soft parts to deal with. Now, it is the prime function of bone to support weight. To transfer weight from healthy parts above to healthy parts below, in order to relieve an intermediate diseased or weakened segment of the spine from mechanical irritation, it is obviously necessary to get hold of some part of the skeleton above and below the diseased or weakened zone, and to introduce some form of artificial prop to take off the burden from the weakened spine. The maximum efficiency can only be obtained by so fastening this prop to the body that the same relative position between it and the body will be maintained at all times. A clear idea of this extremely important part of our subject may be gained from a consideration of the normal relationship of the different parts of the body.

When the cervical region of the spine is affected, the globular form of the skull,

Digitized by Google

together with the lower jaw and the shoulder girdle can be relied upon as points against which pressure may be exerted to relieve the affected region of the neck.

When the dorsal part of the column is affected, we again have the head and chin for the points against which upward pressure is exerted, through the agency of some form of head-rest, and the pelvic girdle, upon which to rest our support. It will thus be seen that we are here working at a far greater disadvantage than when dealing with the disease in the cervical region, for we have the enormous weight of the head, shoulders, upper extremities and that part of the body above the diseased vertebræ, which must be transferred to parts below in order to relieve the irritation.

When the lumbar region is affected, we rest our support upon the pelvic girdle and make upward pressure against the bulge of the thorax. I have elsewhere* stated that "it is a pertinent fact, and one that should never be lost sight of by those who are brought into the responsible position of making a prognosis in any given case of spinal caries, regarding the prevention or arrest of the development of deformity, that the results of treatment in the vast majority of cases tally exactly with the above ideas regarding mechanical support of the spine. That is to say, the best results are obtained when the cervical and lumbar regions are affected; the most disastrous when the middle dorsal region is the seat of disease. Formerly, it was supposed that in the mid-dorsal region the best results could be obtained by treatment. Regarding the diseased and deformed spine as a broken shaft which could be drawn into position by means of a lever, theory suggested the idea that, with a fulcrum in the middle of the back and power applied to the body above and below, deformity could be prevented, or overcome, by drawing the body backward above and below against the lever, where it could be secured. But it must be remembered that the spinal column cannot be likened unto a shaft broken at one point—it has many segments—and that to execute the above idea in the same manner that we would reduce deformity when a long bone is fractured in its middle, is out of the question."

Before leaving this part of our subject, I wish to state that when dealing with patients requiring support, in whom the disease has already advanced so far as to give rise to serious distortion of the bony framework, the question of selecting sites for getting hold of the body not infrequently becomes a difficult problem to practically solve. All such cases must be considered as individual problems with reference to deciding where and how to attach support to the body.

Sometimes, especially when the disease is in the dorsal region of the spine, it is a mechanical impossibility to get hold of the body with any portable support in such a manner as to effectually relieve the irritation. Enough support may be rendered, however, to add greatly to the comfort of the patient. Under such circumstances, if the disease be active, we should not permit ourselves or our patient to rest contented with even the best of portable supports. Certain adjuvants should be made use of, to which the superimposed bodily weight is to be transferred.

We are now prepared to consider what therapeutic agents should be employed to relieve a diseased or weakened spine of irritation and of its undue burden. Without stopping to criticise any of the existing forms of apparatus, I will proceed at once to describe what, in my judgment, after a four years' practical test in a large number of spinal cases, representing various phases of disease, curvature and weakness, excels all other spinal supports which I have met with during eleven years' close study of this subject, in its efficiency, comfortableness, inconspicuousness and elegance.

---

* "Fundamental Ideas Regarding the Support of the Body Proper for the Cure of Disease, the Relief of Suffering or the Arrest of Deformity," *New York Medical Record*, July 16th, 1887.

Digitized by Google

*Moulds or Impressions of the Body.**—The object of making moulds or taking impressions of the body, in connection with the plan of treatment which I now adopt in spinal affections, is to secure a form over which the support to be worn by the patient is constructed. In dealing practically with patients sufficiently good results can be obtained by making moulds over closely fitting knit shirts, after the fashion of applying a plaster jacket, the operation being, in fact, conducted in the same manner and in accordance with the usual directions for applying a plaster jacket, except that the layers of plaster bandage should be carried as far up behind and in front as possible, and the lower margin of the mould extended to the pubis in front and the coccyx behind. As soon as the plaster sets a longitudinal division of the mould and shirt is made in front. It is then removed by gaping the cut edges apart, after which they are approximated and secured in position by a roller bandage. The mould is now placed on supports over a stove or in the hot-air chamber of a furnace, until thoroughly dried. The cast made from such a mould will exhibit all the anatomical points which are so essential to the construction of an accurate and easily fitting corset. To overcome the objectionable feature of this method of taking a mould, which involves at the time of its removal the leaving of the patient's body entirely nude, I have had a special skin-fitting garment constructed. This garment or shirt is furnished with head and arm holes at the top. At the bottom there are holes for the lower extremities, the space between these latter openings being closed. The garment is drawn on by first passing the feet through the neck holes, and afterward through the respective openings at the lower end of the shirt. As the garment is drawn up over the body the hands are passed through the arm holes and the ribbon is tightly drawn about the neck. These shirts are made very thin, and, as I have said, are skin fitting. Over such a garment applied by the patient's attendant, an ordinary skin-fitting knit shirt is drawn, such as is now widely used, over which to apply a plaster jacket. In making a mould of the body, after the fashion of applying the jacket, as can readily be understood, the plaster adheres only to the outside shirt. It is, therefore, only necessary, in removing the mould, to cut through this and remove both the mould and the entire shirt together, leaving the body of the patient entirely covered by the garment first applied. By adopting this plan, the most sensitive lady is not subjected to undue embarrassment.

The Attitude of the Patient while Moulding the Body.—The most convenient attitude for the operator to place his patient in while moulding the body is that secured by suspension. For this purpose I use a modified form of apparatus that enables me to know at all times the amount of traction force used on the head and arms.

In suspending a patient with a view of getting a mould from which to make a cast over which a wire corset is to be woven, it is important that the patient should not be placed in an attitude which, when the corset is applied to him, he will be unable to maintain. To take a mould with the body in such a position would frustrate our therapeutic endeavors, as it would be impossible for the patient to wear with comfort any form of portable apparatus that necessitates the maintenance of an unnatural attitude. The form of suspension styled self-suspension should never be made use of while moulding the body.

Casts.—Having secured a mould of the body, the next step is to make a cast. This is done by making a mortar of plaster-of-Paris and water, and filling up entirely or lining the mould with a thick layer of this mortar, which soon sets. The mould is then cut down, spread apart and removed from the cast in the same manner that it was removed from the body.

---

* See article by author, entitled, "Moulds, Casts and the Modeling of Casts for Therapeutic Purposes." *New York Medical Monthly*, 1887.

Digitized by Google

Modeling of Casts for Therapeutic and Other Purposes.—It will be remembered that the corset which we are to use is to be constructed directly over the cast of the body of our patient. There results from this plan of construction far greater nicety of adaptation to the contour of the body, or the ends of remedial endeavors, than can be obtained in any other way.

If we desire to exert remedial pressure upon any part of the body for the purpose of overcoming deformity or preventing its increase, much can be done to effect this end by modeling the cast of the body of the deformed individual prior to making a corset over it for him to wear. This will be readily understood by a description of the casts of a patient sent to me by Dr. George C. F. Williams, of Cheshire, Conn., who had lateral curvature of the spine. The projecting shoulder on the convex side of the superior curve was cut down, and the corresponding concavity built up. The undue projection on the convex side of the inferior curve was also cut down, and its corresponding depression filled up. The distorted lateral outlines of the waist were made to more nearly correspond to the normal. Changes analogous to those just described were also made in the cast anteriorly. Now, when the corset has been woven over the corrected cast and accurately adapted to the contour of its surface, it will retain its form when removed from the cast. It can be readily understood, therefore, that when it is applied to the distorted body of a patient, a corrective force will be constantly exerted by it that will tend to push the body into the improved form of the corset. All unnatural depressions having been filled up, abundant space will be left for the development of the corresponding parts of the body. All the unnatural prominences of the cast having been cut down, a corresponding degree of corrective force will be exerted on unduly prominent parts of the body. The corset is thus made to fulfill the double function of an automatic remedial agent and beautifier of external figure contours.

Again, casts of patients suffering from lordosis and round shoulders can be corrected so that when the spinal corset made for them is applied to the person, it will exert remedial pressure. To understand how this is done I will describe a cast of the body of a young lady patient sent to me by Prof. Seneca D. Powell, of New York. The cast was divided through at the waist. The upper part of the cast was then tilted forward until the lordosis had been corrected, and it was held in this position by a plug placed between the upper and lower parts posteriorly. The dimensions of the wedge-shaped piece which it was necessary to remove from the upper part of the cast in order to correct the round shoulders, could now be very accurately estimated. Having marked upon the cast the dimensions of this wedge, the base of which pointed posteriorly, it was removed from the cast by means of a saw and used to fill up the gap made in the correction of the lordosis. Having removed the wedge-shaped piece, as already described, from the upper part of the cast, the surfaces were now placed in apposition and there resulted the correction of the cast for the round shoulder deformity. A corset made over such a corrected cast will, when applied to the patient, exert remedial pressure.

There are many other ways in which casts may be modeled for therapeutic and other purposes, which will suggest themselves to those engaged in the treatment of this interesting class of cases. In modeling casts for females it is desirable oftentimes, for the comfort of the patient, but frequently for appearance sake, to make the breasts more or less prominent, according to circumstances. In this way the asymmetrical development of the mammæ in lateral curvature can be concealed from the eye, for a corset made over the corrected model will hold the clothing out equally on both sides.

Material out of which the Corset is Constructed.—It is made directly out of wire in the form of thread. In this respect it differs from all other attempts to construct corsets out of wire. The desirability of constructing an open mesh corset has been recognized in various parts of the globe for a number of years, as shown by the efforts of

Digitized by Google

various inventors. All these efforts, however, so far as I am aware, have had for their object the adaptation of wire gauze in sheet form to the contour of the body. All such garments are very clumsy in appearance and unsatisfactory in use. The application of wire in the form of a thread to curvilinear contours, for therapeutic purposes, is, I believe, original with myself. The wire used in the construction of these corsets may be heavy or light, fine or coarse, soft or hard, according as it is necessary to meet the requirements for support, resiliency, weaving, twisting, etc. It may be made out of any desired metal or alloy of metal. For some time I have been constructing all my corsets out of the incorrodible alloy, phosphor-bronze. This makes a remarkably tough wire and, when woven up into form, a durable corset. The wire may be plated either before or after it has been worked up into corset form. This gives a brilliant finish to the completed garment. Again, the wire may be bound with silk, cotton or other textile filaments of any desired color or shade of color, after the manner of insulating wire for electrical purposes. Or again, the corset, after it is woven up into form, may be covered inside and out with a loosely woven or knitted textile fabric. By using properly selected materials, it is possible to vary the strength of the corset as required for individual cases. That the therapeutic indications can be met with a corset of the construction herein described, in the persons of heavy individuals suffering from Pott's disease and imperatively demanding efficient support, I have demonstrated over and over again.

The Pattern of the Corset.—The pattern of the corset may vary in shape according to the requirements of the individual for whom it is especially constructed, or according to the therapeutic indications which it is intended to fulfill. Remedial stays for the treatment of Pott's disease may be constructed in three pieces. When using the corset for Pott's disease I prefer, however, to construct it in a single piece, with a quadrangular stay in the back, to which the other stays are riveted. By so constructing the corset any desired amount of strength can be secured. When plated, the corset is retained in position on the body by suitable lacings and clasps. Thus arranged, it is easily applied and removed. When a head-rest is required in conjunction with support for the body, as in treating caries of the cervical vertebræ, and usually of the dorsal vertebræ, the corset may be made in one piece and the thoracic shafts of the head-rest incorporated in the meshes of the corset during the process of weaving.

Again, the corset may be made with one or more openings or fenestra in its walls, in order to render feasible the dressing of wounds or sinuses while the corset is in position on the body of the patient. If used for ordinary wear or for the support of weak spines, or the cultivation and perfection of the human figure, it may consist of two lateral halves joined in the back by lacings and clasped in front. Finally, the pattern of the corset may be otherwise modified to meet the exigencies of individual cases.

Weaving of the Corset into Form out of Wire.—As I have already stated, whatever the pattern of the corset may be, its distinctive feature is that it is directly constructed or woven out of wire in the form of thread, over a model of the body. Far greater nicety of adaptation to the contour of the body, or the ends of remedial endeavors, results from this plan of construction than can be obtained by working into form sheets of wire gauze. By weaving a corset of the pattern of twisted mesh, it will be found that when it is applied to the body and properly adjusted, during the respiratory efforts, the meshes themselves anatomically expand and contract with the expansion and contraction of the thorax. This feature of the corset, which can be secured by no other method of construction, is peculiar to my invention.

The Application of the Corset to the Person of the Patient.—Having in hand a corset woven directly over a cast of the body of his patient, the surgeon, on the occasion of first applying it, should see to it that his patient is suspended with a skin-fitting shirt,

Digitized by Google

as when taking a plaster mould of the body. In this position he can apply the corset and adjust it more satisfactorily than in almost any other attitude.* When the corset is first applied the elastic lacings should be loose enough to permit of easily clasping it about the body of the patient. After fastening the clasps in front the lacings on either side and at the back should be drawn up so as to bring the corset closely in contact with the body all around over its entire surface. When this has been done the patient can be lowered from the suspension apparatus, and if a head-rest is attached, the head gear adjusted. Not infrequently, in adjusting corsets which have head-rests attached, it is necessary, as will be understood, to curve the cephalic shaft or alter the inclination of the short piece which connects it with the triangular shafts which are incorporated in the corset, so that the pivot holding the crossbar to which the head straps are attached is over the centre of the head. Should the corset cause any discomfort whatever, it should be removed at once and the part where the discomfort is experienced gently pounded out from the inside with some rounded wooden instrument, or, when applied to the patient, pressure can be relieved at any point by inserting a button hook through the meshes and making outward traction on the wire. Usually, the slightest alteration in the plane of the meshes will be sufficient to relieve any discomfort. If properly adjusted, on the occasion of first applying the corset, it ought to require no breaking in, but be comfortable from the first.

There is one point to which particular attention should be paid. It is the adjustment of the strength of the elastic used as a lacing to the strength of the respiratory effort. When the corset is applied and laced up, as I have directed, the patient should be able to overcome the elasticity of the lacing so as to take a full, deep inspiration, without undue effort. Upon expiring, the elasticity of the lacings should cause the corset to automatically follow the receding chest walls, so that at no time during the respiratory effort do the chest walls leave the corset, as is the case with the plaster jacket, felt jacket and all other rigid enveloping spinal supports. This is the test which must be applied to each individual case. If it is found that the patient is unable to overcome the elasticity of these lacings, so as to take a full breath, it will be necessary to use lighter ones. If, during the process of expiration, it is observed that the lacings do not automatically keep the corset in contact with the receding chest walls, then one of two things is true: the corset has not been laced up properly, or the lacings are too weak and should be replaced by stronger ones. The lacings can be secured in almost any dry goods store. If, however, those of the exact size desired are not in stock, I would advise taking two or three or more threads of small size, and using them as a single lacing.

When the corset has been applied as directed and laced up, it is no longer necessary to readjust the lacings with each successive application of the corset. They will require readjustment only when the strain upon them has been continued for a sufficient length of time to deprive them of a certain amount of the elasticity. A sufficient supply of lacings should be always kept on hand, so that when occasion presents the old ones can be replaced by new ones.

Improving the Appearance of the Hopelessly Deformed.—It cannot be gainsayed that there is a very considerable number of patients who apply for treatment whose deformity cannot be overcome or even ameliorated to any considerable extent. The indications in all these cases are two. First, to relieve the sufferings of the patient by rendering him efficient support with a comfortable and movable apparatus. Second, to give him, when dressed in ordinary attire, as symmetrical a form as possible, in order that he may not

* If, however, this is impracticable, the patient should be placed in a horizontal position and the corset applied and laced up while he is in that position.

Digitized by Google

attract undue attention by the conspicuousness of his bodily deformity. To this latter end it is customary to apply pads of varying thicknesses and sizes over the abnormally depressed parts of the body, either on the inside or outside of the spinal jacket, in order to give the appearance of a symmetrical form when the patient is clothed. These pads are not only hot and uncomfortable, but they also absorb the perspiration of the body, and soon become filthy. There is an additional objection, viz: that such pads are continually changing their form. To overcome these serious objections I resort to the following method in constructing one of my wire corsets: An exact cast of the body is made. Over this cast a skin-fitting knit shirt is drawn. A mortar of plaster-of-Paris and water is now used to fill up the undue depressions of the body, so as to make the cast appear as symmetrical as possible. After having done this, the shirt is cut down in front and removed, together with the newly constructed pad of plaster. We are now ready for the weaving of the corset, which is proceeded with over the original cast after the manner already described in detail. Before removing the corset from the cast or mould over which it has been woven, the plaster pad or pads modeled on the outside of the shirt, and which give the appearance of greater symmetry to the form, are now placed in their respective positions over the wire corset. With these pads in place, strands of wire are intertwisted with the meshes of the corset at the upper margins of the body. The dependent portions of said strands are subsequently intertwisted and woven into a mesh covering the pad and corresponding to its contour. The pad is then removed, and the margins of the newly woven piece are made fast to the original corset. There results, as can be readily understood, a space between the original corset which exactly fits the contour of the body and that part of the garment which corresponds to its external contour, and which restores the symmetry of the body when the corset is applied to the person and the patient is attired in ordinary clothing. In this manner, the hot, uncomfortable pads which are commonly used under the clothing to restore the symmetry of deformed bodies are done away with, and we have a cool and hygienic garment instead thereof.

Special Provisions for Exerting Additional Remedial Pressure through the Agency of the Wire Corset.—Under the head, modeling casts for therapeutic purposes, I have called attention to the fact that corsets woven over corrected models of the body of deformed patients exert remedial pressure when applied. There are, however, a considerable number of examples of spinal asymmetry in which it is desirable to exert upon unduly prominent parts of the body the maximum degree of corrective pressure which can be borne without causing the patient unnecessary inconvenience and discomfort, and at the same time, without necessitating the weaving of conspicuous apparatus. For this class of cases I have devised a useful adjuvant to the already described plan of exerting remedial pressure. It consists in the peculiar weaving of the corset, and in the attachment to it and conjoined use of elastic lacings placed on the outside of the corset over the site of the abnormal prominence. The weaving of the corset is so executed as to permit, when completed, of the crowding together of its meshes from side to side. Being in possession of a properly woven corset, the meshes of which may be crowded together to any desirable extent over the unduly prominent parts of the body, narrow strips of strong cloth or leather, provided with eyelets, are sewed or otherwise fastened to the corset stays on either side of the abnormal prominence. The elastic lacing is now introduced through the eyelets and stretched across that part of the corset which is over the unduly prominent part of the body. By tightening the lacing the meshes of the corset are crowded together and the degree of pressure thus regulated. Now, when the corset is placed in position on the body, it will be necessary to expand the meshes under the elastic lacing in order to get the corset into proper position about the body. Once secured in position on the body, a gentle elastic force will be constantly exerted against the abnormal prominences.

Digitized by Google

Adjuvants.—Under the heading "How and where to get hold of the Body," I called attention to the fact that when disease of the spine was located in the dorsal region, it is impossible to furnish the desired amount of support without resorting to some adjuvants to which the weight of the body above the seat of disease may be transferred. Of several adjuvants which I have invented for this purpose I wish to mention one, the suspension tricycle. It is used as follows: To the wire corset shoulder straps are sewed on either side, and upon these shoulder straps small rings are placed. To these rings are attached elastic straps connected with the crossbar of the tricycle. The crossbar is suspended by means of a cord which passes through the pulley in the tricycle over the head, midway between the two upright shafts. From thence it passes over another pulley down to a cleat riveted to one of the lateral shafts of the tricycle. By means of this arrangement any desired amount of upward traction can be exerted. When the patient is in the tricycle the front part of the shoulders is brought up against the lateral shafts of the apparatus, and in this way it is pushed forward at the same time, being guided by a lever.

Binding of the Corset.—The stays and edges of the corset may be covered with strips of cotton, silk, velvet or any other fabric. I have recently invented a method of covering the stays and the edges of the corset with unabsorbing and incorrodible material, such as hard rubber, celluloid, etc.

Therapeutic Advantages.—The therapeutic advantages of this corset are, that it affords more efficient and constant support than any other form of apparatus; that it is resilient and therefore not irksome to the wearer; that it is extremely cleanly, not absorbing the perspiration; that it affords perfect ventilation to the body; that it permits of easy and unrestricted expiratory movements; that it is durable, elegant, and thoroughly adapted to the varying contours of the body and exigencies of individual examples of diseases, deformities, and weaknesses of the spine. It possesses, in addition, the superior qualities of extreme lightness, compared with many of the corsets or spinal supports now in use. Furthermore, the discharges from the body when brought in contact with it, do not weaken it or render it foul by their absorption, as is the case with the plaster-of-Paris jackets, felt jacket and supports made of felt or other similar materials. It does not absorb and become fouled by the perspiration.

I have now fully described my invention and indicated in general terms its uses. Though this is the first occasion of formally bringing it to the notice of the members of the medical profession, I commend it in no uncertain language. My reasons for doing this are good. During a period of nearly four years I have been engaged in practically testing its value. To this end I have applied it to a large number of patients whose cases represent all forms of spinal affections in which mechanical support can be advantageously used. The results which I have obtained in the treatment of this class of cases during this period are far better than I have ever before known to be generally obtained or have myself been able to obtain while using other forms of spinal support. Complaints of undue pressure at certain points, so commonly met with in using the plaster jacket, felt jacket, and other forms of spinal supports, have, in my experience with the wire corset, been almost unknown. I have found that it possesses a wider range of applicability as a therapeutic agent in spinal affections than any other support which I am acquainted with. In short, extended experience, close observation, earnest study and the subjective testimony of patients who have worn many other forms of spinal supports, force upon me the conviction that, for efficiency, cleanliness, comfortableness, inconspicuousness, and elegance, the wire corset has no equal.

Digitized by Google

DISCUSSION.

Dr. W. J. HERDMAN, of Ann Arbor, Mich.—I have been much impressed with the value of Dr. Roberts' suggestions, and wish that his paper could have been read before the entire Congress, instead of one Section only. The importance of the subject, the careful and painstaking manner in which he has presented it, and the admirable illustrations with which it has been attended, have made it fully worthy of such distinction. Cases of spinal curvature are not of interest solely to the specialist. So numerous are they in all classes of society and everywhere that the general practitioner must, of necessity, have more or less to do with them, and so important is it that the nature of the disease should be recognized early and the proper treatment applied, that an intelligent exposition of the causes of the distortion and of the principles underlying its treatment cannot be too widely disseminated. The primary reason why greater success has not attended the treatment of curvatures of the spine is because of woeful ignorance, on the part of many who undertake its cure, of the mechanism of the parts involved. All will concede the first essential to a proper understanding of spinal curvature is a thorough knowledge of the anatomy and physiology of the trunk of the body—a knowledge of the structure and working of the machine. Who would not think it foolhardy to place a man in charge of the locomotive of a passenger train who was ignorant of the arrangement and purpose of its varied parts? But how much more complicated and intricate, by comparison, is this part of the mechanism of the human body, with its bones, ligaments, cartilages and muscles, making not only one but many joints capable of a great variety of movements? The shoulder, elbow or hip joint is a simple structure to comprehend as compared with the spinal column, and there are few, if any, who have made a mastery of it. But a knowledge of the normal must be the starting point for a proper understanding of the abnormal, and the orthopædic surgeon, above all others, if he would be successful, must heed this fact.

The second essential in the successful treatment of cases of spinal curvature is ingenuity on the part of the surgeon in devising means for correcting the disordered action. No two cases are alike. Each has its peculiarities, and no fixed form of apparatus can be made to meet the requirements of successive cases. It is the mind and dexterity of the surgeon that controls the forces and works the cure, while the material he employs is of minor importance, provided it may be made to fulfill the conditions in a satisfactory manner.

But in the selection of a material for a spinal support it is essential that one be chosen which, while it meets every requirement for the successful treatment of the case, will at the same time be always ready at hand and within the means of all. Plaster-of-Paris has, through its intelligent use by Dr. Sayre and others, proved a most valuable agent, solely for these reasons. By means of it the surgeon's idea of treatment can be materialized by his own hand, and the substance he employs for the purpose is so cheap that it is within the reach of all who are in need of it. No material will be able to replace plaster-of-Paris as an agent in the treatment of spinal curvatures that cannot compete with it successfully in these two particulars, *i.e.*, cheapness and adaptability. It would seem that in the wire gauze or netting employed so successfully by Dr. Roberts we have a material that bids fair to replace plaster-of-Paris, since, aside from being an agent equally efficient in meeting the required conditions of support, it is, or ought to be, cheap; and certainly it is lighter, much more elegant and cleanly.

We are all indebted to Dr. Roberts for calling our attention to this material, and his admirable illustration of its adaptability to the requirements of all forms of

Digitized by Google

spinal curvatures. But those who are engaged in the treatment of cases of spinal disease must not overlook the fact that no form of spinal brace will of itself return a distorted spine to normal shape and action. With it, properly applied, much may be done to arrest the deformity, and by it something may be gained in the way of restoring weakened and diseased portions of the spinal mechanism; but at the best it is a temporizing measure, employed to gain time and arrest further progress of the deformity, while attention to the general bodily condition, massage, electricity and properly devised gymnastic exercises, all have their part to play in effecting a cure or establishing permanent improvement.

PROPER METHODS IN THE TEACHING OF ANATOMY.

MÉTHODS PROPRES À L'ENSEIGNEMENT DE L'ANATOMIE.

DIE RICHTIGEN METHODEN FÜR DEN ANATOMISCHEN UNTERRICHT.

BY A. H. P. LEUF, M.D.,

Of Philadelphia, Pa.

The object of this paper is to point out briefly the defects of the present plan of anatomical instruction, and to show that, in fact, it is not a plan, for it lacks method. There seems to be no objective point except that of cramming facts into unwilling heads, with the trite information that it is most likely to be of subsequent use.

The defects of the present method, or lack of method, may be summarized as follows:—

1. The instruction is special, thorough and complete only as regards certain organs or parts of the body.
2. It is too general as regards other organs or parts of the body.
3. It is neither the one nor the other as regards still other organs or parts of the body.
4. It is the custom to generally overlook those relations of organs to one another which, if properly taught, would be very useful to remember.
5. There is a general failure to prepare the student's mind with a proper incentive to acquire anatomical knowledge before it is imparted.
6. Too little is said of the philosophy or principles of anatomy, which, if properly taught, would make remembrance easier.
7. The failure to always clearly show the relations of anatomy to the other branches of medicine.
8. Incompetent teachers, or such who, though competent, are indifferent or have not sufficient time to do their duty.

The remedy for this state of affairs may be tersely summarized as follows:—

1. Teach the special anatomy of every organ or part of the body distinct from all others, and do it thoroughly and completely.
2. Give also a general idea or outline of each organ or part of the body.
3. Never fail in any instance to do this in the case of every organ or part of the body so far as our knowledge will admit.

Digitized by Google

4. Invariably show the true and exact relations of the parts to each other.

5. Always try to create in the student's mind a desire for the knowledge to be acquired by showing its necessity.

6. Show in a natural way, without too much mnemonics, how many anatomical principles and associations there are which, if remembered, will do alike for many or all parts of the body.

7. Give thorough instruction as to the relations of anatomical study to the other branches of medicine.

8. Obtain the services of thoroughly competent teachers, and only such as can attend to their whole duty.

My purpose now is to separately consider these summarized headings in outline so as to more clearly bring out their meaning.

DEFECTS OF THE PRESENT METHOD.

1. *The instruction is special, thorough and complete only as regards certain organs or parts of the body.*

The heart, blood and lymph vessels, stomach, gut, bladder, seminal vesicles, Fallopian tubes, uterus, vagina, gall bladder and ducts generally are all hollow, contractile organs, having essentially the same structure, and yet are pointed out to the student individually as separate and distinct organs, and as though to have studied one was not, in a way, to have studied all. Here the individualization of parts of the body is extreme and leads to wrong impressions, which too often are never removed. In the study of one or a few of these organs, an effort is made at generalization by a passing remark, for instance, to the effect that they all have muscular coats composed of longitudinal and transverse fibres, and that the longitudinal fibres are external to the transverse. Yet just here there is a failure to impress the fact that this is a general rule that affects the arrangement of muscle bundles throughout the body—transverse fibres internal to those that are longitudinal. The anatomy of the nervous system, as generally taught, is behind the times. While considerable is said about the stomach, gut and liver, the spleen and pancreas are passed over in a few words.

2. *It is too general as regards other organs or parts of the body.*

Thus, muscles are apparently described with considerable minuteness and their actions taken up in the same way, whereas, many of their important anatomical features are overlooked and an analytical study of their actions avoided. Let me instance the deltoid. It is said to abduct the arm. Some books give it credit for doing more. No book, however, credits it, so far as I am aware, with all its actions, and those are abduction, adduction, anteduction, retroduction, in-rotation and out-rotation. That it is of great importance to implant this accurate knowledge, no one who has kept abreast of modern research can deny. Great attention is given to the formation of the axillary plexus, but comparatively little is said of the exact muscular and cutaneous distribution of its branches. In the medicine of the present day, when we often have to locate pathological processes through our knowledge of the significance of anæsthetic and hyperæsthetic areas, of motor joints, and our knowledge of the nerve supply of muscles, it is of extreme importance that every one be taught accuracy in anatomy. This shows the necessity for accurate anatomical knowledge for the physician. So, also, is it necessary to know exactly the various collateral channels of circulation and the exact attachment, action and location of muscles.

3. *It is neither the one nor the other as regards still other organs or parts of the body.*

For instance, we fail miserably in reaching even the usual standard of either specific or general instruction in teaching the anatomy of the lymphatic apparatus, the sympathetic nervous system and the venous system. Especially is this true as regards their connections with each other. The failure in this instance is more with teachers

Digitized by Google

than with books. The confusing nomenclature helps to increase the obscurity of what little is taught in this direction.

4. *It is the custom to generally overlook those relations of organs to one another which, if properly taught, would be very useful to remember.*

Let me call attention to how few appreciate the small interval between the liver, stomach and spleen on the one hand, and the right lung, heart and left lung, respectively, on the other hand. How many realize the relative proportions of the œsophagus and trachea, or the relations of the inferior vena cava to the liver, diaphragm and right auricle? Again, how many can tell with reasonable accuracy the distance between the anterior and posterior lacerated foramina of the cranial base, or between the supra-orbital and mental foramina? How does a man turn his body and head half way round? What muscles do this? Is this often taught? Witness the making of a *post-mortem* examination by what is usually considered a well-informed medical man, and see his utter helplessness. See where he gropes for the kidneys or spleen, and note with what perseverance he searches for a pancreas where it never was intended to be. How many men have grasped the bodies of the lumbar vertebræ, ready to cry "Eureka!" in the fond belief that they had discovered an abdominal scirrhus! The fault here lies at the door of the teachers of anatomy.

5. *There is a general failure to prepare the student's mind with a proper incentive to acquire anatomical knowledge before it is imparted.*

This is a great and, I believe, universally prevalent defect. Occasionally, especially with reference to some parts of the body, it is done, but, as a rule, it is either wholly neglected or but feebly attempted. It is not sufficient to state that such and such knowledge is imperatively necessary to the making of an ideal physician or surgeon, but every statement should be backed by at least one good example. This failure to show a reason for the attainment of difficult facts seems to be one of the most deplorable and most damaging of the present defects in anatomical teaching. Its disadvantages cannot be over-estimated. Anatomy is usually a dry branch of medicine, whereas it should be the most interesting. Let students see why they should learn these facts. Beautify anatomy in their eyes by showing its usefulness and application. Give the students pegs of demand upon which to hang anatomical facts. The present prevailing method is to thrust at learners innumerable isolated or partly connected facts, with the repeated advice that it is knowledge that will prove of much future use; and as they advance in their studies, it is the rule not to show them how their knowledge, not only of anatomy, but other branches as well, helps them in the acquirement and retention of additional information.

6. *Too little is said of the philosophy or principles of anatomy, which, if properly taught, would make remembrance easier.*

For example, not enough attention is paid to the common structure of mucous membranes and their annexa—glands. How many students understand that glands are indented and modified mucous membranes? In the enumeration of the parts of the body developed from the blastodermic layers, no special effort is made to inculcate the principle of the development in such a way as to make it easier to understand and remember. The rule too often is for teachers, on the day before the lecture, to memorize what structures are developed from the various blastodermic layers, and then give it to their hearers like a parrot, and feel relieved when they have successfully passed the ordeal. Then, again, much more should be said than is usually done about the general plan of our bodies and their parts, and they should constantly be compared, both as regards structure and function. It is of vast importance to know the physiological relations of one part of the body or organ to another, and yet this is most imperfectly taught. The fascination of anatomy to those who feel its attraction is in the wonderful mechanism it brings to view, and the incentive it offers for the satisfactory exercise

Digitized by Google

of the reasoning faculties. To bring about a reasoning habit is, therefore, one of the many ends to be attained in the dissecting room, in addition to the common one of imparting dry anatomical facts.

7. *The failure to always clearly show the relations of anatomy to the other branches of medicine.*

Let me instance the relations of the lungs to the heart in heart disease, or of the liver to the heart under the same conditions. Again, of the alimentary tract and portal system to the liver. Also, the necessity of remembering nerve distribution and motor points by giving examples of imaginary or actual cases. Students are permitted to get and retain the idea that thorough anatomical knowledge is only necessary to the surgeon, and only slightly so, and then only in special regions, for the physician; whereas the fact is that this knowledge is equally necessary to both, and can never be either too minute, accurate or extensive. It must be remembered that these defects are lasting. If not eradicated during college life, they become permanent fixtures of one's professional character. No one can gainsay me when I affirm that the mass of practitioners fail utterly to comprehend the true relations of different parts of the body, or of the body itself as a whole, and this fault is principally due to a cause summed up in the few words—incompetent or careless instructors; at all events, defective instruction.

8. *Incompetent teachers, or such who, though competent, are indifferent or have not sufficient time to do their duty.*

Of those who are indifferent or who have not sufficient time to do their duty, it may be said that they should resign and give place to men equally as competent and more willing to do their full duty. Of the incompetent, it may be said that they should never be appointed. College faculties commit a great wrong by appointing men to teach this important branch who themselves confess that the only object in taking the position is to make capital and to compel themselves to learn anatomy to the extent which seems to them desirable or necessary. This is an imposition upon the students, who confidently place themselves in the care of a trusted institution for the purpose of acquiring a medical education at a par with the medicine of the times.

THE REMEDY.

This is simple enough, and consists in overcoming the defects just indicated. For this purpose, it is not only necessary that teachers of anatomy be good practical and thinking anatomists and *students*, but that they be philosophical as well as practical physicians in every sense of the word. At no time in a medical education is instruction so potent for future usefulness, or its neglect so certain of ultimate failure, as the work done in the dissecting room and the quality of the anatomical lectures before the class.

1. *Teach the special anatomy of every organ or part of the body distinct from all others, and do it thoroughly and completely.*

While in following this plan the student may not recall all that is told him, he has the benefit of thorough instruction, and enough will remain to leave a clear outline around which future studies will cause to crystallize those additional facts which at first failed to secure a hold. Thoroughness and repetition are of prime importance in teaching this difficult branch. Innumerable facts are not memorized with ease and never to any decided advantage unless their relations to each other are mastered in the smallest detail. The instruction covering any part, therefore, should be specific, detailed and thorough.

2. *Give also a general idea or outline of each organ or part of the body.*

This is necessary, as it emphasizes the most important facts to be remembered, and thus insures the attainment of the main object of the preceding paragraph. Of mucous membranes, for instance, it might be said that they all are composed of thin layers, *i. e.*, a superficial one of cells, a middle one or basement membrane, and a deep one or

Digitized by Google

submucosa. In the liver, the five fissures, five lobes, five ligaments and five vessels could be briefly summarized. In the thigh, a single mention of its form could be followed by the statement that it is composed of a bone, muscles, vessels, nerves, loose connective tissue, fat, fascia and skin. The bone is the femur; the muscles being the four leg extensors, four adductors, two flexors of the thigh, a superficial group of three, three extensors of the thigh, six out-rotators, and four flexors of the leg; the fascia lata and its intermuscular septa; the anterior and posterior sets of vessels; the anterior and posterior nerves; the loose connective tissue and fat variously predominating in different portions of the thigh, notably in the groin and upon the inner side; and lastly, the skin over all.

3. *Never fail in any instance to do this in the case of every organ or part of the body, so far as our knowledge will admit.*

By adhering to this rule it is much easier to show the relations of different parts of the body to each other, and so make the whole mechanism more easily comprehensible. It also lessens the chance of students receiving false impressions of the relative importance of parts of the body. It is natural to suppose that those organs that are most thoroughly explained are the most important, whereas this rule by no means always follows. Because it is more difficult to teach the anatomy of certain organs or regions is no reason why the task should be shirked. A true and competent teacher would be all the more anxious to devise means for its simplification, so as to bring it before students in a manner to make it easy of comprehension and remembrance.

4. *Invariably show the true and exact relations of the parts to each other.*

This facilitates still more the comprehension of the whole mechanism. For instance, how many students ever have it impressed upon their minds that the upper fibres of the pectoralis major and the anterior fibres of the deltoid act alike? How often is a student made to realize the difference in the action of the anterior, middle and posterior fibres of the deltoid? Again, the same may be asked of the upper and lower fibres of the pectoralis major or trapezius. Note the great similarity of action between the posterior fibres of the sterno-cleido-mastoid and the anterior fibres of the trapezius, and their being both supplied by the one nerve. Are students ever permitted to realize that the deltoid is a downward continuation of the trapezius, and that both muscles may be viewed as one large, triangular, fleshy mass, having its base of origin from the posterior median line of the body from the occipital protuberance to the last thoracic vertebra, whence its fibres converge over the shoulder, forming its cap, to be inserted into the middle of the outer side of the humerus? In its course over the shoulder, it has the bony interruption of the shoulder girdle. Anatomy taught in this way has its fascination. Many and many a dull man have I seen develope a lively interest in the work of the dissecting room after pointing out facts of this kind and affording an opportunity for the exercise of reason. We are too apt to teach that the action of muscle is to produce that effect which equal contraction of all its fibres necessarily yields, but this is fallacious. Parts of muscles may and do contract independently. Contiguous borders of muscles contract while the remaining portions are passive. This is not generally taught, but should be. These four illustrations suffice to show my meaning, and similar ones may be had from all parts of the body.

5. *Always try to create in the student's mind a desire for the knowledge to be acquired by showing its necessity.*

This can be done by supposing fictitious cases or citing actual ones, preferably such as have been seen by the students themselves. Suppose a fracture of the humerus, with a large effusion of callus, and consequent compression of the musculo-spiral nerve, and the incidental wrist-drop and radial anæsthesia. Suppose muscular atrophies and disease of motor or sensory nerves. Suppose disease of the cerebro-spinal axis. Give symptoms and exercise the student's mind in locating the lesion, or suggest a lesion

Digitized by Google

and let the symptoms be worked out by the dissector. Suppose, again, an oral hemorrhage, and then let the student say, if possible, whence the blood came. Show then the different places from which it may have come. They can be taught how a nasal hemorrhage may give rise to bloody vomit, and will be only too glad to master the anatomy of the nose, and learn how to plug it in case of nose-bleed. In this way they will be anxious to learn, will do it readily, and will retain it better than in the usual way. They can be told that a bronchial hemorrhage does not necessarily imply phthisis, and then they will be only too eager to learn how this can be a symptom of heart disease. These examples, I hope, are enough to clearly convey my meaning.

6. *Show in a natural way, without too much mnemonics, how many anatomical principles and associations there are which, if remembered, will do alike for many or all parts of the body.*

Thus, for instance, all organs, whether bones, muscles, or viscera, are composed essentially of a supporting framework of connective tissue containing essential cells, nutrient vessels, coördinating nerves, and sometimes efferent ducts. Again, to quote from a former paper of mine,* "It is an invariable rule for muscles to act on every joint they pass over." "It is evident, also, that the action of a muscle on a joint depends upon its relations to that joint." Now, inasmuch as moving a joint will often change the relative positions of it and its motive muscle, it follows that the actions of muscles may change during contraction, or act either with increased or diminished effect. These will suffice as examples of principles. I will instance associations, as follows: Each bicipital ridge has attached to it a muscle; to the inner, or posterior, the teres major and to the outer, or anterior, the pectoralis major, *i. e.*, a "major" to each ridge, the anterior ridge having inserted into it the anterior or pectoral "major" muscles, while the posterior ridge gives attachment to the posterior or round major muscle. The association is a simple one. Again, take the scapula; it is *three* cornered, has *three* borders and *three* great fossa. (Supra-spinous, infra-spinous and subscapular.) Attached to it are *three* muscles arising from these fossæ and bearing corresponding names. (Supra-spinatus, infra-spinatus and subscapularis.) The anterior border gives attachment to a group of *three* muscles (teres major, teres minor and triceps). The posterior border affords insertion to another group of *three* (rhomboideus major, rhomboideus minor and levator anguli scapulæ). The coracoid process gives origin to *three* muscles (short head of biceps, coraco-brachialis and pectoralis minor). Then there remain *three* unclassified muscles (long head of biceps, omo-hyoid and serratus magnus). There still remains a *pair* of processes (coracoid and the acromio-spine) and a pair of muscles (deltoid and trapezius). Another association is noticed in this way: Going from below upward are both the anterior and posterior scapular border; the first muscle is a "major" and the second a "minor," being the teres muscles in front and the rhomboids behind. Associations of this kind are not too artificial, aid remembrance, and are possible in all parts of the body.

7. *Give thorough instruction as to the relations of anatomical study to the other branches of medicine.*

Take the liver, for example. Show its structure, and let the student realize that the blood of the whole alimentary tract, from the cardiac orifice of the stomach to the anus, inclusive of that from the spleen and pancreas, must pass to the heart through the capillaries of the liver. Show clearly that in doing so it must pass through the central vein of the lobules. Show also that every lobule is surrounded by connective tissue. Explain that if this tissue is inflamed, it proliferates, increases and eventually contracts, and that in doing so it compresses, squeezes, the lobule and its central vein,

* The paper is entitled "Additional Factors Concerned in Fractures of the Humerus in the Vicinity of the Elbow Joint," and appeared in the *Annals for Anatomy and Surgery*, July, 1881.

Digitized by Google

and so obstructs the flow of blood through the liver to the heart. Tell them that this compression or constriction may become so excessive as to lead to marked damming back of blood in the veins of the alimentary tract and cause turgescence, elongation and convolution of the veins; hæmorrhoids; gastro-intestinal hemorrhage; alternating constipation and diarrhœa; anasarca; inevitable failure of the digestive power; loss of appetite; falling strength; and ultimate death. Thus, the study of anatomy would become objective and interesting throughout.

8. *Obtain the services of thoroughly competent teachers, and only such as can attend to their whole duty.*

The too prevalent custom in this country is to appoint dissecting-room teachers from among those who admittedly only desire to learn a little anatomy themselves, and who want to use the demonstratorship as a stepping stone to what, in their eyes, is a better position. The dissecting room is the place where can be laid to best advantage the foundation of the medical education of every student. It is, therefore, of the utmost importance that demonstrators be thoroughly competent. The importance of the position in this country is far below what it is said and appears to be in Europe. Our practical anatomy is too often in the hands of incompetents, who are appointed to their positions through injudicious and vicious influence, at the cost of such as have the requisite ability and would give sufficient time to the scientific advancement of this branch, besides teaching it in a manner making it a pleasant, in fact a fascinating, study. It is an outrage upon the students who confidently pay their money, as well as upon the public at large who will have to employ them when they graduate, to entrust the very fundamental work of their medical education with those who have hardly begun to realize the scope and depth of medical science. Time and again have I seen demonstrators compelled to "read up" for special demonstrations, or even to correctly answer an ordinary question, for instance, as to the attachment of a muscle or the branches of a blood vessel. That the existence of facts like these is a disgrace, no one can deny. That every such appointment is a fraud upon the student and community, all must admit. That it lowers the grade of institutions that tolerate it, is inevitable. That it keeps ambitious, competent and able talent in the background, and thus retards the advancement of anatomical science, goes without saying. Weed out the incompetents; appoint the able, willing and ambitious; remove these when their ardor cools or when their attention is diverted by other interests, or make it to their advantage to devote themselves exclusively to the work for which they have proven their fitness. Good teachers are worth having at even considerable expense, and not over-plentiful at any time Never appoint an incompetent to teach anatomy, or for that matter any branch, but train or grow the teacher. Prove fitness before appointment, and then let the position be first held for a definite period on trial. The excuse that there are not enough in our ranks of those who like or are well versed in anatomy is fallacious, for there are many.

Digitized by Google

SECTION VIII—PHYSIOLOGY.

OFFICERS.

PRESIDENT: JOHN H. CALLENDER, M. D., Nashville, Tenn.

VICE-PRESIDENTS.

J. ROSE BRADFORD, M.D., London, England.
RICHARD CATON, M. D., Liverpool, England.
DANIEL CLARK, M. D., Toronto, Canada.
AUSTIN FLINT, M. D., New York, N. Y.
WM. DOBINSON HALLIBURTON, M. D., London, England.
C. H. HUGHES, M. D., St. Louis, Mo.
WM. ABRAM LOVE, M. D., Atlanta, Ga.
JOHN ALEX. MCWILLIAM, M. D., Aberdeen, Scotland.
H. P. STEARNS, M. D., Hartford, Conn.

SECRETARIES.

RANDOLPH BARKSDALE, M. D., Petersburg, Va.
R. W. BISHOP, M. D., Chicago, Ill.
EDOUARD FOURNIÉ, M. D., Paris, France.
BERNARD FRANKEL, M. D., Berlin, Germany.

COUNCIL.

O. Everts, M. D., Cincinnati, Ohio.
W. Hutson Ford, M. D., St. Louis, Mo.
J. Henry Jackson, M. D., Barré, Vt.
C. H. A Kleinschmidt, M. D., Washington, D. C.
T. S. Latimer, M. D., Baltimore, Md.
Thomas O. Summers, M. D., Jacksonville, Fla.
A. Wetmore, M. D., Waterloo, Ill.
J. H. Wythe, M. D., Oakland, Cal.

Digitized by Google

FIRST DAY.

The Section met in the Physiological Lecture Room of Columbian University, on Monday, September 5th, 1887, at 3 P. M., and was called to order by its President, Dr. J. H. CALLENDER, of Nashville, Tenn., who delivered the

INTRODUCTORY ADDRESS.

GENTLEMEN:—In formally inaugurating the proceedings of the Physiological Section of the Ninth International Medical Congress, it affords me great pleasure, in the name of the workers in that field of medical science in this country, to tender an earnest and cordial greeting to those present who have come from other countries to contribute to the information expected to be imparted in those proceedings, and to afford interest to its discussions, and to participate in this great convention of medical men representing many peoples and all departments of our extensive science. This salutation is as comprehensive in its scope as the area from which you have journeyed, and as sincere in its expression as the spirit which has moved you to come. I hope that the exercises through which this Section may pass will in some measure repay you for the journey, and form an agreeable and profitable memory to you in the history of this Congress and the recollection of your visit to the Capital city of the United States of America. We are assembled not only as devotees of science, hoping to derive mutual benefit from the interchange of opinions on themes of professional interest, and from new researches into the mines of scientific store to be unfolded, but we are here as fellow-men, bonded in the sympathy of common labors and pursuits, and entertaining for each other sentiments of esteem flowing from the elevated calling of which we are members. I earnestly trust the former expectation may be amply realized, and that the estimation in which we hold each other may be enhanced by the personal contact and intellectual and social enjoyment which will be felt in the sessions of this Section and of the Congress; and that the occasion may be instrumental in forming valued friendships between those heretofore known only by name and character, and of strengthening the ties of those heretofore formed. In such assemblages, the scientific object is hardly more important than the amenities and humanities with which it may be mingled, and it cannot but be, that all of us—whether foreign or native here—shall come to know, in this current week, that the "bright consummate flower" of these can be cultivated to the highest perfection in this representative city of enlightened, prosperous and hospitable America.

Since the Presidency of this Section was assigned to me by those in charge of the organization of this Congress, it may not be out of taste for me to say, that in undertaking its duties, a more than usual pressure of multifarious official duties has oppressed me with more than a usual sense of my unworthiness of the distinction it confers and the responsibility it imposes, and that I have been painfully distrustful that its work, under the imperfect management it has received, would fall short of

Digitized by Google

the just expectations of those who might attend its sessions; and at times have felt I should have obeyed the first impulse and declined the duty, knowing it could have been more capably performed by many of my brethren more justly entitled to the honor of the position. Under this feeling, I have only been sustained by the generous aid offered in the task of organization, and by the knowledge that in the discharge of my functions now, I shall have the coöperation of the accomplished men who form the roster of Vice-Presidents and Council. Until almost the latest moment before leaving home, other engagements have so taxed my time, that it seemed the only contribution possible to the meetings of the Section I could make, and perhaps the best to be made, would be the performance of the routine duties of the chair and a respectful listening. A proper sense, however, of the trust and dignity of the position, has impelled me to prepare an introductory address, in some degree appropriate to this opening Session, the brevity of which, at least, will perhaps be appreciated, and a pardon for these personal allusions, I hope, may be, at the same time, granted.

This Congress has met to consider the present state of the science of medicine—to promote its advancement as a pure science and its perfection as a practical art—and to consult of its interests as one of the highest professions which can enlist the thought and energies of men. Its general meetings will, in some respects, doubtless, pass under review many of the features of these important subjects, in which all of its members are immediately and deeply concerned. Those of us, however, who compose the body of this Section, and are entrusted with the conduct of its exercises, in the subjects to be discussed here, are more directly concerned with medicine in its strictly scientific relations. Medicine, to speak accurately, is a mixed science and art for the cure and relief of all physical disorders and infirmities. As a science, it is an aggregation of different species of knowledge, or separate sciences, and that of physiology, or the conditions and laws of life and organization in a state of health, and of the functions of tissues and organs, is one of the cardinal and indispensable components of medical science. Until by the accumulated labors of many generations of observers, there was a systematic body of knowledge, more or less exact, of the functions of organized bodies, medicine was nothing more than an unqualified empirical art, and could not be otherwise. Indeed, while it is true that a knowledge of anatomical and histological structure is necessary to a knowledge of the functions of organs and tissues, the latter is even more necessary as an equipment for the scientific physician, and only as physiological science has developed, has medicine positively emerged from the mists of blind experiment, and become securely established on a scientific foundation. The great events in physiological discovery have specially marked great epochs of advancement in scientific theories and modes of cure. It may be claimed, then, that in the group of natural sciences from which the profession of medicine receives its most reliable strength as a scientific practical agency, physiology is outranked by none, and that in the great muster of sciences which this Congress parades to-day, it must be conceded to march with the vanguard.

Many intelligent unprofessional persons who may observe the proceedings of a Congress like this are moved to inquire why medicine, which the general mind is accustomed to regard as a unit, should be so subdivided into departments or sections; and there be conservative critics in the profession, who question the wisdom of what they fear may lead to conflicting division of studies and an injurious diffusion of energies. The trend toward specialism is indeed strong and increasing, but notwithstanding the liability to become excessive, and the danger that physicians in pursuit of special lines of work, may become shallow in the fundamental principles of the

Digitized by Google

science as a whole, and consequently narrow in views, there are yet cogent reasons impelling this movement. It is not in the scope of an address of this character to set these forth, and this digression may end with the remark, that the body of knowledge under the term medicine is becoming so copious and complex, that it requires diversity of cultivation that it may be fully mastered—no one mind, however endowed, being equal to the task. In whatever degree such criticism may be applicable to some features of the organization of this and previous similar Congresses, the relationship of physiology to every possible refinement of practical medicine is so intrinsic and essential, its students and cultivators, are, by necessity, recognized as forming one of the grand divisions of the comprehensive science, and are properly assembled in a Section, from whose exercises and discussions all others may possibly derive facts and suggestions of great value.

Our science—biology and physiology—for generically and strictly they are one—resting on histology and organic chemistry, under the exquisite modern methods of research constantly being rendered more accurate and subtle, can now legitimately claim to be a true science, answering the Ciceronian definition—"cognitio certa ex principiis certis deducta"—a body of approved facts, rather than a system of imperfect theories and ingenious doctrines, with here and there only an indisputable fact. Mainly it is above such obscuring fogs, resting in the sunlight. The great mass of its data is sound, proven and germinal in its character, fertilizing every department of medical science and producing a fruitage so wholesome and perfect, that the genuineness of the seed—the truth of the facts—is therein fully vindicated. We have advanced to that point when we no longer speculate, or vainly search for the principle of life, but are wisely content and more profitably employed in investigating the conditions and phenomena of living beings. The results of this philosophical mode of prosecuting the study of physiological truth, has afforded scientific basis for pathological deduction and therapeutic agency. As we have been enabled to know the true molecular and atomic structure of tissues and their functions and normal processes, so we have been enabled to perceive with greater clearness their disturbances which constitute disease, and so also are we enabled to measure with greater science and skill the qualities and force of remedial resources for their rectification and relief.

In any reference to physiology and its inexpressible importance as the basis of a true science of medicine, it would seem impossible, as indeed it would be ungrateful, if not almost irreverent, not to date its own true scientific origin and development in the discovery and verification of the circulation of the blood by the immortal Harvey. That became a bright light and landmark, and indeed a condition precedent to much we have learned, with slow and patient labor, in the two and a half centuries since, a very great deal of which is directly traceable to and immediately dependent on that cardinal fact. It is to physiology, in some sense, what the law of gravitation is to physics. None perhaps will question that the supreme and commanding fact in the physiology of this century, and of to-day, in its potential consequences on the progress of medical science, is the function of the cell in all forms of life, vegetable and animal. The full history of its promulgation and demonstration and the attempt to award the honor therefor, is not pertinent in this cursory allusion. Like many other of the greatest of the achievements made in natural science, the horizon was gradually illuminated from it long before the intervening peaks which obstructed its brilliant and vivifying rays were surmounted. The genius of Bichat, at the opening of the present century, first caught a clear glimpse of the field wherein lay this invaluable gem of scientific truth. His great work on "General Anatomy" and Treatise on

Digitized by Google

Membranes," philosophically evolved under laborious investigation of facts, while it cut him down in the morning of life, marked one of the most signal advancements in human knowledge of which the century has since been so prolific. The microscope, first under the eye of Schleiden in plants, and soon after that of Schwann in animals, and since by a host of observers, followed the magical scalpel of Bichat, and now reads the marvelous susceptibilities and metabolic characteristics of cells as the alphabet and cypher to nearly all the phenomena of vital activities of which we are cognizant.

Though with the rise of what may be called modern physiology, in the sixteenth century, the fantastic dreams of the ancient alchemists conceived, in the crude, early days of chemistry, that there was resident somewhere in the human organism a subtle essence, or force of life, for which their mystic art could find an elixir rendering it perpetual, were effectually dissipated, still, eminent scientific minds, who led the thought of their time at successive periods until a comparatively recent one, continued to hold the theory that, in some form, animal bodies contained a sort of regnant archeus president over the mechanism of life. The demonstration of the cell and its functions has scientifically exploded the idea which underlaid alchemic absurdity, and equally disproved the more enlightened type of error. It has wrought a thorough revolution in our conceptions of life. Instead of being regarded as an entity whose sum was liable to constant waste, which, if unarrested, would result in final expenditure or death, or a conservative principle stored up in some great organ or apparatus of organs, the degeneration and decay of either of which must be stopped in order to combat disease or to preserve life, we now know that this degenerative alteration —this incessant decay—is life itself, and that the perversion of the order and character of these perpetual changes is disease, and that their total and universal arrest is death. We know that the sum of the life of an organism is the myriad lives of the minute cells of which its tissues are composed, and that the changes in these cells are metamorphoses of their structure in which they are constantly dying and being reproduced. Such and so great has been the transformation in biological science by the discovery of this notable fact and its laws.

How important and profound have been the results of its application to medicine. The existence of the normal physiological cell and its properties opened the vista of a new pathology, and dispelled every cloud and fleecy rack of the obscurity of the animistic, humoral and visceral theories. The cellular pathology, which is so clearly unfolding and elucidating many of the problems of disease, was a scientific induction from the physiological theory, and the accumulating array of facts in each is corroborative of the truth of both, and that the cell doctrine is a certain and enduring addition to the body of our knowledge of the laws of living bodies in health and disease.

Not alone in the sphere of pathology proper have the principles of this doctrine touched in its effects on practical medicine. Within the last decade or two, the parasitic or germ theory as to the causation of diseased states and conditions, though long before speculated upon in the manner of hypothesis or conjecture only, has fixed the attention of investigators, and the discovery of these microscopic vegetable and animal bodies in living organisms and the unquestionably important relation they bear to the generation, or development by presence, of cell disturbances specific in their character, is a scientific fact yearly coming to have fewer gainsayers or doubters. Whatever of solid and permanent acquisition in the field of the ætiology and prophylaxis of disease that may accrue to science from the labors of Haller and Koch and Obermeyer and Pasteur, the initial point of their profoundly interesting

Digitized by Google

inquiries must be conceded to be found in the new laws of biology and organic chemistry established in physiological investigations.

In the realm of hygiene and remedial medicaments, the newly-developed physiological data traceable to cell metamorphosis and modifications are making a profound impression. The efficacy of external antiseptic agencies, which, under Lister and his associates, has so strengthened the hand of the surgeon, is in great part due to laws whose clues were found first in the physiological laboratory, and whatever now of promise may yet be accomplished in antiseptic measures applied to the blood must acknowledge the same guiding finger in its scientific results. And, again, within the last half-century a considerable amount of accurate and positive knowledge of the rationale of the action of a large number of the articles of the materia medica—vegetable and mineral—have been only elucidated and verified by experimental physiology.

But this discursive reference to the fundamental importance of this branch of science to the great organic body of medical science this Congress has assembled to represent, is unnecessarily extended in the enlightened presence before me. The utterance of the claim preferred will doubtless, in the main, be sustained by all, if not entirely for the reasons advanced in so desultory a manner. It has been the object merely to emphasize the truth, that as the science and art of healing in all its branches progresses, it becomes more obvious that its true philosophical line of advancement lies primarily, and in no degree subordinate to any other, in the field of work occupied by this Section.

If the attempt were made to further fortify this title to a first position among the cultivators of medicine, by citing the eminent names in physiology which have illustrated the annals of that science, it would require me to recite, in great part, its history. From the "De partibus animalium" of the great naturalist and philosopher, Aristotle, whose pregnant writings on whatever subject his extraordinary mental endowments were employed incited among the ancients, and indeed all coming after him, the thirst for knowledge, down to the learned men who, during the sessions of this body, are to continue inquiries into the same subject, our special science displays a galaxy of ability and achievement unsurpassed in any other sphere of intellectual effort by the human race. Their names are as familiar to you as household words, and this allusion to them passes the long line in procession before the mind. They represent all past and present nations, tongues and literatures, as this body represents in its composition those living, and their united contributions are before us, for taking an account at this day of what we know of life and its phenomena, and for measuring as best we may what we do not know, and, in this finite state, may never know of those stupendous problems.

The programme of subjects for consideration and discussion at the several sessions of this Section, as submitted through me to the Council and approved by that body, will be found of great interest, not only in the fact that each one is of leading importance, but that an entertaining variety is offered. It would be violative of propriety and good taste to remark at length upon any of these in anticipation of the discussion they will elicit. It may not be improper, however, to briefly allude to each, or to those which may be naturally grouped.

We shall have before us the blood in some of its most interesting components and characteristics—the function of the red corpuscle and of its coagulating element, and the relation of that physiological process to a peculiar post-mortem condition.

Digitized by Google

The problem of nutrition and its forces acting under mediate and collateral conditions will be presented, and the trophic influence of the nervous system will be considered in further elucidation of a process which, in some degree, may be held to be the leading object of all other functions of the organism.

The first of the secretions of the alimentary tract in observation of its properties and uses—local and general—as seen in some of the lower order of animals, will be offered; and as more or less cognate to processes thereby commenced, the ultimate evolution through further chemico-physiological changes of alcohol in the organism.

A paper on the physiology of elemental fibres, and one analyzing the soundness of certain prevalent doctrines touching the inter-relation of nerve and muscle fibres, will form an interesting topic in the exercises.

The par vagum, with its extensively ramified anatomy and its multifold and profoundly important physiological connections and sympathies, will claim that share of your attention the discussion of its different functions demands.

We shall have the electrical force, whose phenomena in relation to living bodies are yet so recondite, but which, even in their imperfect development, intimate such wonderful possibilities, brought to view in a paper on their presence in cerebral gray matter, and one also in its action on the heart.

Two papers touching the highest function predicable of organic matter—its psychical expression—will complete the catalogue of subjects. One will interrogate the basal ganglia, and the other the hemispherical ganglia in this supreme relation of matter to mind. Both, it may be assumed, will apply the physiological method of inquiry to the modes by which mental phenomena are produced, and in this view we shall be brought to contemplate the cell energies of the cerebrum and its connections as the seat and source of intellection, volition, emotion, and self-consciousness, and be made to appreciate Gœthe's sublime thought, that man is so fashioned and endowed as to be the medium of the first and only dialogue that nature has ever held with God.

It will be noted that the physiology of the nervous system in some of its divisions and functions, and electro-physiology, so closely connected, as it probably is, with nerve functions, will occupy a large portion of the attention of the Section. This may be fortuitous only, or it may be due to the fact that recent physiological inquiry has been, in a great degree, so directed, and that in a body of representative physiologists at the present time it should legitimately offer itself for full consideration. Whatever may be the cause or causes, it may be safely asserted, that within the professional memory of all who hear me neuropathic disorders have increased in number and variety. Whether this be attributable—to borrow a military term—to the rapid mobilization of human energies which have ensued from the activities in all the concerns of life, set in motion by Puck's girdle of communication around and in every direction over the realm of civilization, with the speed of locomotion which supplements it and stirs every fibre to its utmost tension, will only be mooted as I pass to a conclusion. Certain it is that "the wear and tear" of what Prof. Huxley, in his fine army figure of the human organism, calls the "headquarters and field telegraph," has been great within that time, and its results have taxed the diagnostic skill and therapeutic resources of practical medicine. Neurological investigation has moved with the consequent requirements for more accurate and scientific insight into the wondrous relations and sympathies of that most intricate and delicate apparatus, and the localization and explication of its functions is, at this time, equipping the surgeon and physician with most valuable suggestions of treatment and remedy.

Digitized by Google

But, gentlemen, I am transgressing the limit promised you. Repeating the general welcome extended, and presuming to express the sense of pleasure which inspires us all, I now declare this Section of the Ninth International Congress opened for business.

Dr. Daniel Clark, of Toronto, Canada, read a paper entitled—

THE BASAL GANGLIA OF THE BRAIN AS PSYCHIC CENTRES.

LES GRANDS GANGLIONS CERÉBRAUX COMME CENTRES PSYCHIQUES.

DIE GRÖSSEN GEHIRNGANGLIEN ALS PSYCHISCHE CENTRA.

The following points are to be discussed :—

1. The radical difference found in the circulation of the blood, both as to mode of distribution and quantity, suggests the reasonable inference of greater functional activity existing in the centre than in the circumference of the brain.

2. The want of uniformity in psychical and functional results when definite and similar portions of the cortical substance are stimulated, impaired or destroyed ; hence exists no positive proof that this is the seat of so-called true motor centres.

3. It would be consistent with pathological and experimental facts to consider that these motor and psychical centres are located in the base and central ganglia, yet in sympathetic relations with, being influenced by, but not absolutely controlled by, the cortical substance.

4. The localization of function in the brain surface ignores the fact that there is a want of distinctive physiological features in the several convolutions.

In taking up the first of these statements, let us remember it is an axiom that the more life action in any part, the more is blood supply needed. In studying the brain circulation, we find that the centre and base are much more plentifully supplied with blood than are the cerebrum and cerebellum. This is a striking fact, when we remember that all the parts constitute one organ, with multifarious functions and offices. Let us remember how direct and ample is the blood supply to the base and central ganglia in comparison with the cortical supply. This is especially true of the quantity which is furnished to each *corpus striatum* and *thalamus opticus*. The cortical substance is nourished in a round-about way through the *pia mater*, but the central system is reached directly through the large vessels springing from the circle of Willis and the cerebral arteries shooting upward and inward from this polygon of vessels. These furnish a perfect and copious fountain of blood supply near at hand. These two sources of supply are not only distinct as between each of these groups, but also unconnected, to a great extent, with one another. The anastomoses between these two sets of vessels is very slight indeed. The streamlet in each can be dried up or seriously interrupted in many ways without disturbing the neighboring vessels to any appreciable extent. This accounts for so many circumscribed lesions in the cortical parts, and for the little effect their proximity produces on the adjacent tissues and circulation. I am inclined to think that, on account of this localization of circulation, and consequently, a tendency to restricted areas of disease, a good many fallacies of reasoning have obtained currency in respect to cortical centres of function. Heubner cites a large number of pathological cases which prove conclusively that obliteration of any one of the large vessels of the

Digitized by Google

cortical system or any of its branches has, during life, given no pronounced abnormal symptom to indicate depreciation of function. (Charcot.)

Let us now turn to the arterial circulation in the *gray central ganglia.* It needs only a moment's reflection on our knowledge of Anatomy, to remember that the basal ganglia are largely supplied with blood from the Sylvian artery, as well as from the nutrient vessels which spring in large numbers directly from the great central reservoir, and from the basilar at its bifurcation. The sum total of the calibre of those vessels which go to the basal organs, shows a much greater capacity for blood supply per inch square, by double the quantity, than does any other part of the brain. Such being the case, we know this augmented blood supply, being normal, means proportionally increased functional activity. Hence it follows, as a matter of fact, that any abnormal increase or decrease of blood circulation which will disturb the natural equilibrium always brings about more or less physical and mental perturbation. In this respect congestion, equally with anæmia, is followed by analogous results; that is, more or less suspended sensibility, impaired voluntary action, and conscious deficiency in mental activity. It is my impression that, because of this inequality in these vital centres, do we find the important pathological fact that, in hemiplegia from cortical disease it is "limited, transient, and variable" (Charcot), but in paralysis of the body from disease of the centre or base organs of the brain it is permanent, general and uniform. It is a pathological fact that, general or partial paralysis is produced by *any part* of the brain becoming affected with inflammation, embolus, tumor, or exostosis. So distinct and important is the circulation in these small brains at the base, that when the partial or complete obliteration of the middle cerebral takes place all the ganglionic centres become immediately seriously affected, and cerebral hemiplegia, accompanied by hemianæsthesia, is the result. This fact alone shows the greater importance these ganglia hold as functional centres, in comparison to the cortex, or even the entire hemispheres. Professor M. Schiff, of Florence, has caught the same idea, when he says, in his monograph on "Motor Centres," that "human and comparative pathology have stated, with certainty, that the motor centres do not extend above the base of the brain."

A sharp controversy is being carried on and antagonistic opinions have been uttered by the leaders of thought in our profession on the functions of the convolutions of the brain. These have been mapped out by imaginative surveyors with the accuracy of the streets of a city, and each district has been declared to be a specialized centre of functional energy. Although no dividing lines exist in the substance of the brain, yet the comparatively slender divisions of the sulci are made to do duty as boundaries of organs in which great differences of operation are said to exist. These observers seem to forget that in mammalia next higher in order to man these outlines are very feeble, yet these animals have many physical functions of a higher capacity than have the *genus homo.*

All anatomists know that although the outward depressions on the brain of man maintain a certain degree of uniformity in direction and outline, yet the differences in detail are considerable. These fissures do not make distinct and complete division of the surface. The even continuity of the surface of each convolution by an isthmus (so to speak) at the extremities and sides of each, indicate no striking dividing lines, but rather the contrary. The dips in the gray matter underlying these fissures and in proximity to the white substance show that a certain degree of uniformity in quantity of gray matter exists throughout the periphery of the brain. It is true that differences in cell formation are seen in the various layers of the cortical substance, but these cellular distinctions are found only in each layer lying parallel to the surface. There is no physiological distinction found in one convolution distinct from another. The uniformity of cell structure in the several layers of the cortical substance is continuous, and nowhere bounded by the surface fissures and convolutions. In other words, all

Digitized by Google

the convolutions are similar in structure. Were sections of any parts of the cerebrum cut out from without toward the centre and submitted to the closest analysis no microscopist could tell, from the construction of such sections, where each topically belonged. The same is true of the structure of the cerebellum.

The various shaped cells are uniform in texture, in whatever layer they may be found, and there is no convincing evidence to show that large and small, round and ovoid, angular and caudated, deep and superficial, transparent and opaque, are not members of the same family in different stages of development.

It is also to be remembered that there is no direct nervous communication with the body from the cerebrum and cerebellum except through the basal ganglia, notwithstanding statements to the contrary. Whatever injury disease or traumatic lesion may inflict on these upper nerve masses with comparative impunity, so important are the centre and base ganglia that similar abnormal conditions cannot be found in these without dangerous results.

These are, in my opinion, the true motor and psychic centres of the system. Physiology teaches this, and there is no necessity to go beyond them to prove a localization theory. The distinctness and variety seen in the base ganglia, with well-defined boundaries, and the want of uniformity in structure, point strongly to specific functions. The juxtaposition of the *medulla oblongata*, the spinal cord, the nerve ramifications not only to the organs of special sense but also to the locomotive and organic systems, point out these districts as being peculiar and focal centres. If this theory be correct it can explain all the phenomena manifested through experimentation and pathological conditions, without resorting to the chart based on such shifting, incomplete and varied boundaries as the sulci of the convolutions afford.

Let us consider this point in another aspect. Fritsch, Hitzig and other experimenters, agree that in no appreciable degree do mechanical or chemical agents excite motion in the cerebral substance. Excitation by galvanism is said to be very feeble and very limited in either cerebrum or cerebellum, and this want of response is seen throughout. It is evident that by this powerful agent no functional centre could be found. Herrmann shows that even after the gray matter is destroyed by chemical cauteries, a very feeble current of galvanism applied to the abraded surface produced only a slight movement. He significantly adds, that in cutting away slices of the brain the effect was more decided in proportion as the *central regions were approached.* (Richet.) In other words, the foci of nerve energy seemed to be in the ganglia at the base of the brain, and that the destruction of the cerebral substance did not produce that disturbance of the system commensurate with the loss of such tissue, once supposed to be so necessary to the continuance of physical function and mental action.

Richet says, in speaking of the localization theory as propounded by Ferrier and his ardent followers, that "absolute inflexible localization of the motor zones is impossible. There are zones which encroach upon each other, but none of these zones have limits of determined vigorous constancy. The best proof of this is the difference existing among authors." If this statement means anything, it is that although paralysis and abnormal functions, in many instances, follow the destruction of certain cortical parts, from experiment or as the result of disease, and although a certain degree of uniformity in physical results may follow, yet it is equally true that these same areas may be destroyed without any such manifestations following. Their own experiments are taken as proof of this statement. These circumscribed areas cannot, therefore, by any show of reasoning, be *the* organs which are the centres of distinct functional activity. These upper nerve masses doubtless are the centres of *general* power and activity, but the functions for coördination and mental scope must be sought for elsewhere.

The unsatisfactory nature of their own theories seems to have received the attention of these able and earnest workers. Richet, in speaking of cerebral excitation by means

Digitized by Google

of electricity, is forced to say, in explanation of certain phenomena, "Known facts demonstrate that excitation of the convolutions which surround the *sigmoid gyrus* act with extreme energy upon the ganglionic centres of the brain (opto-striated). It is possible that such excitation culminated in the cerebral centres, and that these centres thus surcharged discharge to the muscles." Charcot says, in speaking of the lenticular nucleus of the *corpus striatum:* "These gray nuclei are possibly so many centres endowed with distinct properties and functions." Dr. Symond, in the Gulstonian lectures, says: "Pain does not seem to be in the nervous matter, whether vesicular or tubular, of the cerebral hemispheres, or of the cerebellum. No evidence of feeling has been obtained by vivisectors till they approached the sensory ganglia, the *thalami optici* and *corpora quadrigemina.* But these are the centres of sensation to all parts of the body as well as to the head."

Maudsley says, in his "Pathology of Insanity," "The disturbance of the cortical cells is, in reality, secondary; it is a reflex functional result of the primary morbid action that is going on in the neighborhood." And again: "Portions of the hemispheres may be cut away without the patient feeling it, though he is fully conscious." Quotations to the same effect might be made from the works and monographs of writers of note in many lands, to show that the cortical localization theory is still *sub judice*, even among its advocates.

This theory of basal influence was advanced by me in this city before "The Association of Medical Superintendents," at the meeting of 1878. I spoke then in the following terms: "Large portions of the cerebrum and cerebellum may be taken away from the living body without immediate danger of death; but the organs in the base of the brain, from which spring the numerous nerves so essential to life, cannot be touched, in vivisection or by disease, with impunity. From this central region nerve influence radiates to every part of the body, making its connections with the depositories of nerve power in the spinal cord and with the ganglia of the sympathetic system." (*Vide* "The Animated Molecule," page 38.) The writer advocated the same views in a paper read at Ottawa, before The Dominion of Canada Medical Association, in the following words: The functional area is really circumscribed to the most vital parts of the brain. It is then evident why traumatic injury and pathological processes in the cortex are not always followed by aberrations of function and mental unsoundness. The surfaces and upper portions of these nerve masses are adjuncts to vital organs in the centre and base of the brain. The former give power, but do not impart special function; they are auxiliaries but not necessities to the ganglionic centres; they bestow and intensify energy, but do not direct; they are—as it were—additional cells to the battery, but not its controlling power. It is a matter of experiment and pathological history that such a large area as the Rolandic zone can be physiologically destroyed, and yet leave the intellect unimpaired. A considerable portion of the frontal or even of the occipital lobes can be removed without any apparent alteration of the physical or intellectual powers. The corresponding lobes of either the frontal, occipital or parietal regions have been destroyed without affecting the conscious being or those functions said to have their seat of power in these parts. It is certain, then, that the hemispheres and the cerebellum are not the *sole* habitations of mind or of specialized physical operations. The reciprocity between mind and body is strikingly seen in amnesic aphasia. My experience teaches there can be no aphasia without more or less impairment of the memory, judgment and volition. Yet the functional and mental disorder can exist either without or with injury to the third frontal convolution. There is no basis for believing that this part is functionally energized to perform certain special operations, as an organ. It is the fact that we can have aphasia, paralysis of the legs, arm and face, with these so-called centres of nerve force unimpaired, or if impaired, without these results.

Digitized by Google

We know that aphasic conditions can exist with the centre of disease in the island of Reil or in the paracentral lobe. I remember a case of amnesic aphasia in which the post-mortem revealed softening of the upper limb of the first frontal, and no disease in any other part of the brain, as far as the microscope could reveal. In a monograph of mine, styled "Brain Lesions and Functional Results," published in 1880, I have given a large number of cases recorded in medical literature, especially in war records, to prove this point beyond all controversy.

I took a brain and passed wires through it in all the directions described in medical literature within my reach, in cases where there was brain injury and no functional or psychical perturbation after the shock of injury had passed away. I found a large number of such cases recorded in Mathews' and McLeod's "Surgical History of the Crimean War," in Chenn's "Surgical History of the Same War in the French Army," and in that admirable compilation, which has no equal, "The Medical and Surgical History of the War of the Rebellion." The result of my experiment was that the brain substance had been injured in every conceivable direction and in every half-inch square of the hemispheres, yet with no results at all commensurate with the lesions inflicted. If these parts are motor centres, then have we the miraculous phenomena of organic operations without organs; of varied and distinct functions without motive power; of uniform results without efficient causes. Were we to consider the brain a dual organ, the difficulty would remain, because there is any number of cases on record where corresponding sides have been simultaneously injured. In all the dual organs of the body we find sudden injury to one is always followed by imperfect and diminished work in its fellow until time is given to allow provision to be made for the extra labor imposed.

When we find no impairment in function consequent on injury to even *one* so-called motor centre, we are led by uniform analogy to doubt a doctrine so anomalous and contradictory, notwithstanding the great names which cluster round its acceptance. It is better to receive with caution a theory, now so popular, but which is accepted from experiments in isolated and exceptional cases. It is evident to me that more recent investigations establish my contention as being most consistent with physiological and pathological facts.

It seems to be overlooked, however, that in changed conditions nature, with wonderful aptitude, accommodates itself to new situations. In its vicarious provisions, in the inroads of tumors, in the cutting off of blood or nerve supply, and in the gradual breaking down of tissue, we always find heroic efforts put forth to repair the mischief, and often successfully. These gradual invasions and the physical results which flow therefrom may be altogether different in intensity and manifestation to the sudden shock of electricity, or of the traumatic injury made by cautery or scalpel. It is very fallacious to reason in every case from the insidious results of disease to those of violent experiment.

DISCUSSION.

Dr. Ralph Stockman, of Edinburgh:—The subject of localization of cerebral function is one about which there may be admittedly much difference of opinion. If I understand Dr. Clark aright, he maintains that volitional impulses of all kinds take their rise in the basal ganglia, and that there is no special localization of function in the cortex of the brain. Holding these views, may I ask him how he explains the well-known facts regarding Broca's convolution, and how, after this centre has been destroyed, it is the same convolution on the other side which assumes its functions.

The most recent experiments on the subject—those of Drs. Horsley and Beevor—seem to show that localization of function in the cortex is carried to a very high degree. In addition, we have the results of grosser experiments on frogs and warm-

Digitized by Google

blooded animals. In the former, when the cerebral lobes are removed, spontaneous movements cease, and the animal only reacts when external stimuli are applied to it.

In some recent experiments with camphor, I have found also that removal of the cortex in rabbits prevents the supervention of the characteristic epileptic convulsions caused by large doses of that drug. This points to the fact that these convulsions have their origin in the cortex, and that the cortex is, therefore, a motor centre. From Ferrier's experiments we conclude, further, that it is divided up into a number of motor centres for different muscles.

A large number of clinical cases have been adduced in support of Dr. Clark's views, but these can hardly be accepted in opposition to exact experiment. We do not know what exact conditions are actually present in such cases; and, besides, it is notorious that an organ—*e.g.*, the heart—may be seriously diseased and yet perform its functions comparatively well. I cannot, therefore, agree with the views which have been put forth in Dr. Clark's paper.

Prof. J. H. WYTHE, M.D., of California:—The paper just read has interested me greatly. I fully agree with the author that the periphery of the brain and cerebral substance are not essential either to motility or intellection. As long ago as 1853, I made post-mortem examinations of the brains of three children, who (at different times) had suddenly died in bed without any appreciable symptoms of disease, either mental or physical. Each case revealed extensive brain disease, and in one the brain substance was soft as melted butter. There was, evidently, no relation between the diseased brain and the mental power. At the last session of the California State Medical Society, a skull was exhibited by Dr. Chase, which belonged to a man who had been scalped and tomahawked by the Indians, leaving an opening three by four inches on the top, from which the brain protruded and gradually rotted away for ten years, but during all that time the man was capable of business, and was esteemed by his townsmen as a man of shrewdness and ability. I have collected about one hundred such instances from medical journals, which show that the popular idea of the identity of mind and brain is a fallacy. I would like to ask Dr. Clark, however, how he would answer the objection to his view based upon the localization of motor centres which has produced such brilliant results in the surgery of the brain. I think I can account for it without admitting the cerebral psychology, but I should be glad to hear his explanation.

Dr. W. D. HALLIBURTON, of London, defended the views of cerebral localization as taught by Ferrier. He mentioned several experiments which he had seen performed by Profs. Schäfer and Horsley, in which it was demonstrated that excitation of certain areas of the cerebral cortex produced a constant effect (movement), and removal of the same areas produced loss of that power. Injury to the basal ganglia produces widespread effects, probably owing to the injury to the neighboring internal capsule, the path by which the cortex is connected to the lower portions of the nervous system. He held, also, that the recent brilliant achievements in brain surgery amply confirmed the experiments made on animals. The cases, also, in which no ill result followed extensive injury to the frontal lobes were fully in accord with experiments on animals, and could not, therefore, be cited as adverse to the localization theory. He pointed out that the statement of the reader of the paper, that no difference of histological structure can be made out in different convolutions, was erroneous, as it is perfectly easy with the microscope to distinguish a sensory from a motor convolution. With regard to aphasia, he pointed out the complex nature of

Digitized by Google

this affection. True aphasia is always connected with disease of Broca's convolution, while all admit that certain forms of amnesia are not.

Dr. LOVE, of Atlanta, Ga., said:—Mr. President, I have listened with intense interest to the reading of the paper by Dr. Clark. It is a research in an interesting field—one in which we all feel deeply interested, and one in which there are very many points remaining in deep obscurity. The localization of function centres in the brain is still an open question, though it has received the attention of some of the best minds in the profession. While physiological experiments, on the one hand, would seem to throw much light on the subject now and then, physiological investigation as quickly knocks all its rays back into utter darkness. But such researches as are evidenced in the paper just read give token of a hope that physiological experimentation and pathological investigation may yet come up hand in hand, in evidence, the one for the other, reconcile all these points of difference, and demonstrate that *truth* for which we are all earnestly seeking.

That Dr. Clark is correct in localizing the more important functions in the basilar ganglia to which he alludes—if I understood correctly the reading of his paper—I have no doubt, yet, if he is correct, what becomes of the localization theories? I confess that I have found it difficult to settle down contented anywhere along the line between these two points of contention. Early impressions are not only very lasting but very prejudicing, and many, doubtless, find it difficult to drive out of the deep-cut grooves in which we have been running for so many years. I remember well my earlier impression; then it was thought that an injury to, or destruction of, the brain, however small, was necessarily fatal, but experience in early private practice brought me bravely out of that. If it had not, the terrible pass at arms from which our countrymen suffered so much, brought under my observation, in field and hospital service, abundant evidence of the fallacy of such teaching. I had ample opportunity at our central city, where the sick and wounded of "the Army of Tennessee" (under Joseph E. Johnston) gathered. Through orders issued by the post surgeon, all cases of wounds of the brain and of serious lesions of the nervous system were sent to our hospital and placed under my immediate care. The number of brain wounds and lesions, from the very nature of the engagements and defenses of the Confederates, was very large. Among the many thus gathered under my care were some of the most remarkable brain lesions I have ever known—cases that were both physiological and pathological curiosities to me, in the fearfulness of extent of the tissue destroyed and the final favorable termination of the cases. These cases brought to my mind—as does the paper of Dr. Clark—the researches and conclusions of that brilliant young Russian physiologist, Jacubovitch, whose researches were so rare, who rose so high, and, like many others who give early token of prospective leadership in the field of science, died so young. He contended that in early life the basal ganglia were the centre of all physiological action, and that the outside convolutions of white and gray matter were made up, in early life, of mere elemental material, to be, during life, worked up into fusiform, monopolar, bipolar and multipolar cells—reinforcements, as it were, to these great basal centres—that as the one or the other form of these cells prevailed in the neuro-histological development of the brain—sensory, fusiform or motor, polar—there was prevalence of sensation, or power of motion, in the man or animal. His researches were interesting in the extreme, and bid fair to make him a leader, in his day and time, in this department of ours, but he laid aside his lens and his labors, and science mourned his early departure to the great beyond.

In private practice, after the war, a case fell into my care which my experience in military practice, alluded to before, enabled me to manage with a success which I

Digitized by Google

could not otherwise have hoped for, as I believe. It was a case in which there was seemingly more extensive lesion and more extensive loss of brain tissue than in the celebrated case of Bigelow, of Boston, where the patient had a tampon rod, by a premature explosion, driven through the face and anterior lobe of the brain, yet made good his recovery and lived, as I understand, several years after the accident. The case falling under my care was a youth of fourteen years. On his return from school, in the afternoon of the day on which it occurred, he got into an altercation with a stout negro fellow and was cut in the head with an axe. The blade was driven deep into the left anterior lobe of the brain, the cut extending from the left supra-orbital ridge, through the frontal into the parietal bone, and deep into the brain. The distal corner of the cutting edge of the axe had been, in time, broken off and ground down—was thick and blunt—while the sharper edge cut directly through both outer and inner plate of the skull. This thick, blunt portion of the blade failed to cut through the harder internal plate, but the force detached and drove deep down into the brain a piece of this plate, about an inch by an inch and a third, irregular in shape. When the blade of the axe was at its greatest depth, the heavier pole of the axe deflected to the left, the cut skull acting as a fulcrum; the blade broke up or dished out all that portion of the brain laying to its right in the direction of the blow. This carried away most of the left anterior lobe, yet, strange as it may seem—in this condition, bleeding and faint—he, with the aid of his school-mates, walked to his home, a distance of over two hundred yards.

When I reached his bedside, which was only a few minutes after he had reached his home, he recognized me and extended his hand, but went instantly into convulsions. Chloroform was at once administered to control the convulsions, the case examined, the deep piece of bone detected and removed. The greater part of the left half of the frontal bone had been broken off, remaining attached to the scalp. Remembering Ollier's experiments for the reproduction of bone by the periosteal membrane, that membrane, in the removal of the bone, was left with the scalp, with the hope of reproduction. With kindly care and close watching, the patient, under cold-water treatment, made good recovery.

Now to the points of interest here in the discussion of Dr. Clark's paper: This patient must have lost between four and five fluid ounces of brain matter; quite a quantity was left on the ground where he received the injury; it was found on his hair and hat, and clothes, to his feet. In removing the bone and broken-up brain matter, I filled a two-ounce wine-glass with brain matter—not blood clots—and then, in the process of suppurative action which followed, much more necessarily passed away in the healing process.

After his recovery, he resumed school and resumed his study of music and of telegraphy. After the lapse of a sufficient time, to test the case thoroughly, I inquired of his respective teachers—

1. Whether his mind had been found impaired in any way?

2. Whether his memory of the lessons learned before the injury had been found good?

3. Whether in any one particular branch of study he seemed to evince defectiveness or want of ability to grasp the subject?

4. Whether in his study of music any difference had been noticed as to his measurement of time?

5. Whether in his study of telegraphy (a work in which the perception in time must be acute and quick) he had either forgotten the past or evinced deficiency in the then prosecution of the study?

Digitized by Google

To all these the most satisfactory answers were given. This youth then is a man now; he and all of his teachers yet live. I have watched the case with intense interest. The accident occurred in February, 1868; now, September, 1887, nearly twenty years, he is (or was not long since) an operator in a very important office of the Western Union Telegraph Company.

Now, in conclusion, Mr. President, I must say that such cases as this and others photographed in clear outlines upon my memory—from experience in both private and military practice—when brought side by side with the theories and results of experimental research by the localizationists, as reported to us, I find myself in a condition in which, as I said before, I cannot in content settle down anywhere on the line between the two.

With thanks to you and the Section for the time and your attention, I am done.

Dr. H. C. Boenning, of Philadelphia, said:—Mr. President, I do not desire to protract the session by discussing Dr. Clark's paper, but I desire to call attention to the fact that an examination of a hundred brains will show frequent variations in the position of the convolutions. I also desire to call attention to the fact that asymmetry in the two sides of the same brain occurs sufficiently often. It must be borne in mind, also, that the gray matter of the brain is continuous from one convolution to another, and if, in rare cases of disease of well-recognized convolutions, certain paralyses do not occur, it is not a proof of the absence of cortical centres. One point more: Experiments are being constantly performed by disease, and any one with sufficient patience and acumen can, in the dead-houses of our hospitals for nervous and mental diseases, discover cortical lesions for, if you will, brachial or crural or other paralyses. I am strongly in favor of the doctrine of localization.

Dr. Richard Caton, Professor of Physiology, Victoria University, England, read a paper entitled—

RESEARCHES ON ELECTRICAL PHENOMENA OF CEREBRAL GRAY MATTER.

RECHERCHES SUR LES PHENOMÈNES ÉLECTRIQUES DE LA SUBSTANCE GRISE CÉRÉBRALE.

UNTERSUCHUNGEN ÜBER ELEKTRISCHE ERSCHEINUNGEN DER GRAUEN HIRNSUBSTANZ.

While the electrical phenomena of peripheral nerves have been fully investigated by Prof. Du Bois Reymond, Prof. Donders and others, few researches have been made regarding those of the central nervous system. Du Bois Reymond's paper on "The Electrical Current of the Frog's Brain," and a brief reference to the subject in a communication made by Prof. McKendrick and Dr. Dewar to the Edinburgh Royal Society, are all I am acquainted with.

The questions I set myself to solve are the following:—

1. Does the gray matter of the brain give evidence of electrical currents comparable with those of nerve fibre and muscle?

Digitized by Google

2. If such currents exist are they related to the functions of the brain, and will the study of such currents threw any light on those functions?

I have, hitherto, experimented on the brains of forty-five animals, viz., cats, rabbits and monkeys. The instruments used were Sir William Thompson's reflecting galvanometer, with accessory apparatus; Du Bois Reymond's non-polarizable electrodes, modified for the special requirements of the experiments.

The animal employed was etherized and a portion of the scalp, skull and dura mater removed, so as to expose the greater part of one hemisphere. Small, light, non-polarizable electrodes were clamped securely to the skull in such a manner that thin sculptor's clay points could be placed on any region of the exposed brain. Light insulated wires suspended from a support overhead connected the electrodes with the galvanometer. The animal experimented on was tethered loosely to the centre of a table a yard square, and allowed to move about, eat and drink at its pleasure.

The experiment is attended with much difficulty, and is liable to failure from several accidental causes, such as hemorrhage, convulsions, collapse, death from use of anæsthetics, as well as non-polarity of electrodes from admixture of the clay with blood, serum, etc.

From one or other of these causes many of my experiments were quite valueless. Under favorable circumstances I always obtained evidence of the existence of electrical currents of considerable energy—of much greater energy than those of nerve fibre. In some cases the currents were so powerful that shunts were needed.

On applying one electrode to the external surface of the brain, and the other to the surface of a vertical section, vigorous currents passed through the galvanometer. The external surface was usually positive to the vertical section. If both electrodes were placed on the external surface a feebler movement of the galvanometer needle resulted.

Further fluctuations of the current were observed sometimes, coinciding with some movement of the animal's body or some change in its mental condition. In deep anæsthesia, for example, considerable deflection of needle was generally observed, lessening gradually as the animal regained consciousness. And when death was caused by prolonged anæsthesia, or by that and the opening of a large vessel, considerable deflection occurred, and shortly after death the needle fell to zero.

I obtained more definite results when experimenting on Ferrier's motor and sensory areas.

1. There is a region in the gray matter of the rabbit's brain, stimulation of which by the interrupted current causes rotation of the head to the opposite side. In the brain of the monkey there is also a corresponding centre. In several instances I found that by producing a sound, or by offering food on the one side of the animal experimented on, I could induce it to turn its head voluntarily to that side; when this movement was made, electrodes placed on the centre in question of the opposite hemisphere showed a fall in the current toward zero, in fact a negative variation; the movement of the needle exactly coincided with the movement of the animal's head to the opposite side. Probably the explanation is that the brain cells of the region were in a state of functional activity connected in some way with the head movement, and that a negative variation of the electric current occurred similar to that which is well known to occur in a nerve fibre when a reverse impulse traverses it.

2. It is difficult to induce a rabbit or a monkey to perform any definite voluntary act and to repeat the action frequently enough for the basing upon it of a physiological inference. The act of mastication is more easily induced than any other. A rabbit will frequently eat a piece of fresh lettuce, and a monkey will usually eat a raisin or a piece of raw potato as soon as it is offered him. I experimented, therefore, frequently on that centre of the brain which when stimulated causes masticatory movements. In half the animals used, I found that when the non-polarizable electrodes were placed on

Digitized by Google

this centre, negative variations occurred invariably when the animal masticated, the variations lasting as long as mastication and ceasing when mastication ceased. In some instances it was evident that the thought or expectation of food caused the movement of the needle. If I showed the monkey the raisin but did not give it, a slight negative variation in the current occurred. When the electrodes were applied to this region, I found that sensory impressions made on the mouth or face caused a similar movement of the needle; for example, the introduction of the handle of a scalpel into the mouth, pinching of lips or cheeks, or stimulation of skin of face by interrupted currents. It seemed from this experiment as though the centres for movement of jaw, for perception of sensory impressions from mouth and face, and for ideas of food derived through the eye, coincided or were closely adjacent to one another.

The area associated with these functions appeared to be small. I frequently had to search for it for some time. If the electrodes were not upon it but merely near it, no relation was observed between mastication and the movements of the galvanometer.

3. Placing the non-polarizable electrodes on a given motor area, for example, Ferrier's region No. 1, related to the hind limb, I found that if I stimulated the limb with an interrupted electric current, negative variations frequently, though not always, occurred. Stimulation of other parts of the body had no such effect. This experiment seemed to indicate that the centres for the production of muscular movement and for sensory perception in the skin coincided for the limb in question.

4. Not unfrequently after continuous exposure of a hemisphere of the brain, spasms occurred in one or both limbs on the opposite side. If I placed the non-polarizable electrode on the brain area corresponding to the movement, I usually found that a strong negative variation coincided with each spasm. This was seen in numerous experiments, though not invariably.

5. I found no part of the brain the electrical currents of which were influenced by stimulation by odors or by sound.

6. I tried the effect of alternate intervals of light and darkness on seven rabbits and four monkeys, placing the electrodes on the region (13) stimulation of which causes movement of eyes. In three rabbits and two monkeys I found that light caused negative variation almost invariably. In those five experiments in which I was successful the relation between the intervals of light and darkness and the movements of the galvanometer needle was quite beyond question. If I partially shaded the animal's eye from the light, the effect on the electric current was diminished. The exact way in which the light produced its effect is not so easy to determine. It may have excited the visual centre especially, or it may have acted as a general excitant to the whole brain, or the result may possibly have been due to the heat radiated from the flame acting on the electrodes; I think one of the first two theories is more probable than the third.

These are the chief results of my researches. The large number of experiments from which I obtained no result at all (more than half the entire number) may be explained by the difficulties I have already mentioned. I may add that when swelling and congestion of the gray matter occurs, it is commonly accompanied by great unsteadiness of the electrical current, a condition which renders any observation made quite untrustworthy.

To sum up my results :—

1. There can be no doubt that the brains examined manifested vigorous electrical currents.

2. These currents were increased during the suspension of functional activity produced by anæsthesia, and in that occurring at the time of death. Shortly after death they decreased and finally disappeared.

3. These electrical currents were seen to be influenced coincidently with certain

Digitized by Google

voluntary acts or sensory perceptions, associated with the region of gray matter under examination.

The current was almost invariably *diminished* when the convolution examined was functionally active. In fact, a true negative variation occurred.

4. The electrical change demonstrated in Ferrier's areas in the gray matter coincidently with the muscular movement or sensory perception associated by Ferrier with such area, is further presumptive proof of localization.

5. These experiments afford some evidence that the areas of brain related to special muscular movements, are also related to sensibility in the adjacent skin.

Dr. T. O. Summers, of Jacksonville, Florida, presented a paper entitled—

TROPHIC INFLUENCE OF THE NERVOUS SYSTEM.

L'INFLUENCE TROPHIQUE DU SYSTÈME NERVEUX.

TROPHISCHER EINFLUSS DES NERVENSYSTEMS.

That there is some subtle force which presides over the distribution of tissue pabulum and determines its fixation in the cellular elements of the organism, no physiologist of the present day will deny. That this force is independent of the chemical affinities which arrange the proximate principles of the organism, is also sufficiently apparent from pathological observations, a perverted nutrition disturbing these chemical relations and disrupting their normal physiological arrangement.

I shall advance one step further, and declare that, *a priori*, it is from the nerve system alone that these nutritive elements receive their distributing orders, determining, by supreme authority, their ultimate destination in the organism, whether for physiological or pathological results. To substantiate this I need only refer to the striking phenomena of ideo-motor arcs of nervous action, in which that ethereal product of the sensorium commune which men call thought, runs like an electric thrill to the remotest parts of the organism, solidifying expression in the face, arresting or stimulating digestion, quickening or slowing the arterial pulses, and inhibiting every function of every organ. The direct influence of the cerebro-spinal system upon the sympathetic has been deemed sufficient to explain these phenomena of emotion, but this is not sufficient for phenomena of elective assimilation in the nutritive process. We are driven to search for some other agency, whose function it is to direct and to determine the nutritive elements to their physiological destination and purpose. The idea of trophic centres seemed to have been developed originally from the logic of physiological necessity, for without this *tertium quid* in the domain of nerve force there was no explanation of the manner in which the unconscious activities of the sympathetic executed the orderly functions of nutrition. When the chemical acts of digestion are complete, when the pabulum is ready for distribution to the various tissues, the organic system of nerves can go no further. Presiding over glandular action, it responds to the stimulus of blood and sets in motion the great process of secretion and excretion, but it has nothing to do with the assimilation of food or the fixation of the pabulum prepared for the support of tissues in its proper place. Here comes in that great factor without which the problem of nutrition could never be worked out. There must be a portion of the general nerve system whose peculiar function it is to feel out after all the prox-

Digitized by Google

imate principles of the organism as they sweep by in the current of the blood, just as the antennæ of the articulates float out in the water of the sea to find amid the myriads of organisms that which is adapted to its own nutrition, and rejecting all others with the accuracy of chemical affinities in the laboratory of nature.

The arrangement of these nerves is very peculiar. As far as I have been able to differentiate them, they are small nerve cells of gray matter, lying imbedded in the several tissues of the organism. In muscular tissue they appear upon the ends of the muscular fibrillæ, and without very close examination would seem to be a prolongation of the sarcolemma. These cells are multipolar, and, from the fact that their poles run out to the wall of the adjacent blood vessel, I feel assured that they have nothing to do with the contractility of the muscle, but only serve to telegraph to it the fact that muscular pabulum is passing by, and thus set the osmotic relation between the waste materials of the tissue and the nutritive products necessary for its regeneration and support. It is in the periosteum, however, that these cellular nervous links are more clearly seen, and in all other tissues of the body where I have been unable to demonstrate them, I think analogy would justify us in presuming upon their presence.

If the question should be asked me, What difference in cellular structure determined this power of elective assimilation? I should answer that I can no more explain this than the chemist can explain "why sulphuric acid will take baryta from all its combinations, or iodine will leave its potassium spouse whenever it meets its mercurial mistress." These affinities run through the whole of nature, and determine all its ongoings, and I am not surprised that the immortal Darwin should have founded upon them the philosophy of universal life.

Under the anatomical relations which I have described it is easy to explain all pathological formations and trace out the process of development in all neoplasm which so nearly resemble the tissues in which they are imbedded; and not only so, but the evolutional changes which are continually going on in the variation of species fron generation to generation.

Prof. E. D. Cope, our greatest comparative anatomist, first directed my attention to this subject just sixteen years ago, in his remarkable paper upon "*growth force*," and I have gone thus far in my investigations to be well assured that the problem of nutritive assimilation is completely solved by the cellular nerve links which bring the tissues *en rapport* with the quality of pabulum necessary for its existence and development, which would, otherwise, pass unrecognized and unappropriated.

Digitized by Google

SECOND DAY.

The Section met, with the President, Dr. CALLENDER, in the chair.

W. D. HALLIBURTON, M.D., B. Sc., Assistant Professor of Physiology, University College, London, England, read a paper on—

A COMPARISON OF THE COAGULATION OF THE BLOOD WITH RIGOR MORTIS.

UNE COMPARAISON DE LA COAGULATION DU SANG AVEC LA RIGIDITÉ CADAVÉRIQUE.

EIN VERGLEICH ZWISCHEN BLUTGERINNUNG UND DEM RIGOR MORTIS.

The coagulation of the blood after it is shed is, in many points, similar to the stiffening of the muscles which occurs after death, called *rigor mortis*. The blood plasma is, during life, a liquid; the muscle plasma, *i.e.*, the contents of the sarcolemma is also, during life, a liquid. Both contain various albuminous matters of a complex nature. In both, certain of these proteid or albuminous substances undergo a change after death; this change is a solidification; and the solid substance is called the clot; the liquid residue being called the serum.

In the case of blood, the clot is composed of fibrin (with entangled corpuscles), and after coagulation has occurred it floats in blood serum. In the case of muscle plasma, the clot is composed of myosin, and the liquid residue is termed the muscle serum.

The theory now most commonly received as to the cause of the coagulation of the blood is Hammarsten's. He regards it as due to the conversion of a previously soluble proteid, fibrinogen, into an insoluble one, fibrin. This change is due to a ferment action; the ferment (fibrin ferment) being one product of the disintegration of the white corpuscles which occurs when the blood is removed from the vessels. The action of this ferment can be inhibited by cold, or by the admixture of the blood with certain proportions of neutral salts; and thus the coagulation of the blood can be prevented.

Kühne obtained muscle plasma by squeezing it out from the chopped-up muscles of the frog at a low temperature. He found that cold prevents the coagulation of muscle plasma, in the same way as it will prevent that of blood plasma. I have been able to confirm this statement with regard to warm-blooded animals (rabbits and cats); and I also found that admixture of the muscle plasma with solutions of neutral salts, like sodium chloride, sodium sulphate or magnesium sulphate, prevents its coagulation. Dilution of such salted muscle plasma removes the inhibitory influence which the strong saline solution has, and brings about the formation of myosin; coagulation occurring much more rapidly at the temperature of the body than at lower temperatures. In all these points the behavior of muscle plasma is exactly similar to that of blood plasma.

This similarity suggested to me that the cause of the coagulation was the same in both cases, viz.: a ferment action; and I succeeded in obtaining what I propose to call

Digitized by Google

myosin-ferment, by the same methods employed by Schmidt in the preparation of fibrin ferment. Finely-chopped muscle was subjected to the action of alcohol for some months, to coagulate the proteids, and then dried over sulphuric acid. Aqueous extracts of this dried muscle had the power of hastening the coagulation of salted muscle plasma, and contained the myosin ferment. The ferment is identical with or closely connected to a proteid of the class to which Kühne and Chittenden have given the name of albumose. Its activity is destroyed at 100° C. It is not identical with fibrin ferment, as it does not hasten the coagulation of blood plasma, nor does the fibrin ferment hasten the coagulation of muscle plasma. The myosin ferment acts upon a previously soluble proteid of the globulin class which I have been able to separate, and for which I propose the name myosinogen.

It is thus seen that there are very striking resemblances between the formation of fibrin and that of myosin. There are also certain differences, of which the most important are the three following:—

1. Myosin can be easily redissolved by solutions of neutral salts, and this solution can be made to undergo re-coagulation by dilution and addition of myosin ferment. Fibrin, on the other hand, cannot be made to undergo such a re-coagulation.

2. The formation of myosin is accompanied by the formation of lactic acid. This same acid is formed when the re-coagulation of pure myosin occurs, and its source is thus shown to be a proteid one, not a carbo-hydrate, as some have supposed. The formation of fibrin is not accompanied, so far as we know, by the development of any acid.

3. The conversion of myosinogen into myosin is not accompanied by the formation of another globulin, as is the case in the conversion of fibrinogen into fibrin.

The question of the cause of the disappearance of *rigor mortis* at a certain time after its onset, is a question which I am at present engaged in investigating; the commonly accepted theory, that it is due to putrefactive changes, appearing to me to be unsatisfactory. So far as my experiments at present show, the passing off of rigor seems to be due to a retroversion of myosin into the more soluble substance, myosinogen. The agent in this backward change appears to be pepsin, which Brücke showed to be present in the tissue, having been absorbed from the alimentary canal. During life it is inactive, as the tissues have an alkaline reaction; but after death, when the muscles turn acid, it is enabled to act, and at the temperature of the body peptones may be actually obtained. After death self digestion does not go so far as this, as the body becomes cold; still, I am inclined to believe that it is sufficiently powerful to bring about the first stage of digestion, namely, the reconversion of myosin into myosinogen. The varying time after death at which rigor passes off may be found to depend on the varying amount of pepsin in the tissues, and this again would vary with the condition of the alimentary canal at the time of death.

Digitized by Google

Dr. J. Alexander McWilliam, Prof. of Physiology in the University of Aberdeen, read a paper—

ON ELECTRICAL STIMULATION OF THE MAMMALIAN HEART.

DE L'IRRITATION ÉLECTRIQUE DU CŒUR DES MAMMIFÈRES.

ÜBER ELEKTRISCHE REIZUNG DES SÄUGETHIER-HERZENS.

The question of the effects of electrical stimuli upon the mammalian heart is one of obvious importance, both in its purely physiological and its clinical aspects, bearing as it does on the possibility of restoring the cardiac beat after it has been arrested by causes of a temporary nature. In the clinical aspect it specially concerns the cardiac failure which is so grave a possibility among the results of the action of anæsthetic agents. It may, indeed, be alleged that in such instances the cardiac failure is frequently due to a general loss of vascular tone, or to a gradual enfeeblement and paralysis of the nervous or muscular arrangements of the heart in consequence of the prolonged action of the poison upon the cardiac tissues. But, on the other hand, it would appear, as Lauder Brunton and others have urged, that in very many instances—probably in a large majority of the cases of heart failure during the administration of anæsthetics—the cardiac standstill is due to inhibitory impulses transmitted along the vagi nerves; impulses that do not necessarily cause any permanent damage to the cardiac tissues, but simply depress or suspend the *spontaneous* discharge of beats by the organ. The possibility of artificially exciting the action of the heart will, in such cases at least, appear to be an eminently feasible one. Among the means that have been tried to excite the cardiac action, electrical stimuli have been frequently employed, and usually without success. The mode of excitation commonly employed has been the application of a faradic current to the chest wall over the heart; at times electro-puncture has been used. But Ludwig and Hoffa showed that the application of strong constant currents or of faradic currents to the ventricles of the dog's heart immediately abolishes the normal beat, and throws the ventricles into a remarkable state of arhythmic, quivering movement (fibrillar contraction, Herz-delirium, delirium cordis) which is entirely insufficient to propel the blood from the ventricular cavities. The heart becomes gorged with blood, the blood pressure falls rapidly, and the animal dies as rapidly as though the root of the aorta had been ligatured. The essential features of this condition appear to occur under faradisation, not only in the heart of the dog, but in all mammalian hearts. I have, myself, observed it in the dog, cat, rabbit, rat, mouse and hedgehog. I have found it to occur both when the thorax has been opened and the heart laid bare, and when the current has been passed through the walls of the intact thorax. I have no doubt that it may readily occur in the human heart. Hence, it is obvious that the employment of faradic currents as a means of faradic excitation is one fraught with the gravest danger. It has, indeed, been argued that the currents used to stimulate the heart in man are not so strong as those employed by Ludwig and others, and would probably be insufficient to produce such a disastrous result. But this argument is one on which there is no reliance whatever to be placed, notwithstanding the fact that Von Ziemssen and others have described the application of faradic currents to the human heart without unfavorable results, for one of the facts with which I have been most strongly impressed during my experimental investigation of the subject has been the variability of the behavior of the heart in this respect, the extreme readiness with which, in certain circumstances, the ventricles pass into the arhythmic, quivering

Digitized by Google

condition described. This state of increased sensitiveness to electrical currents appears to occur most frequently in abnormal conditions—when the respiration has been interrupted or when there has been symptoms of incipient cardiac failure from various causes (excess of chloroform, etc.). In such cases I have seen again and again an exceedingly weak faradic current—such as could hardly be felt by the tongue—cause an immediate arrest of the ventricular beat, and an immediate supervention of the arhythmic, fibrillar condition. Von Ziemssen, indeed, tried the effects of faradic currents upon the human heart (in a case of "Ectopia Cordis") without obtaining any marked results. So also have Herbst and Dixon Mann, experimenting upon the normal chest. But the conditions obtaining in these experiments are entirely different from those present in the cases where stimulating currents are required clinically. In the former the heart was beating normally; in the latter the cardiac action has been markedly impaired or entirely suspended. And it has been seen that in such conditions the heart frequently manifests an extraordinary sensitiveness to the influence of faradic currents; a sensitiveness that differs most markedly from the state of comparative insusceptibility obtaining in the normal heart. Further, a faradic current too weak to provoke fibrillar contraction in the ventricles appears to have no considerable stimulating influence upon the heart.

It is certain that some form of stimulation must be sought other than faradic or strong constant currents. Such a means of stimulation is available in the form of single induction shocks. A single induction shock acts as a powerful excitant of cardiac contraction. During cardiac standstill, brought about through the vagus nerves, a single induction shock readily excites a single contraction of both auricles and ventricles. A slow series (say one per second) of induction shocks of moderate strength causes a series of complete heart beats at the same rate; and never in my experience have single induction shocks produced the fibrillar condition. Single induction shocks are thus readily available for the purpose of exciting cardiac beats. The order of the beat (*i. e.*, the succession of auricles and ventricles) varies, however, according to the part of the heart most strongly excited by the shock. When a shock is applied to the auricles the contraction follows the normal order. But when the shock is applied to the ventricles, the ventricular contraction occurs first and then the auricular contraction; in other words, the heart beat occurs in reversed order. And this raised the question as to whether such a reversed form of beat would be effective in discharging the blood from the heart, especially as in employing induction shocks upon the heart in the intact thorax it would necessarily be uncertain what part of the heart would be specially acted on by the stimulating shocks. And during my work on the hearts of cold-blooded animals, I found in the case of the eel that a series of induction shocks applied to the ventricle gave a series of reversed heart beats which were *not* effective in keeping up the circulation of the blood. The auricle became gorged, and the circulation became palpably obstructed. I have tested this question on the mammalian heart, the thorax being opened and artificial respiration kept up by comparing the effects of induction shocks applied to the auricles and ventricles respectively during cardiac arrest brought about through the vagus nerves. I have found that the state of matters is very different from what obtains in the eel. Contraction of the mammalian heart, whether resulting from stimulation of the auricles or of the ventricles, whether excited in the normal order or in reversed fashion, is effective in causing a discharge of blood from the ventricles into the great vessels and a marked improvement in the condition of the heart and of the circulation generally. It would appear, then, that during an arrest of the heart's action, brought about through the vagus nerves, there is available an effective means of excitation by which the activity of the organ may be aroused, provided the standstill has not been of so long duration as to cause the death of the cardiac tissues.

Digitized by Google

In those instances where cardiac failure has occurred, not from inhibitory impulses but from a gradual depression and paralysis of the heart by prolonged and excessive action of an anæsthetic, it is much more difficult to obtain successful results. But the experiments of Böhm show that, in favorable circumstances rhythmic compression of the heart in animals, together with artificial respiration, may lead to recovery of the heart even after long periods, *e. g.* 40 minutes, and I have myself witnessed the complete recovery of the cardiac action in cats after there had been apparent death of the organ, in consequence of an excessive administration of chloroform (after section of both vagi). Experimenting with the thorax open, I employed rhythmic compression of the heart, which is probably the most effective means of restoring the cardiac action in such cases. And though such a method available in animals possesses advantages not possessed by any method applicable to the human subject, still I have obtained favorable results from the use of induction shocks, and it is probable that cardiac recovery even in man occurs to a much greater extent than is commonly accepted.

The mode of excitation by induction shocks appears to be the method plainly indicated by our physiological knowledge of the subject, based upon facts that appear to be applicable not merely to one animal but to the whole mammalian class.

Induction shocks should be employed in preference to the make and break of a primary current, on account of the greater power which the induced current possesses of overcoming resistance. The shocks ought to be sent through the heart, one electrode being applied over the apex of the heart and the other over the sixth or seventh dorsal vertebra, the electrodes to be of large extent. The shocks ought to be about the normal rate of the heart's beat.

Dr. Thomas W. Poole, of Ontario, Canada, read a paper—

ON THE NECESSITY FOR A MODIFICATION OF CERTAIN PHYSIOLOGICAL DOCTRINES REGARDING THE INTER-RELATIONS OF NERVE AND MUSCLE.

SUR LA NÉCESSITÉ D'UNE MODIFICATION DE CERTAINES DOCTRINES PHYSIOLOGIQUES À L'ÉGARD DES RAPPORTS ENTRE NERF ET MUSCLE.

ÜBER DIE NOTHWENDIGKEIT EINER MODIFIKATION GEWISSER PHYSIOLOGISCHER LEHREN HINSICHTLICH DES VERHÄLTNISSES ZWISCHEN NERV UND MUSKEL.

For some years past I have endeavored to bring to the notice of the profession a view of the inter-relations of nerve and muscle—more especially of the vasomotor nerves and the arterial muscles—which is entirely at variance with what is taught in our physiological text-books.

I should be unable to find any excuse or apology for attempting so bold a task, were it not that the proofs which I have to advance are drawn entirely from the authentic storehouse of physiological research. While the facts to be here advanced are the results of observation by the great Masters in this department of science, I hope to be able to show, conclusively, that the inferences or interpretations placed upon these facts are, in some instances, erroneous, and ought to be modified or reversed.

In the examples now to be cited of an erroneous interpretation of authentic experi-

Digitized by Google

ments, the idea evidently dominating the physiological mind was, that a stimulus from nervous energy is necessary to induce muscular contraction. As a corollary to this idea, of course, it followed that when the motor nerve supplying a muscle was cut, or paralyzed from any cause, the muscle thus deprived of nerve influence was rendered incapable of displaying its contractile power.

That such an idea was apparently justified by the behavior of the *voluntary* muscles is undoubtedly true; but not so in regard to the non-striated or involuntary muscles of organic life, which have been pronounced by physiologists to be paralyzed and powerless, at the very moment that the observers saw and recorded the palpable evidences of their more or less active contraction. In fact, so far from the current teaching of physiology being true, as regards the relations of motor nerves to involuntary muscles, the very reverse is true; the actual fact being that *muscles of the involuntary class, as a rule, contract, not when stimulated by their proper motor nerves, but when these nerves are cut, or are paralyzed, or dead.*

The Œsophageal and Gastric Muscles.—To come now to the facts. The statement continues to be repeated in each succeeding text-book on physiology, that section of the pneumogastric nerves (vagi) is followed by paralysis of the œsophagus and stomach. Now, on the theory uppermost in the minds of physiologists—referred to above—the œsophagus *ought* to be paralyzed here, and to be reduced to the condition of a mere flaccid tube. But that such is not the case is evident from the fact that after the operation, food and drink fed to the animal, "in a few moments are suddenly rejected by a peculiar kind of regurgitation." (Dr. Dalton's "Phys.," p. 473.) It needs no argument to prove that the sudden rejection of ingesta, in the manner stated, so far from being an evidence of paralysis, is really a proof of active contraction in the muscle.

But it is said that sometimes the ingesta are detained in the œsophagus for a time, and, "owing to paralysis of this canal," are not conveyed into the stomach. (*Ib.*) Dr. W. B. Carpenter, F. R. S., refers to this by stating that "if the pneumogastric be divided in the rabbit, on each side, above the œsophageal plexus but below the pharyngeal branches, and the animal be then fed, the food is delayed in the œsophagus, which becomes greatly distended." ("Hum. Phys.", 5th Amer. Ed., p. 404.) Now the pharyngeal branches supply the upper part, and the œsophageal plexus the lower extremity of the muscular tube. Mark what follows on section of the vagi between these two! The upper part of the œsophagus, whose nerves are intact, admits the food and drink apparently in a normal manner, while the lower part of the tube, which has been deprived of nervous influence, contracts upon itself, and so lessens the calibre of the "canal" as to arrest the further passage of the superimposed ingesta, as a consequence of which the œsophagus "becomes greatly distended."

Whether the ingesta are thus forcibly detained or "forcibly ejected" would appear to depend on the point at which the vagi are cut. But in either case, the result, so far from being a proof of paralysis, really bears evidence of activity of the muscle. And this is confirmed by the observation of Dr. M. Hall, that "the simple contractility of the muscular fibre [of the œsophagus] occasions a distinct peristaltic movement along the tube *after its nerves have been divided*, causing it to discharge its contents when cut across." [Italics mine.] (Dr. Carpenter's "Hum. Phys.," 5th Amer. Ed., p. 404.)

Dr. Burdon Sanderson expresses the idea uppermost in the physiological mind, in stating that after section of the vagi "the muscular fibres of the œsophagus are paralyzed, so that regurgitation of food from the stomach is apt to take place." ("Handbook for Phys. Lab.," Amer. Ed., p. 318.)

Dr. W. B. Carpenter seems to pass over this part of the subject lightly, and it is not till treating of the effects of section of the vagi on the gastric secretions that he plainly states that, "the first obvious effects of this operation are vomiting (in animals that are capable of it), and loathing of food." (p. 423.) He also tells us, in another place,

Digitized by Google

that the reopening of the cardiac orifice, on pressure from within, is one of the first of that series of reverse actions which constitute vomiting. (p. 404.) It is evident that the "pressure" referred to and the force necessarily required to eject the contents of the stomach and œsophagus could not come from "paralyzed" muscles, which the facts show to be really undergoing active contraction.

That nerve force is actually in abeyance in the act of vomiting was fully recognized by Dr. Anstie, who places it among the effects of paralysis of the medulla oblongata in narcosis. ("Stimulants and Narcotics," p. 168.) While the vomiting of migraine, he says, "marks the lowest point of nervous depression." ("Neuralgia," p. 39.)

Had those eminent physiologists, Drs. Todd and Bowman, doubts of the truth of the physiological theory of the day, and a prescience of what the future had in store, when they wrote: "The office of the gastric branches of the vagi nerves appears, from Dr. Reid's experiments, to be chiefly to *control* the movements of the muscular coat of the stomach." [Italics mine.] ("Phys. Anat.," p. 493.) That is precisely what the scope of this paper is designed to show—that in so far as the involuntary muscles, at least, are concerned, the function of nerve force is not to stimulate, but to restrain and control muscular activity; which all physiologists regard as an inherent endowment of muscular tissue.

The Bronchial Muscles.—Dr. Burdon Sanderson informs the readers of the "Handbook," that after section of the vagi "the muscular fibres of the bronchial tubes are in a similar condition" to those of the œsophagus and stomach. (p. 318.) Then it is evident that these muscular bands come under the rule of law laid down above, and contract, like other muscles of this class, when deprived of nervous influence.

The Nasal Muscles.—It is a curious fact, that "owing to the great size of the velum pendulum palati, the horse is unable to breathe through the mouth." (Strangeway's "Veterinary Anat.," p. 209.) As a consequence, respiration is carried on in this animal exclusively through his nose; and when both the facial nerves are cut, or paralyzed, "the nostrils immediately collapse, and the animal dies by suffocation." (Bernard, quoted by Dr. Dalton, "Phys.," p. 458.)

A result very similar, so far as the closure of the nostrils is concerned, has occurred in the human subject, during paralysis of the facial nerve. Thus, Sir Thomas Watson, reporting the case of the girl, Jane Smith, says: "When she tried to snuff in air through her nose, not being able to keep the right nostril stiff and open, its sides came together, and no air passed up that side." (Lectures, Prac. Physic, p. 366.)

A little reflection will show that this is necessarily due to muscular contraction. The effect produced is not to be accounted for by any filling up or stuffing of the nasal passage by relaxed or paralyzed muscles, because the muscles are on the exterior of the cartilages, and mucous membrane or fibrous tissue does not contract or respond to nerve action. The obstruction is caused by the cartilages of the nose coming together, for which the only adequate explanation is the action of the constricting muscles, which, as in other similar cases, assert their power when nervous restraint is removed.

Spasm of the Glottis due to Nerve Paralysis.—We now come to a still more striking illustration of the truth of the proposition laid down above. The aperture of the glottis is closed by one set of muscles and opened or dilated by another. The constricting muscles are the arytenoidei and crico-arytenoidei laterales, while the dilators of the glottis are the crico-arytenoidei postici.

Dr. Burdon Sanderson states that "the widening of the glottis is a condition of general muscular relaxation." He further states that the closing of the glottis is equally due to a general contraction of all the muscles; so that the glottis is closed, "not because the postici crico-arytenoidei muscles and the other dilating muscles * do

* There are no "other dilating muscles" than the crico-arytenoidei postici.

Digitized by Google

not act with the rest, but because they are overpowered by the constricting muscles." (Handbook, p. 308.)

The situation thus depicted becomes quite remarkable and full of interest, when it is remembered that the sole motor nervous supply to both these sets of muscles passes through the inferior laryngeal (or recurrent) nerve, a branch of the pneumogastric, and that when this nerve is cut or paralyzed, the closure of the glottis takes place, as a result of spasm of both of the antagonizing muscles, as just stated.

On page 318 of the Handbook the same eminent physiologist, describing the effects of section of the vagi, says: "The glottis is partially closed, just as it is in death." How the glottis is closed in death will appear from the fact, vouched for by Dr. Austin Flint, in the fifth edition of his "Practice of Medicine," when he says, the operation of passing a probang within the larynx "is extremely difficult, if it be practicable, on the cadaver." (p. 294.)

There can be no doubt about the effect of the section referred to being of a paralyzing character, so far as the nerve is concerned, seeing that the simple section of the nerve during life, and the extinction of all nerve force in death, lead to precisely the same results as regards the closure of the glottal aperture. Dr. Burdon Sanderson adds that, "in animals with divided vagi, life may be prolonged by tracheotomy," showing how complete and fatal is the spasm thus produced. Other evidence of similar import is not lacking. Thus, Dr. Austin Flint, discussing the "danger of death from suffocation" in the "obstructed inspiration" occurring in nervous aphonia says: "The condition is analogous to that after the physiological experiment of dividing both recurrent laryngeal nerves." ("Prac. of Med.," 5th Ed., p. 309.) The same author has "reported a case in which the left recurrent nerve being situated between a calcareous deposit and an aneurismal tumor, spasm of the glottis occurred so frequently and to such an extent as to prove fatal." (*Ib.*, p. 371.)

Now, since the recurrent nerve is the only motor nerve supplying these muscles, and since section or pressure on a nerve trunk cannot increase nerve activity—the nerve trunks being mere carriers and not producers of nerve force—it is evident that no other conclusion is possible than that the spasm here referred to is due to the absence of nerve force, and not to a stimulus from excited nerve action. And since nerve paralysis is thus shown to be directly the cause of spasm of the glottis, it is not necessary to infer that whatever is done by reflex action to cause spasm of the glottis must be of a paralyzing character to the nerve also? Thus, what is vaguely called "irritation," by which is usually meant an excitation or exaltation of nerve power, and which consists really in a perturbation of nerve force, must necessarily be an influence of a paralyzing character to the nerves it traverses. Such reflex "irritations" are usually attributed to brain lesions, to indigestible food, and other causes of a more or less debilitating character which may well arrest, rather than develop, the flow of nervous activity.

If it be true, that pain is "an expression of impeded and imperfect nerve energy, not of heightened nerve function," for which there is high authority (Anstie, "Neuralgia," pp. 12 and 163), how much more is the perturbation of the nerve molecules, which constitutes "irritation," a disturbance of normal activities which is equivalent to paralysis.

Relation of Vasomotor Nerves to the Arterial Muscles.—I propose to show here, on the very best physiological authority, that what is known as "paralytic hyperæmia" is—contrary to the accepted opinion—venous and not arterial.

I need not delay to offer proof that the middle muscular coat of the arteries is under the control of the vasomotor nerves of the sympathetic, which regulate the calibre of these tubes; or that the chief vasomotor centre is in the medulla oblongata, with probably lesser centres in the spinal cord. These are among the well-authenticated facts of recent physiology. It is in determining the action or play of this mechanism that I

Digitized by Google

have the temerity to claim that our physiologists have made an "unscientific use of the imagination."

The theory of the text-books is, that when the influence of the vasomotor centre is cut off from the arterial muscle in any way, hyperæmia of the arteries results. Thus, in destruction of the nervous centres by the operation of "pithing"—as a result of section of the spinal cord just below the medulla, and on section of the chief vasomotor nerve trunks, in the body or viscera, it is claimed that the corresponding arteries are more or less dilated. Dr. Burdon Sanderson contents himself with stating that under these circumstances "the arteries are relaxed," and again, that they "become permanently larger." ("Handbook," pp. 245–256.) Other physiological teachers, such as Prof. Kuss, say that here the arteries are "dilated," while Dr. Sidney Ringer, in his excellent "Therapeutics," has it that "the arteries remain widely dilated." (5th Amer. Ed., p. 312.) We shall presently see how far these statements are justified by the facts.

Section of the Cervical Sympathetic.—To M. Claude Bernard and Dr. Brown-Séquard we are largely indebted for what is known on this subject, as observed by them in the famous experiment on the cervical sympathetic. Dr. Brown-Sequard enters into the details at great length in his "Physiology and Pathology of the Central Nervous System." Yet, nowhere in this work, in regard to this or any other section of cord or nerve, does he once assert that the arteries are dilated. In the pages devoted to it he refers to the contemporary experiments on this subject by Waller, Donders and his pupils, by Kussmaul and Tenner, Moritz and Schiff, yet he makes no mention of an allusion to dilated arteries by any of these eminent observers. This is surely significant. With him it was always "the blood vessels" which are "paralyzed" and "the blood vessels" which are "dilated." He says that "the hanging down of an animal, by holding it up by its hind legs, in producing a congestion of the brain, produces very nearly all the effects of this section." (p. 143.)

From these considerations it will be evident, first, that it was by no means apparent—was, indeed, a matter of great difficulty to determine accurately—what particular "vessels" were enlarged, hidden as they mostly were beneath the skin and its subjacent tissues. Nay, it is not too much to say, that the statement that it is the arteries that are enlarged is purely hypothetical, and not based upon an actual demonstration of the facts. Secondly, it will be also evident from the statement just quoted from Dr. Brown-Sequard, that venous hyperæmia, the result of the blood being forced out of the arteries by their partial contraction, "very nearly accounts for all the effects of this section." The truth of this will not only appear from what is to follow now, but from the effect of other sections to be noted.

Notwithstanding an increased afflux of blood, and consequently a relative elevation of temperature, with heightened sensibility, "the intimate acts of nutrition appear to be modified in nothing. . . . Nor does it appear that this hyperæmia, however intense or prolonged it may be, has ever the effect, save under exceptional circumstances, of determining by itself the development of inflammatory action." (M. Charcot, "Lect. Nerv. Sys.," pp. 90–91.) This could hardly be the case if the hyperæmia were arterial.

Among the effects of this section on muscles, as reported by Dr. Brown-Sequard, are contraction of the pupil, retraction of the eyeball, partial closing of the eyelids, contraction of "almost all the muscles of the eye," and also of the muscles of the angle of the mouth and nose; contraction of the erectile muscles of the ear, and others. Now, seeing that it is *contraction* and not relaxation of all these muscles, which follows section of this nerve, the law of analogy would require that the muscles of the arteries supplied by this nerve be contracted also; otherwise the anomaly would exist of the same nerve producing contraction in a large number of muscles and relaxation in a single instance. Why should the arterial muscle be regarded as an exception among

Digitized by Google

so many others, especially when all the facts of the case are compatible with arterial contraction and venous fullness?

As for the second part of the experiment, in which the hyperæmia is dissipated by faradisation of the distal end of the cut nerve, that is easily accounted for. The terminal branches of the cut sympathetic evidently influence the muscles of the head and face over a wide area. As is well known, the effect of faradisation is to set up a succession of rapid contractions and relaxations in muscular tissue. The pressure thus brought to bear on the swollen veins would amply suffice to force their contents onward, and thus to dissipate the venous congestion. Examples of this very result are not lacking. Thus, when Kölliker applied one pole to the umbilical artery and vein of a fresh human placenta, there followed contractions by which the veins forced out their contents and changed into bloodless strings. ("Meyer's Elec." Hammond, p. 88.)

The following quotations from Rosenthal's "Diseases of the Nervous System," Vol. II, Wood's Library, have a peculiar fitness here: "Kussmaul and Tenner have shown, in a series of experiments, by placing a watch-glass in the opening of a trephined skull, without allowing the air to enter (Donder's plan) that compression of the carotids causes capillary anæmia and venous hyperæmia of the brain and meninges." (p. 64.)

"In Verneuill's patient, upon whom ligature of the carotid was performed for a tumor of the parotid gland, persistent contraction of the pupil developed shortly afterward, with rise of temperature and vascular dilatation upon the temple and gums, and abundant perspiration upon the side of the face corresponding to the operation. All these symptoms can be produced experimentally upon animals by dividing the cervical sympathetic." (p. 26.)

Here is a remarkable proof that the section referred to causes arterial contraction (and not dilatation), seeing that the other effects of the section are equivalent to those produced by ligature of the carotid.

Section of the Splanchnics.—In a "demonstration of the vasomotor functions of the splanchnic nerves," the chief editor of the "Handbook for the Physiological Laboratory" (Amer. Ed., p. 258) informs his readers that these nerves contain vasomotor fibres which "are distributed to the arteries of the abdominal viscera."

We approach this "demonstration" expecting to find that when these nerves are cut the predicted results will follow; in the arteries they supply being more or less "relaxed" or "dilated." What is our disappointment to find in all that follows in this chapter of the "Handbook" the arteries are never once alluded to! Thus the very pith and point of the so-called "demonstration" is entirely ignored! What occurs is thus stated by Dr. Burdon Sanderson: "After section of both nerves the vessels of all the abdominal viscera are seen to be dilated." What "vessels" are these? Not the arteries, because Dr. Burdon Sanderson continues: "*The portal system is filled with blood;* the small vessels of the mesentery and those which ramify on the surface of the intestine are beautifully injected; the vessels of the kidney are dilated, and the parenchyma is hyperæmic; all of which facts indicate, not merely that by the relaxation of the abdominal blood vessels a large proportion of the resistance to the heart is annulled, but that *a quantity of blood is, so to speak, transferred into the portal system, and thereby as completely discharged from the systemic circulation as if a great internal hemorrhage had taken place.*" (p. 260.) [Italics mine.]

It needs no italics to give point and force to this remarkable admission. It is merely stating, with a little circumlocution, that the arteries are empty and the veins are full! The "beautiful injected vessels," which the learned editor so much admired, are not arteries but veins, the blood in which has become "bright red, like arterial blood," as Prof. Kuss explains of venous blood in the mesentery, "because oxygenation has been effected simply by exposure to the air." ("Lec. Phys.," p. 326.)

Digitized by Google

The contraction and emptiness of the arteries after section of their vasomotor nerves is thus proved on the very highest authority. Where now is the justification of the assertion that after a section of this kind the arteries are dilated and hyperæmic?

Whatever obscurity there might be as to the actual results of section of the cervical sympathetic, for obvious reasons, there can be no mistake as to the results here.

Now the law of uniformity of cause and effect demands that what is true of the relative state of the arteries and veins after section of the splanchnics, must be true also after section of the cervical sympathetic; and since the arteries are thus shown to be empty and the veins full in the former case, the same condition must be held to prevail also in the latter.

It is worthy of note, in this connection, that both after section of the spinal cord and after section of the splanchnics, blood pressure falls, and in both cases may be restored by faradisation of the divided cord or nerve. It is evident from this that the fall of blood pressure (as shown by the kymograph in the carotid) on section of the cord is not to be regarded as an indication of arterial relaxation, as appears to have been done; because blood pressure fell also after section of the splanchnics, where we know positively that arterial dilatation could not have taken place.

It may be asked, how could faradisation of the spinal cord or of the nerve restore the pressure or tension in the arteries, if the heart and arterial system were already empty? Dr. Burdon Sanderson supplies the answer indirectly, in stating: "It is seen that after section of the cord the heart is flaccid and empty, and that its cavities fill and its action becomes vigorous when the vascular contraction caused by excitation of the peripheral end [of the cut cord] forces the blood forward so as to fill the right auricle." (p. 251.) Now the only blood which could be "forced forward so as to fill the right auricle" is *venous* blood from the distended portal system. Thus it will be seen that all the facts fit and, as it were, dovetail into each other, in establishing that nervous paralysis and contraction of the arterial muscle go together, the result being hyperæmia, not of the arteries but of the veins.

The explanation just quoted from the Handbook, as to the forcing forward of the venous blood as an effect of the faradic current, confirms the explanation made above, as to the dissipation of the venous hyperæmia by the same current after section of the cervical sympathetic.

State of the Arteries in Death.—Not only are the arteries invariably as empty as their physical structure will permit them to be, when their nerves are cut or paralyzed in the living body, but such is also their condition *in death* of the body, when nerve force is extinct. This is a fact too well known to need any special proof. It is a fact, however, which ought to be explained by those who hold that in a condition of nerve paralysis the arteries are "dilated" and hyperæmic.

The Operation of Pithing.—What has just been said of the contracted and empty state of the arteries is true also after the operation of "pithing" (in which the medulla and spinal cord are destroyed); as any one can easily satisfy himself, as I have done, by actual experiment. This is inadvertently proved to be the case by Dr. Burdon Sanderson in his account of an experiment designed to prove the contrary. Two frogs are taken. One is "pithed," in the other the nervous centres are uninjured. In both the heart is carefully exposed and the single ventricle slit open, so as to show the state of the great vessels. The experiment is intended to prove that in the pithed frog the arteries are "relaxed" and full of blood. On Dr. Burdon Sanderson's showing, the results are these: In the pithed frog, "although the heart is beating with perfect regularity and unaltered frequency, it is empty, and in consequence, instead of projecting from the opening in the anterior wall of the chest, it is withdrawn upward and backward toward the œsophagus." The heart and its appendages "are alike deprived of blood;" but on opening "the rest of the visceral cavity," "*the intestinal veins are*

Digitized by Google

distended." In these, "the whole mass of blood has come to rest, *out of reach of the influence of the heart.*" (p. 246.) How significant is this! If the arteries were dilated, and consequently full of blood, this blood could not be said to be "out of reach of the influence of the heart." But this is not all. The Handbook continues: "In the frog deprived of its central nervous system *only a few drops of blood escape*—the quantity, that is to say, previously contained in the heart and in the beginning of the arterial system. In the other, *bleeding is not only more abundant but continues for several minutes after the section.*" (pp. 246–296.) [Italics mine.]

Is it not evident that in the case of the pithed frog the arterial system promptly emptied itself into the now "distended veins," and had "only a few drops of blood" left to drain away through the open ventricle (the frogs being both suspended); while in the case of the other frog, whose nervous system was intact, this arterial contraction did not take place, and the arteries continued to bleed for several minutes till drained of blood.

The "Handbook for the Physiological Laboratory," from which I have quoted so often, occupies to-day a leading place as an exponent of physiological science. The reader who studies the details of the experiment just quoted will be surprised to find that here again, in an experiment specially designed to prove that "all the arteries are relaxed," the condition of the arteries is completely ignored, and never once alluded to! The arteries *ought* to be "relaxed," "dilated," and even "widely dilated" here, on the theory of the text-books, but they are empty and contracted, their final act being, as in death from other causes, "to drive their contents into the veins." (Kuss, "Phys.," p. 181.)

An Experiment of Dr. Brown-Sequard.—In this connection I must notice in the briefest manner an experiment of Dr. Brown-Sequard, in which the doctrine here supported is confirmed in a remarkable manner.

In a dog, a section was made of a lateral half of the spinal cord, just below the medulla. The result was extreme hyperæmia of the "blood vessels," to use Brown-Sequard's term, of one posterior limb, while the "blood vessels" of the other posterior limb displayed a state of spasm and ischæmia quite as extreme. "Very often the spasm persists for days," wrote the observer, "and it may be so great that the circulation is almost entirely suspended," so that "the cutting of the skin hardly gives a drop of blood."

The question at once arose, was the paucity of blood in one limb due to the excess of blood circulating in the other, or *vice versa?* Was the spasm on one side, or the dilatation on the other, the primary or direct effect, through the spinal vasomotor nerves of the half section of the cord?

In order to solve this question, Dr. Brown-Sequard made "direct experiments." Among others he ligatured the iliac artery feeding the dilated blood vessels of the hyperæmic limb, thus directing "almost the whole of the blood coming from the aorta" into the iliac artery of the limb in which the circulation was so much diminished. Notwithstanding this, the spasm was but partially overcome; "the temperature rose but little;" and "it was quite evident the small arteries near the toes did not allow the blood to pass freely."

Here was complete evidence, not only that there was spasm, but also that this spasm was arterial. Although the vasomotor mechanism of the spinal cord is as yet only very imperfectly understood, there seems no reason to doubt that this active contraction of the arterial muscles was here, as elsewhere, due to nervous paralysis, the result of the half section of the spinal cord.

More about the Arterial Muscles.—It will be obvious that the relative state of the arteries and veins in the foregoing experiments is incompatible with what M. Charcot calls "the paralytic dilatation" of the arteries, as a result of vasomotor nerve section,

Digitized by Google

and could not occur, if after this section the arteries remained "widely dilated," and "permanently larger," as asserted by other authorities already quoted. If this were the condition of the arteries, it is evident that they would be wholly incapable of contracting upon their contained blood, so as to force it forward through the capillaries and into the veins; an act depending entirely upon arterial contraction, because the force of the heart has already expended itself, and the capillaries have no muscular walls; while, that the veins are merely passive, is shown by the fact that they have no vasomotor nerves, and their calibre is not, as in the case of the arteries, regulated by nerve influence. (Dr. M. Foster's "Phys.," pp. 263-265.) Thus all the facts show that the arteries, so far from being "dilated" and "paralyzed," are undergoing active contraction.

Some recent authorities appear to suggest the modified idea that the dilation of the arteries, instead of being "permanent," as alleged by some authorities, is a temporary effect—"an opening of the floodgates," so to speak, in order to facilitate the transmission of blood to the veins. Thus Dr. M. Foster writes: "The section of the splanchnic nerves causes the mesenteric and other abdominal arteries to dilate, and these being very numerous, a large amount of the peripheral resistance is taken away and the blood pressure falls accordingly; a large increase of flow into the portal veins takes place and the supply of blood to the face, arms and legs is proportionately diminished." ("Phys.," 3d Amer. Ed., pp. 220 and 240.)

It would appear that here, as elsewhere, "the fall of blood pressure" is regarded as evidence of "lessened peripheral resistance," and a proof that the arteries are "dilated," the fallacy of which will presently appear.

We read again: "When the nervous system is destroyed, dilation of the splanchnic vascular area causes all the blood to remain stagnant in the portal vessels; and probably these, as well as other veins, are rendered unusually lax, so that the blood is largely retained in the venous system, and very little reaches the heart." (*Ib.*, p. 367.) And further: "When in the frog the brain and spinal system are destroyed, very little blood comes back to the heart, as compared with the normal supply, and the heart in consequence appears almost bloodless and beats feebly . . . the veins become abnormally distended, and a large quantity of blood becomes lodged and hidden, as it were, in them." (*Ib.*, p. 263.)

Here is the secret, both of the emptying of the arteries and of the fall of blood pressure. The blood comes to rest in the more capacious venous system (*Ib.*, p. 154), "out of reach of the influence of the heart." Now, seeing that the rapidity of the arterial circulation is such that only one-seventh of a second is required for blood to pass from the heart to the radial pulse, how long, think you, would be required to empty the arterial system of the pithed frog, seeing that at first little blood, and very soon no blood, finds its way back through the heart, into the arterial trunks? Why, the time required would be counted by seconds rather than by minutes. There would be no time and no necessity for the terminal arteries to dilate; the emptying of the arteries and the fall of blood pressure being amply accounted for by the fact that *blood is passing out of the arterial system faster than it is being returned to it.*

A precisely similar condition to that just described as resulting from nerve destruction, occurs also in the fatal stage of asphyxia. Here, too, the arteries are "contracted" and empty, and the large veins are so distended that "if cut into they spirt like arteries." (Dr. Burdon Sanderson, "Handbook, etc.," p. 332.) And here, also, Dr. M. Foster tells us there is a fall of blood pressure in the midst of general arterial contraction. He says: "On account of the increasing slowness and feebleness of the heart, the blood pressure, in spite of the continued arterial contraction, begins to fall; since less and less blood is pumped into the arterial system." ("Phys.," p. 445.) It will be seen that the parallel between the two cases is complete, and that the plain facts, as given by the

Digitized by Google

highest authorities, do away completely with the assumption that here the fall of blood pressure is to be regarded as a proof of arterial relaxation.

Even in the slower forms of death, when the process of emptying the arteries is more gradual, there is still no evidence of, and no necessity for, a dilation of the terminal arteries to give exit to the blood; for, granting that contraction of the terminal arteries would tend to hinder the outflow of blood, this effect would be counteracted by the stronger contraction of the larger arterial trunks above, forcing the blood through and out of the numberless terminal branches ending in the capillaries.

The facts thus far presented refer only to the great vasomotor areas of the cervical sympathetic and splanchnics. It seems unnecessary to attempt to discuss the lesser and local vascular mechanisms, about which little is known, and that little comes to us under the ægis of an erroneous theory. The greater always includes the less. What happens when the life of the chief nervous centres is killed, either by sudden and intended destruction, or in death from ordinary causes, happens also in a more limited area when local or subordinate centres are killed or paralyzed. Since in the former case the arteries are found contracted and empty, the same rule must be held to hold good in the case of the individual nerve and artery.

The Stimulation (?) of Asphyxia.—Is it not a strange proposition to put forward in the name of medical science, that an animal dying of asphyxia is actually undergoing a high degree of nervous excitation? Yet such is actually the teaching of the text-books in physiology to-day! Dr. Burdon Sanderson, treating of asphyxia, says: "One of the effects of diminishing the proportion of oxygen in the blood is to excite the vasomotor centre, and thus to determine general contraction of the small arteries. The immediate consequences of this contraction is to fill the venous system." As the process advances, "the heart's contractions become more and more ineffectual till they finally cease, leaving the arteries empty and the veins distended." (Handbook, etc., p. 333.)

There is no mention here of arterial relaxation or dilation to facilitate the outflow of blood. On the contrary, "the immediate consequences" of "a general contraction of the small arteries" is "to fill the venous system," and in a few minutes "the arteries are empty and the veins dilated," the animal being dead. This is precisely the condition which we have seen in a former page to be the direct result of destruction of the nervous centres. It is a process which invariably prevails in the dying and is complete in death. Thus, according to Paul Bert, quoted by Prof. Kuss, "death is always owing to asphyxia." ("Phys.," p. 330.)

Why has it been assumed by physiologists that in this rapid sinking into death the nervous centres are undergoing an unusual excitation? Because, as we have just seen, there is "a general contraction of the small arteries," and other spasms and contractions of the respiratory muscles fixing the chest and arresting respiration; and in accordance with the theory of the day, these spasms and contractions of the muscles depend on active discharges of nerve force, stimulating the muscles to contact. How is this assumed extraordinary activity of the nerve centres to be accounted for in an animal actually dying? There is a "physiological law" which declares that the activity of an organ is directly dependent upon its receiving a due supply of arterialized blood (Dr. C. B. Radcliffe); and Dr. W. B. Carpenter has said of venous blood, that "it exerts a depressing influence upon the nervous centres," from which they are at length "completely paralyzed." ("Hum. Phys.," p. 537.)

One would have imagined that bad blood, deficient in oxygen and loaded with carbonic acid, would have been the very last thing which a physiologist would have chosen as a pabulum from which to generate an excess of nerve force! and, doubtless, the choice was embarrassing enough. But necessity compels. The exigency of the theory is inexorable. Muscular contraction without nervous stimulation is deemed impossible,

Digitized by Google

and there being nothing else to fall back upon, it has been assumed that impure, non-arterialized blood plays the part of a stimulant to the nervous centres.

Accordingly, we find a recent and popular writer—Dr. J. Milner Fothergill—in his "Antagonism of Therapeutic Agents," declaring that "the more venous the blood the greater the activity of the respiratory centre. The effect of venous blood is to augment the natural explosive decomposition of the nerve cells. . . . The effect of defective arterialization causes more rapid as well as deeper breathing; more perfect and extensive respiration is set up until properly oxygenated blood is procured." This author would almost lead one to believe that a kindness was done to the rabbit in having its vagi cut. He says: "When the vagi are cut, the respiration is modified; it becomes deeper and more prolonged, fuller and more complete." (p. 88.)

But, unfortunately, this view of an apparently improved respiration is wholly delusive; for, as Dr. Burdon Sanderson tells us, "notwithstanding the vigor of the respiratory movements, the blood becomes more or less venous,"—the animal is dying, and does die, "commonly before the end of the first day." (Handbook, p. 317.)

Let it be kept in view that the theory of the day explicitly teaches that "the muscles receive from the nervous system a preternatural stimulus to action" (Dr. Pereira, Vol. II, p. 541), and that spasm and convulsion "are dependent upon excessive activity of the spinal centres" (Dr. W. B. Carpenter, *Ib.*, p. 846), and we shall see presently to what apparent absurdity this doctrine has led. In one of Kussmaul and Tenner's experiments, the carotid arteries are ligatured, with the effect of inducing "immediate loss of consciousness and general and violent convulsions," which are promptly recovered from, and nervous control over the muscles restored, as soon as the ligatures are untied and blood is admitted to the brain. Dr. M. Foster's view of this experiment is, that here "the nervous centres being no longer furnished with fresh blood, become rapidly asphyxiated through lack of oxygen." And yet, strangely enough, he holds that in this almost fatal condition of "rapid asphyxiation" the nervous centres are undergoing stimulation! for he adds: "similar anæmic" convulsions are seen after sudden and large loss of blood from the body at large; the medulla being stimulated by the lack of arterial blood." ("Phys.," p. 441.) Surely such a view as this may be gravely challenged, even when put forward on high physiological authority. Dr. M. Foster remarks in another page, in his chapter on "Death," that "blood is not only useless but injurious unless it be duly oxygenated." (p. 833.) And again he says of venous blood that if it "continues to be driven through a muscle the irritability of the muscle is lost even more rapidly than in the entire absence of blood. It would seem that venous blood is more injurious than none at all." (p. 126.) Why should nerve function be augmented by what is useless and injurious, not only to muscle, but to every other tissue in the body?

The Cheyne-Stokes Respiration.—What seems a lower depth of absurdity, if possible, has yet to be reached in the explanations of the Cheyne-Stokes respiration. I quote here from Dr. L. Sansom's "Physical Diagnosis of the Heart," by whom Traube's theory on this subject is said to be "the most plausible." According to Traube, "the first thing which occurs is the establishment of a condition of impaired irritability of the respiratory centre through mal-oxygenation; the long respiratory arrest gives time for the accumulation of carbonic acid in excess in the blood. Arrived at a certain maximum this begins to stimulate, slowly and imperfectly at first and afterward in increasing degrees, the centre, so that it develops the respiratory efforts till they culminate in dyspnœa. Then as the centre ceases to be stimulated or becomes exhausted, dyspnœa again supervenes." (p. 37.)

It will be observed that here the *deficiency* of oxygen and subsequently the *presence* of carbonic acid are made to play opposite and antagonistic parts! The lack of oxygen (instead of stimulating the medulla, as supposed by Dr. M. Foster) first enfeebles the

Digitized by Google

respiratory centre, in the medulla, and then, the same blood, still deficient in oxygen, but now loaded with carbonic acid, counteracts the previous depression, and tones up the weak nerve centre, so that, ere long, it displays extraordinary activity. But, unfortunately, this exhilarating pabulum—carbonic acid—is soon exhausted, and the nerve centre resumes its former feebleness till a new supply can be procured. The physiologist is certainly quite impartial, and allows the rivals to have their "innings" turn about. How such nonsense as this "most plausible theory" could find a place in physiological literature seems explicable only on the exigency of the hypothesis so long in vogue.

Filehne's theory in explanation of this state is more complicated, and at least equally absurd. Instead of the respiratory centre being stimulated (as Traube says), it is the vasomotor centre which is excited by the presence of carbonic acid. Arterial contraction follows, till "a gradually increasing anæmia of the respiratory centre" is brought about. This anæmic condition excites the respiratory centre, "and inspiration becomes more and more deep," till oxygen is supplied to the blood; "the arterial spasm is thus relieved," owing to the freshly oxygenated blood failing to stimulate the vasomotor centre (so as to contract the arteries), as the carbonic acid had previously done. With the relief of arterial spasm, and a consequent normal dilation of the arteries, "the anæmia of the respiratory centre passes off, and with it the exaggerated impulse to respiration, and breathing once more becomes superficial." (p. 137.) In other words, the respiratory centre functionates best when it is supplied not only with non-arterialized blood, but when it has too little even of that; as soon as the anæmia passes off, and this nervous centre gets a fair supply of blood, it ceases to act—suspends business—till the better times of bad blood and deficient blood come round again, when it is moved to activity once more!

There is still another explanatory theory to be noticed, which I find referred to editorially in the *Canada Lancet* for February, 1886: "Bramwell, who follows the teachings of M. Foster and others, supposes that the respiratory centre consists of two portions, one accelerating (or motor), and one inhibitory. He further believes that these two portions are acted on in opposite directions by the blood, whether arterial or venous. Thus, while venous blood stimulates the discharging cells of the centre and depresses the inhibitory portion, arterial blood acts in exactly the opposite direction."

"At the close of the period of apnœa, the discharging portion of the centre is stimulated by the venous blood," with its excess of carbonic acid, and this same blood, at the same time, is depressing the rival, or inhibitory part of the centre. The motor or discharging portion of the centre triumphs; respiration becomes established and even exaggerated. Unhappily the victor fails to "hold the fort." As soon as the blood becomes "fully oxygenated," the "inhibitory portion becomes stimulated, and gradually overpowers the discharging portion," so that "the respirations grow weaker and weaker until the state of apnœa results." Then the suspension of breathing restores the venous character of the blood and accumulates a store of carbonic acid, the stimulation of which reanimates the centre previously depressed by the presence of oxygen in the blood. Such appears to be the scope of this theory.

In this, as in the previous explanations, arterial blood is made to play the part of a depressor and paralyzer of the respiratory process, which it is constantly tending to arrest; but while paralyzing one portion of the respiratory centre it is stimulating another; and a similar double character is attributed to the action of venous blood. Thus during the brief time from the beginning of apnœa to the culmination of dyspnœa —a period rarely exceeding one minute—the blood passing to the brain is called upon to exert four different and even diverse effects; first as venous blood stimulating one part of the respiratory centre and paralyzing another portion of the same centre;

Digitized by Google

reverse effects being produced a few seconds later by the same blood on its becoming oxygenated.

One is really at a loss to understand how such an explanation could have been admitted to a place in physiological literature. Again, it is the exigencies of an erroneous theory which have led to such a complicated and unsatisfactory hypothesis.

If it be asked how the state of apnœa is induced by forced vigorous respirations, if it be not due to an excess of oxygen introduced into the blood, and how the opposite condition, or demand for air by breathing, seems to attend the absence of oxygen and the presence of venous blood, I can only answer as to the last that if no better explanation than that venous blood is a stimulant has yet been found, some better explanation is surely to be looked for. And as to the state of apnœa referred to, I find Dr. Austin Flint stating that, "according to Hoppe-Seyler, apnœa, in the limited sense above mentioned, is to be attributed, not to an excess of oxygen in the blood, but to fatigue of the respiratory muscles." ("Prac. of Med.," 5th Ed., p. 70.)

A New Theory Suggested.—Dr. Sansom regards the condition of the respiratory centre in this case as one of paresis and direct exhaustion. He shows that during the apnœal period "the arteries are strongly contracted." The proof of this is found in the rise of arterial tension; in the depression of the "great fontanelle" of the head, and also in the arrest of the process by the inhalation of nitrite of amyl, which dilates the arteries. On the theory of these pages, arterial contraction is due to vasomotor nerve depression or paralysis; and accordingly we find here that the vasomotor centre, as well as the respiratory centre, is depressed in function. It has been amply shown above, that contraction of the arteries occurs in the dying and is complete in death. It is also one of the prominent phenomena during the last stages of asphyxia and is invariably attended by venous fullness. The condition present during the stage of apnœa in the Cheyne-Stokes respiration, with its contracted arteries and dilated veins, appears to correspond very closely to that present as death approaches and in the latter stages of asphyxia. The original paretic and exhausted condition of the respiratory and vasomotor centres is aggravated by the further depression caused by mal-oxygenation of the blood; which, when venous and loaded with carbonic acid, is invariably a depressing and never a stimulating agent to nerve function. Vasomotor nerve failure induces contraction of the arterioles, systemic emptiness and venous engorgement, as the foregoing examples abundantly prove; and as a consequence, the great mass of the blood "becomes lodged and hidden, as it were" in the great venous trunks. At that moment death is very near, but as the heart continues to beat, it is fair to assume that a small quantity of blood still finds its way through the lungs, and, from its very scantiness, is capable of being aerated by means of the exchanges of gases still going on in the lungs, owing to the presence of residual air, during the temporary, partial or complete arrest of respiration. As a consequence, the quantity of blood reaching the nerve centres, though small, is at least partially oxygenated, and serves to revive the function of these centres "imperfectly at first," but with momentary improvement.

The effect of this revival on the vasomotor centre is to facilitate the dilatation of the arterioles; in which the pulmonary vessels share, permitting, ere long, the inrush of venous blood from the distended vena cava and portal system, and its transmission onward through the heart and lungs.

This corresponds to the period of increase in respiratory function, in which the laborious efforts of a feeble mechanism have been mistaken for an "exaggerated impulse" from excited and over-acting or "exploding" nerve centres.

Meanwhile, impure blood from the venous reservoirs (finding an entrance through the now fairly dilated pulmonary vessels,) begins to fill the lungs in such a quantity (as it is drawn onward by an inequality of pressure, toward the as yet unfilled arteries), that the whole mass of blood, failing to be arterialized with sufficient rapidity, again

Digitized by Google

becomes unfit for the maintenance of nerve function and the perpetuation of processes depending upon it.

In such a case, a previously weak organ or centre is the first to suffer. The medulla oblongata is such an organ in this case, and its contiguous centres for respiration and circulation fail together; bad blood and deficient blood, acting on centres previously paretic, or enfeebled, have done their work, and again the respiration is suspended. The vasomotor centre is again so functionally weakened that it loses control of the arterial muscle—the "inherent contractile force," which all physiologists assign to muscular tissue, thus freed (as in the examples enumerated above), induces "the strong arterial contraction" referred to by Dr. Sansom, which contraction of the artery is all the stronger the nearer nerve force is to cease in the extinction of life.

This arterial, or systemic contraction, again empties the lungs and refills the venous reservoirs, from which the blood is again drawn, at first slowly and then again more rapidly, as the process repeats itself.

Here, then, is an explanation of the Cheyne-Stokes respiration, based upon sound, though as yet unacknowledged, physiological principles, according to which paretic and enfeebled nerve centres are helped by their appropriate pabulum—oxygenated blood—and are overwhelmed and have their function suspended by what is naturally calculated to poison and paralyze them, impure, venous blood, deficient in oxygen and loaded with carbonic acid.

The Intestinal and Uterine Muscles.—In sustaining the contention that, as a rule, muscles of the involuntary class contract, not when stimulated by their appropriate nerves, but when deprived of nerve energy, I have not yet alluded to the involuntary muscular fibres of the intestines and uterus. The antagonism of nerve and muscle is not here so evident as in the cases already cited, but here the relations of nerve and muscle have not as yet been completely investigated. (Dr. L. Brunton.)

Dr. M. Foster states that section of the vagi "renders difficult the passage of food along the œsophagus," and causes "a spasmodic contraction of the cardiac orifice of the stomach; in other words, the tonic action of the sphincter is increased;" ("Phys.," pp. 346, 347),—facts which sustain what has been already stated above as to the non-paralyzation of the muscles concerned, after section of their nerves. The peristaltic movements of the intestine, he states, may occur "wholly independent of the central nervous system," and are "at bottom automatic" (p. 348). We have it on the authority of the late Dr. W. B. Carpenter, F.R.S., that "the intestinal tube from the stomach to the rectum is not dependent upon the nervous centres either for its contractility or for its power of exercising it, but is enabled to propel its contents by its own inherent powers." ("Hum. Phys.," p. 410.) So also of the uterus, the contractions of which are not due to a reflex activity of the spinal cord, but to its own inherent power of contraction; parturition having taken place after destructive injury and paralysis of the cord, and even after somatic death. (*Ib.*, pp. 979, 980.) In these cases, also, the nerve would seem to be useless as the ally of the muscle, but would play an important part in controlling and regulating, by antagonizing, its contractile energy.

I must notice, in this connection, an observation of Dr. M. Foster regarding the bladder. He says: "The escape of the fluid [from the bladder] is, however, prevented by the resistance offered by the elastic fibres of the urethra, which keep the urethric channel closed. Some maintain that a tonic contraction of the sphincter vesicæ aids in, or, indeed, is the chief cause of, this retention. The continuity of the sphincter vesicæ with the rest of the circular fibres of the bladder suggests that it probably is not a sphincter, but that its use lies in its contracting after the rest of the vesical fibres, and thus finishing the evacuation of the bladder. On the other hand, the fact that the neck of the bladder can withstand a pressure of twenty inches of water so long as the bladder is governed by an intact spinal cord, but a pressure of six inches only when the

Digitized by Google

lumbar cord is destroyed or the vesical nerves are severed, affords very strong evidence in favor of the view that the obstruction at the neck of the bladder to the exit of urine depends upon some tonic contraction maintained by a reflex or automatic action of the lumbar spinal cord." ("Phys.," p. 448.)

But this experiment admits of a very different inference. We have just seen, on the authority of Dr. M. Foster, that section of the chief motor nerves of the stomach "increases the tonic action of the sphincter" of the stomach, as we had before seen it does of the entire contractile tissues of that viscus. We have a right to look for a similar increase of tonic contraction in the bladder, when deprived of its nervous connection with the spinal cord, or when the latter is paralyzed. Admit that here, as in the examples cited above, the spinal nerves exercise a restraint over the contractile fibres of the bladder, tending to prevent its contraction. With this restraint intact, the bladder is able to bear a pressure of twenty inches of water before the sphincter is overcome; whereas, with nerve influence withdrawn by section or paralysis, and the muscular fibres of the bladder set free to contract (as in the case of the œsophagus and stomach), the resistance at the outlet, though also relatively increased, is overcome by the superior expelling force from above, with the aid of only six inches of water pressure.

The same principle applies to involuntary discharges from the rectum, which Drs. Todd and Bowman say is due not to paralysis of the sphincter, against which the fæces are driven, but to the "active pressure of the parts above which are not paralyzed." ("Path. Anat.," p. 180.) The "parts above" are the intestinal muscles, which, in the last stage of exhausting disease (when such discharges usually occur), have attained their freedom, just as the arterial muscles do under like circumstances, owing to the general decadence of nervous energy.

Vomiting of Pregnancy.—With the evidence before us as to the contraction of the gastric muscle on severance of its nerves, vomiting in general may surely be regarded as due to nerve depression rather than to nervous excitation. An additional observation in proof of the same is to be found in the fact that injury of the vagus may produce constant vomiting (Bryant's "Surgery," Amer. Ed., p. 208); and further, that vomiting is mentioned by Dr. C. Bastian among the symptoms of hemiplegia. ("Brain Disease," p. 56.) An explanation of the vomiting of pregnancy would be found if we might assume that a monopoly of nerve energy was being expended in the uterus, owing to the extraordinary development taking place in that organ, thus starving the gastric nerves, so to speak, which, no longer able to restrain the gastric muscle, permit the untimely and abnormal contractions of that viscus. That this occurs chiefly in the early months of pregnancy might be accounted for by the unusual demand rather suddenly made upon the nervous resources, which tend to equalize their expenditure, as the months go on, and the organism becomes accustomed to its new condition.

How Arterial Sedatives Act.—Ergot of Rye is an agent which produces, in a marked degree, contractions of involuntary muscular fibre everywhere, but whose effects are especially seen in the arterioles and uterus. Must not a uniform law or rule govern the occurrence of such contractions? We have seen that they occur best under a deprivation of nerve action, and are never so complete as in the general death of the body. How, then, can ergot be regarded as a stimulant? Who would ever think of administering it in cases of faintness and exhaustion as a restorative of nerve energy? Must it not act, like nerve section and nerve paralysis, in lessening the tone of the vascular and motor nerves, so setting free the contractile energy of the arterial and uterine muscles, which contract accordingly?

Dr. Sidney Ringer grows enthusiastic over the action of aconite in acute congestion of the tonsils, and that, too, in doses too small to reduce the action of the heart. Aconite undoubtedly causes contraction of the arterioles, and accordingly on the theory

Digitized by Google

of the day it must be classed as a stimulant, as it actually has been by some authors, Dr. Edward Meryon, M.D., F.R.C.P., for instance, who holds that "it stimulates the dormant fibres of Remak, and by so doing diminishes the calibre of the arterioles." ("Rational Therapeutics," p. 52.) Errors of this kind must be charged to the misleading guidance of an erroneous theory. Aconite is a profound paralyzer, and, in small doses, by lowering the activity of the vasomotor nerves, it frees the contractile power of the muscular bands of the arterioles, which contract accordingly, lessening or curing congestive states.

Is not this precisely the *rôle* of the galvanic current, when brought to play upon the cervical sympathetic, say in exophthalmic goitre? The thyroid gland and its appendages are being overfed by dilated arteries. Bring about contraction of these arterial tubes, by lowering the activity of the vasomotor nerves in the way just indicated, and the congestion and hyperplasia are relieved, if not cured.

But the electric current, for therapeutic purposes, has been classed as a stimulant! So has strychnia; so ought to be prussic acid, for it, too, causes spasms and convulsions of muscle! So is fatal hemorrhage. All stimulants, as well as aconite, on the theory of the day!

It would require a volume to elucidate these points, and I must condense what I have to say into a few paragraphs.

Strychnia a Paralyzing Agent.—Dr. Harley has shown that strychnia probably acts by preventing the oxygenation of the blood, which Dr. C. B. Radcliffe very properly holds cannot be the *rôle* of a stimulant. Dr. Ringer tells that "after traumatic and strychnia tetanus the functions of the motor nerves and muscles are depressed; the motor nerves conveying impressions imperfectly." But may not this motor nerve depression be due to a reaction from previous over excitement? Dr. Ringer says no! and adds, "Strychnia directly depresses motor nerves, for large doses kill without exciting convulsions, when the motor nerves are found to have lost their conductivity," ("Therapeutics," 5th American Ed., p. 499) which in physiological language means that the nerves are paralyzed.

Dr. W. A. Hammond has recounted an experiment performed by himself and Dr. S. Weir Mitchell which, he says, "shows that the action of strychnia is to destroy the nervous excitability from the centre to the periphery." ("Dis. Nerv. Syst.," p. 539.)

Dr. Ringer further furnishes strong evidence that paralysis, and not over-action, is the condition of the nerve centres in tetanus. He instances "certain poisons, like gelseminum and buxus sempervirens, which produce *at the same time* both weakness of natural coördinated reflex action, cord paralysis and *tetanus.*" He says "it is impossible that the tetanus should depend on stimulation of the cord, for we have seen that the tetanus was preceded by considerable depression of the cord and continues until the depression ends in extinction of all cord function;" or, as he says again, the tetanus "occurred in a dying cord." (London *Lancet*, Feb. 17th, 1887, p. 228; "Braith. Retros.," July, 1887, p. 98.)

In strychnia poisoning, death occurs from asphyxia (Fothergill, "Antag. Ther. Agents," p. 55), with its contracted and empty arteries and engorged veins—the precise condition of the vascular system produced by destruction of the spinal cord, as in pithing, as already shown in a previous page. Do not the foregoing facts show that strychnia does not kill as a stimulant, or excitant, of the spinal cord? Moreover, medical literature clearly shows the value of alcoholic stimulants in strychnia poisoning, but I cannot delay to quote it. On the other hand, chloral hydrate, which has some reputation in these cases, is "not by any means antagonistic" to the action of strychnia. It acts by simply lessening the contractile energy of the muscles, like other anæsthetics, by deoxidizing the blood, and thus retarding the chemical processes in the muscle, whereby its contractile force is generated. In this way the convulsions

Digitized by Google

are arrested, and time gained for the elimination of the poison. But dangerously large doses—seven or eight grammes—(about two drachms)—are required for this purpose. (Lyman's "Anæsthetics," Wood's Library, pp. 264, 267, 275.) "Strychnia affects paralyzed sooner than unparalyzed muscles," writes Dr. Ringer: but this is not exact. Strychnia does not affect the muscles at all, as Dr. Ringer himself shows; and the muscles are not paralyzed in the cases to which he refers. What he means is that strychnia induces twitches and spasms in muscles whose nerves are enfeebled, sooner than in muscles whose nerves are acting normally. Why is this? If strychnia were a stimulant, would it not sooner excite vigorously acting nerves than enfeebled ones? But since its effect is to cause "depression of the motor nerves," nerves already suffering in this way have their vital activity more easily extinguished, and their muscles set free, than is the case with healthy nerves. The same thing is equally true of the other paralyzer, electricity. Twitches, tremors, spasms and tetanus are all but varying stages of nerve paralysis and of muscular freedom.

Electricity a Paralyzing Agent.—Prof. Tyndall tells us that a mere trace of iron in the coils of a galvanometer, of even such splendid instruments as those used by Prof. Du Bois Reymond in his researches on animal electricity, caused a fallacious deflection of the needle, to the extent of thirty degrees and more. ("Heat as a Mode of Motion," p. 34.) It is therefore not to be wondered that erroneous conclusions were sometimes arrived at in experiments so beset with fallacies, even when conducted apparently with the greatest care.

So mysterious a force, which exhibits itself alike in the lightning's flash, in a tiny spark and in the quiver of the eminently sensitive protoplasm of a muscle, might well excite wonder and enthusiasm. As investigation proceeds, however, the exaggerated ideas as to the important part played by electrical currents in the phenomena of nerve and muscle, and even of life itself, which prevailed some years ago have been rapidly on the decline among students of electro-physiology; but will doubtless linger long in the popular and even in the professional mind.

But electricity is not nerve force, nor can it cause the generation of nerve force, which is impossible in a mere nerve trunk separated from its nervous centre. This must be obvious. If it produce effects equivalent to a loss of vital action such as occurs in the death or destruction of portions of the nervous system, it must be classed as a sedative and not as a stimulant. In the experiments about to be mentioned the currents employed are those used for ordinary physiological and therapeutic purposes.

The effect of such a current applied to the inferior laryngeal nerves is to induce spasm of the muscles of the glottis. "The rima is completely closed." (Dr. B. Sanderson, Hardbook, p. 308.) That is to say, it does precisely what we have seen above is done by section and paralysis of these nerves. Applied to the lower ends of the vagi it causes contraction of the œsophagus and stomach, and "in most cases vomiting" (Meyer's "Prac. Elec." Hammond, p. 87), just, as we have before seen, results from section of those nerves. We have had proof that section of the spinal cord and of vasomotor nerve trunks induces contraction of corresponding arterioles. Similar effect is produced by electrization of the same parts, the calibre of the arteries being sometimes reduced to one-sixth of their normal size. (Weber-Meyers, *Ib.*, page 88.)

Dr. M. Foster tells us that section of the spinal cord at the medulla, or in the dorsal region, arrests the secretion of urine; and such a section of the cord is of course a paralyzing act. He also tells us that electrization of the spinal cord below the medulla also arrests the secretions of urine. Then is not this a paralyzing act also? It is unnecessary to multiply examples. Shall we continue to call an agent a stimulant and refer to it as an excitant of nerve activity, the ordinary effects of which on nerves are equivalent to nerve section, nerve paralysis and death!

Mild Currents Paralyze.—It is sometimes said that powerful currents may paralyze

Digitized by Google

and even kill, but that mild or weak currents merely stimulate or excite. Is there any proof of this? Where in the records of electro-physiology do we find a claim for opposite effects from weak and strong currents? It is true that we are cautioned against the depressing effects of long-continued applications of even mild currents. But this is not to the present point. The short *seance*, with its mild currents, may and probably does afford a simulation of increased vigor, but this is mainly due to the moderate exercise which it gives the muscles and their consequently improved nutrition (Drs. Beard and Rockwell); perhaps also in some degree to the mental impressions of the patient. The longer *seances*, with stronger currents, are fatiguing and exhausting in proportion as they are depressing or paralyzing.

Is it not true that the weakest current which can affect a muscle at all causes a momentary contraction of the muscle; and that the strongest current that can be borne during life, or that can be brought to play upon a still irritable nerve and muscle after death, simply produces a more vigorous effect of the same kind; the contraction becoming continuous in spasm or tetanus? It is never contraction on one hand and relaxation on the other, unless, indeed, other conditions intervene and muscular contractile energy is at an end. As a matter of fact, weak and strong currents act precisely in the same manner, and differ only in the lesser or greater contraction of the muscle which they produce. The process is a uniform one, as indeed it must be, since a purely physical force cannot change its character, and play fast and loose in the mode of its operation.

The treatises on this subject bear ample evidence of the paralyzing effects of electrization when even weak currents are used, as could only be the case for therapeutic purposes. Althaus found that the electric current produced an anæsthetic and paralyzing effect on the ulnar and sciatic nerves. Drs. Beard and Rockwell tell us that "in rhinitis, pharyngitis and laryngitis,"—where only very mild currents are admissible,—"they have for years been accustomed continually to make use of the benumbing effects of electrization." ("Med. and Surg. Elec.," p. 123.) Even "weak electrization of the upper part of the neck may arrest respiration," as well as produce spasm of the glottis and of the muscles of inspiration. (*Ib.*, p. 133.) Currents necessarily weak, because applied to the neck of "a sensitive young lady," induced anæmia of the brain, with drowsiness and other effects indicative of arterial contraction. (*Ib.*, p. 134.) Other authors equally allude to the "paralyzing effects of the constant current,"—(Valentine, Matteucci, Eckhard and Meyers.)

From these considerations I hold that there is no evidence whatever that weak and strong currents produce opposite effects, or that one may paralyze and the other stimulate.

Direct and Inverse Currents.—A great deal has been written about the different effects of direct and inverse currents. Dr. J. Russell Reynolds, in reply to the question, "What current should I use to relieve pain and spasm, the direct or inverse?" answers: "All I have to say is that so far as I have seen it does not make the smallest difference. Theoretically it makes a very great difference, but practically it makes none." ("Clinical Uses," etc., p. 18.) Now, I think that the evidence showing that both these currents are paralyzing is indisputable. Take the direct current first. A nerve muscle preparation is prepared. To the middle of the nerve trunk a salt solution or the poles of an induction battery are applied, and in either case the effect is so regulated as just to fail to cause a contraction of the muscle. If, now, the poles of a galvanic battery are applied to the distant end of the nerve trunk, the positive pole furthest from the muscle, so as to produce a direct current, throwing the lower end into catelectrotonus, the muscle will contract at once. Hence the direct current is said to increase the irritability of the nerve. But electricity is not nerve force, and nerve force cannot be generated in a mere nerve trunk. The true change in the nerve is not

Digitized by Google

one of increased strength or vigor; it is simply that the feebly paralyzing action of the salt solution or of the induction battery has been supplemented or reinforced by the additional paralyzing wave of the direct current, and nerve force is for the moment annulled. What is just asserted is nothing new. Thus, "According to Volta, both directions of the current are depressing in their effects." (M. Meyer, p. 57.) Prof. Matteucci found that "the direct current" not only "diminished the excitability of nerves," but produced in them "a temporary paralysis." (Braith. "Epit.," Vol. II, p. 661.) Dr. W. B. Carpenter wrote, "The direct current weakens and at last destroys the excitability of a nerve." ("Hum. Phys.," p. 351.) So much for the direct current.

The inverse current produces in the nerve trunk, between the electrodes and the muscle, a condition of analectrotonus, which is admittedly one of "diminished irritability," which term is in itself an acknowledgment of lowered vital activity, which can only be accounted for as a degree of paralysis, and is induced by weak as well as relatively strong currents. Dr. C. B. Radcliffe states of M. Eckhard: "This very able physiologist has ascertained that so long as the inverse galvanic current is closed it is impossible to produce contraction of the muscle by pinching, pricking or otherwise acting on this part of the nerve . . . which is consequently in a state of suspended irritability." ("Epilepsy," etc., p. 75.) This is a state of paralysis, because "a nerve that is deprived of its irritability can neither receive impressions nor transmit them." (*Ib.*, 78.)

Drs. Beard and Rockwell say that "in regard to the differential action of the ascending and descending currents there has been an almost infinite amouht of shallow observation and impulsive writing." These writers offer ample evidence that the effects in question are due, *not to current direction*, but to *the physical effects of the poles*, at one of which acids accumulate and alkalies at the other.

Two Experiments.—Here are two experiments which show that the combined effects of strychnia and electrization are equivalent to the destruction of the spinal cord. In a rabbit undergoing the convulsions of strychnia poisoning the spasms will be at once arrested on breaking up the spinal cord by a wire thrust into the spinal canal. If instead of destroying the spinal cord in this manner it be subjected to electrization the spasms will be averted, or arrested if already present. The rabbit dies, but without the characteristic spasm. (Matteucci, Pereira, Radcliffe.) Is a powerful electric current needed here? Not at all. Quite a moderate current will suffice; because the strychnia poison is causing general contraction of the arterioles (Fothergill), filling the veins and deoxygenizing the blood. Asphyxia is also setting in from the same cause, joined with fixation of the chest by spasm of its muscles, whose motor nerves are being paralyzed. (Ringer.) Electrization produces parallel effects and intensifies the fatal processes already in operation. A weak current suffices to complete the arterial emptiness, the venous engorgement and the non-oxidation of the blood. The spasms cease, probably, because such blood as is now present is inimical to the life of the muscle, and destroys its contractile energy more rapidly than no blood at all. (Foster, "Phys.," pp. 126, 833.)

If the theory of the day were true the rabbit ought not to have died! With the stimulating and vitalizing action of an electric current added to the previous exhilaration of strychnia stimulation the rabbit should have lived and flourished, in the interests of the theory, which alas! as usual, is found to be out of harmony with the facts.

Why does Dr. J. Russell Reynolds say that "it would be very unwise to use any form of electricity during the period of shock"? ("Lect. on Clin. Uses," p. 84.) Why do eminent authorities discourage its employment in cases of suspended animation, as in apparent death from drowning? (Dr. Ringer. "Ther.," p. 792.) Why does Dr. B.

Digitized by Google

W. Richardson, F.R.S., of London, write: "I feel it too unreasonable to recommend galvanic action as a means of resuscitation in threatened death from chloroform" . . fearing lest under the semblance of restoring life he should clench death! (*Medical Times and Gazette*, 1861; Braithwaite, Jan., 1873, p. 256.) These are precisely the conditions under which a "stimulant, tonic and vitalizer" should be eagerly sought for and diligently employed! It is evident that electrization is none of these, and therefore it is forbidden "in any form."

I think I am justified in claiming for the foregoing facts that they prove, as fully as any doctrine in physiology can be proved, that electrization as ordinarily employed is a paralyzing process.

Beneficial Effects of Electricity.—Electricity is no doubt a valuable therapeutic agent, and, like other paralyzing agents, does good in appropriate cases. But its beneficial effects may all be accounted for in strict accordance with its *rôle* as a paralyzer of nerve activity. Thus, it eases pain in a perturbed nerve by temporarily paralyzing it. It lowers the activity of the vasomotor nerves, and by thus setting free the contractile energy of the muscle it reduces the calibre of the arterioles, lessening or curing congestion, and consequently starving the hypertrophic growths. In other cases, by a momentary arrest of nerve action in the motor trunks, it induces prompt spasmodic contractions in the muscles, thus exercising them, and by attracting blood and pabulum to wasted muscles or tissues in the same way, it improves their nutrition. In chronic indurations and hyperplastic growths the purely chemical effects of the opposite poles or electrodes so modifies the nutritive activities of the tissues as to prove beneficial in restoring a more normal condition. Thus the curative effects of electrical treatment are all accounted for in strict accordance with its *rôle* as a paralyzing agent. To proclaim it, therefore, as "nature's own tonic," or to laud it as a "vitalizer" or extol it as the ally of nerve force, may be pardonable in the instrument makers, but is to be condemned on the part of scientific medicine.

How Therapeutics has Suffered.—It has sometimes been remarked that the department of therapeutics lags behind other branches of the medical art. Perhaps it will be pardoned if I venture to suggest that therapeutics has suffered greatly from the adoption of the *dictum* that electricity is a *stimulus* to nerve function. How much of a huge and hypothetical inhibitory system has found, perhaps, its chief support in this very error. When electricity stopped the heart, some mechanism had to be found for the arrest of its action by a stimulus. On what must the excitation expend itself? Not on the proper motor ganglia of the heart, which a stimulus would drive faster. To meet the exigency of the theory, it was necessary to imagine a purely hypothetical system of inhibitory nerves, the excitation of which, by antagonizing the proper motor ganglia of the heart, would bring it to a standstill. It is worthy of notice that in this experiment "the most marked effects are produced when the electrodes are placed on the boundary line between the sinus venosus and the auricles." (Dr. M. Foster, "Phys.," p. 232.) Now, this is the precise location of the chief motor ganglion of the heart in the frog,—the animal in which this observation has been made, so that the assumed stimulus has to pass over the proper motor ganglion in order to reach the supposed inhibitory ganglia, further away in the septum dividing the auricles! It needs explanation why, under these circumstances, the "stimulus" should ignore the motor ganglion in order to excite its rivals, which are further out of reach of the current.

The theory of the day on this subject, or rather the "temporary hypothesis," as Dr. M. Foster calls it, necessitates that the action of drugs be wrought out amid the struggle for supremacy between two rival nerve factions or camps, as it were, with results which are far from encouraging. For instance, a recent physiological work on the "Action of Medicines," informs us in the opening paragraph regarding belladonna, that "it paralyzes the motor nerves in frogs at the same time that it excites the spinal

Digitized by Google

cord; after they recover from the motor nerve paralysis the tetanic symptoms of spinal stimulation appear!"

Would it not be well to try how far the results might be simplified on the view that, under the circumstances, the heart's action ceased from paralysis of its motor ganglia;—thus dispensing for a time with this part of an inhibitory incubus, which threatens to become unmanageable through its very complexity?

The Voluntary Muscles.—The foregoing considerations have reference especially to the relations of nerves to involuntary muscles. Why it is that muscles of the voluntary or striated class do not also pass promptly into a state of spasm or contraction when their motor nerve trunks are cut, or when the body is dead, I am unable to explain; unless it be admitted that here the motor nerve trunks are more than mere carriers of nerve force—are in fact, with the nuclei and nerve plates at their endings, miniature magazines of nerve energy, which continue for a time to restrain the muscle after section of the nerve trunk or after somatic death.

Post-mortem Muscular Contraction.—If such an hypothesis were admitted it would serve to explain certain phenomena for which an explanation is necessary, such as the remarkable contractions of muscles which are known to occur in certain cases after death. There can be no doubt that the activity of both nerve and muscle survives for a time the death of the organism. The life of the nerve which is more intimately dependent upon vital conditions succumbs before that of the less vital and more enduring contractile power of the muscle. (Foster, "Phys.," p. 121.) And as one fasciculus or one muscle or one group of muscles attains its freedom the contraction which follows gives rise to the movements referred to.

Rigor Mortis.—Is a muscle contracted or shortened when it passes into rigor mortis? All observers agree that such is the case, and Dr. M. Foster tells us that the shortening and contraction "may be considerable." ("Phys.," p. 94.) Is this contraction and shortening the last act of the muscle in dying, or does it occur after the actual death of the muscle—that is, in a dead muscle? Let us consider the latter view first, since it appears to be the one in favor by our physiological teachers at the present time.

If the muscle be dead, not only is its nerve force extinct, because nerves die first, and, consequently, there can be no stimulus from nerve energy to cause the muscle to contract, and, further, the chemical changes in the muscle which generate its contractile force must also have ceased to operate, so that its contractile power is at an end. In the assumed absence of contractile energy it has become customary to attribute the death stiffening to coagulation of the muscle plasma in the muscle. This would account for the rigidity of the muscle, but would fail to account for the contraction and shortening admittedly present. Muscle plasma, in the living muscle, bears the same relation to the myosin of dead muscle that certain albuminous substances in the circulating blood do to fibrin, after blood is drawn off in a vessel. According to Dr. Lionel Beale, fibrin is "non-living matter, and is the product of the death of albuminoid bioplasm." ("Disease Germs," pp. 136, 137.) If this be true of fibrin, it may fairly be assumed to be true also of myosin, which closely resembles the former. Coagulated plasma, or myosin, is dead, and if the muscle also be dead, and its inherent contractile power at an end, in what manner does dead myosin acting on a dead muscle produce so perfect a counterfeit of muscular contraction, that one of the keenest observers of the day pronounced it "The most steady and persistent contraction which muscle can possibly exhibit," (Anstie, "Stim. and Narc.," p. 70); so perfect a counterfeit, indeed, that our eminent English physiologist, the late Dr. Carpenter, employed the microscopical appearances of muscle during rigor mortis as the chief basis for his description of the changes taking place in ordinary muscular contraction, as he himself has told us. ("Hum. Phys.," 5th Amer. Ed., pp. 307, 308.)

Again, the reaction of a living muscle in repose is neutral, or alkaline, but after

Digitized by Google

exercise, or tetanus, the reaction becomes acid, an effect in some way depending upon the chemical processes in the muscle associated with its contraction. In rigor mortis the reaction becomes "most distinctly acid" also. But if the muscle be already dead and these chemical changes at an end, what is the source of the acidity? To the presence of this acid, the coagulation of the myosin and the rigidity of the muscle are of late attributed. But since the acidity is the *result*, or *effect*, of muscular contraction in the living muscle, how can it be the *cause* or starting point of the contraction and stiffening in the dead muscle?

Dr. Lauder Brunton finds that muscle plasma "coagulates too quickly in the muscles of warm-blooded animals to allow of its preparation from them." Now, rigor mortis does not usually set in for several hours after death—Dr. Brown-Sequard found it to be ten hours in four rabbits—and its onset may even be artificially delayed. The statement, therefore, is only explicable on the supposition that coagulation of the muscle plasma and rigor mortis do not occur together—that is, as cause and effect. It would seem to be implied that the muscle plasma coagulates too early to be the cause of rigor mortis. Dr. Brunton further shows that the muscle plasma may coagulate without producing rigor mortis. In an experiment, detailed on page 363 of the Handbook, it is shown that, if half a fresh muscle be immersed for a few minutes in water at a temperature of 104° Fah., the reaction will be acid, as Dr. Brunton says, "from development of rigor mortis." The other half of the muscle is to be placed for a similar time in boiling water; and here the reaction "will be alkaline." Dr. Brunton adds: "Before rigor mortis had time to set in the albumen of the muscle was coagulated. This coagulation set free a quantity of alkali, hence its reaction." Dr. Brunton's exposition of this experiment, if correct, would be fatal to the myosin hypothesis, since if the coagulation of the muscle plasma be attended by an alkaline reaction, while in rigor mortis the reaction is strongly acid, the former could not be the cause of the latter, and they must be regarded as separate and distinct processes.

The foregoing difficulties certainly seem to create distrust in the myosin hypothesis; and we now turn from it, with its dead muscle and inert myosin, to the other aspect of the case, under which the complete cessation of nerve activity and the final contraction of the muscle marks the onset of rigidity. "The rigidity, the loss of suppleness and the diminished translucency," observable in the muscle in this state, are reasonably accounted for by the condensation of tissue, which is here permanent, as the contraction is continuous. That a certain relaxation subsequently occurs, during which meat or game, which is at first tough, becomes more tender and toothy, is attributed by M. Rosenthal to the action of the acid referred to, which relaxes the connective tissue which holds the fibres together, so that the latter separate more readily. ("Muscles, etc.," p. 87-8.) This is but the beginning of the chemical change which ends muscular contractility in the ruin of putrefaction. The following remarkable series of conditions are common both to muscular contraction and to rigor mortis. In both the reaction becomes acid. In both carbonic acid is set free in the muscle. In both the temperature rises—often markedly so in rigor mortis. In both the muscle is contracted and shortened; in some cases, as in death from cholera, "rigor mortis may be said to be simply a continuation of the cramps and contractions occurring during life." ("Wood's Prac.," Vol. I, p. 717.) In both, glycogen is converted into sugar. Do not all these coincidences in appearances and effects point strongly to a similarity of processes in muscular contraction and cadaveric rigidity? Of course the parallel is not complete in every particular. It is said that the muscular sound emitted during ordinary muscular contraction is absent. This sound is attributed to vibration of the muscle substance. Might it not be due in part to the altered circulation in the ordinary muscle during contraction, for it is well known that the blood channels, under certain circumstances, give out a musical note? In rigor mortis, of course, the circulation of the blood ceases, as

Digitized by Google

does also the removal of waste products. That the muscle substance continues to vibrate in rigor mortis is evident, because chemical changes are still taking place there, as is shown by what is said above, and especially by "a marked accession of heat" (Foster, p. 542); and "heat is only another form of motion." (Rosenthal, p. 42.) So that, after all, it would seem as if the atoms of the muscle continue to vibrate, even though no sound is audible.

That indefatigable observer, Dr. Brown-Séquard, some time ago, related to the Biological Society of Paris some experiments he had made, by a special instrument, to determine the movements of single muscles in the body after death. He found that there was a very considerable degree of contraction and relaxation, as much, for example, as two and a half millimetres in a muscle measuring only six millimetres in length. He thought that the results of his experiments disproved the theory of coagulation in the muscular tissue as the cause of cadaveric rigidity. (*N. Y. Med. Rec.*, Jan. 9th, 1886.)

I am not necessitated to prove that rigor mortis is due to post-mortem contraction of the muscles; but in the absence of any other satisfactory explanation of this state, I am entitled to refer to it in support of my thesis; and I would ask those who dissent from this view, and who, in consistence with their theory, must hold that nerve stimulus is necessary to muscular contraction, to account for the presence of nerve force under the conditions referred to.

Spasms in Voluntary Muscles.—It would, perhaps, be no difficult task to show that even voluntary or striated muscles pass into a state of partial spasm or contraction during life, much oftener than might at first sight appear, under a form of "irritation," which may very properly be regarded as consisting in a lowering of nerve activity.

"Irritation" is not increased nerve action. A splinter under the nail is attended by a loss of tactile sensibility. A mote in the eye irritates, but it obscures vision. Why should indigestible food oppressing the digestive functions of a child be regarded as a source of increased nervous "discharges"? Such sources of irritation ought to be considered as depressing rather than exciting nerve action; a view of the case for which authorities have been already quoted, and others are to follow.

Dr. Anstie wrote: "Convulsive action of the muscles, as every one knows, are very common complications of neuralgia," and the same acute observer held that "pain is not a true hyperæsthesia; on the contrary, pain involves a lowering of nerve function." ("Neural.," p. 12.)

Dr. Hilton, in his work on "Rest and Pain," points out that the irritation of peritonitis induces contraction of the abdominal muscles. In the same way, pleuritis renders the chest walls fixed by spasmodic contraction of its muscles; while the muscles of an inflamed joint, he says, "are invariably contracted, and continually tend to increased flexion of the limb, not because such a position is easiest for the patient, which is not always the case, but owing to a reflex perturbation transferred to the muscles of the adjoining surface." (p. 96.) That peripheral irritations *do* produce nerve paralysis must be admitted, on the authority of Dr. Brown-Séquard. ("Lect. Cent. Nerv. Syst.," pp. 160, 170), and others.

What is the "irritation" in these cases but a mild form of nerve paresis, just as "the irregular muscular action" which shows itself in tremor, fibrillary contractions, or in spasm, denotes the failure of the ordinary nervous restraint over the corresponding muscles.

Why should "morbid conditions of the medulla oblongata," avowedly depending on "defective nutrition," be supposed to give rise to "explosive and atactic manifestations of nerve force" (Anstie, "Neural.," p. 156), when they are much more naturally explained as depending upon nerve failure? The weak point in the theory of the text-books is, that nerve force is required to be displaying the full activity of robust health, and even more, in exaggerated "discharges" and "explosions" at the very

Digitized by Google

time there is the most undoubted evidence of nerve failure and exhaustion. Why, in cases of "early and late rigidity" of muscles, should a clot in the brain be held to be an exciting irritant, seeing that the brain tissue is wholly insensitive, and may be cut, pricked or seared with a red-hot iron without eliciting any signs of pain? It is difficult to express here the multitude of facts which show the very frequent association of paralysis and spasm in disease of the brain and spinal cord. The paralysis is of the nerve and the spasm of the muscle—conditions very embarrassing to the theory of the day, but consistent and harmonious states in the theory of these pages. Is there not much significance in the statement of Seguin, that "a lesion of the lateral columns of the spinal cord produces paralysis with contracture" of muscle. Why? Because, as Dr. Brown-Séquard has shown, "the motor fibres run on the exterior of the cord in its antero-lateral columns." (Erichsen, "Concus. Spine," pp. 29, 30.) Motor nerve disease and destruction induces contraction of the muscle, which later on becomes atrophied, partly, no doubt, from inaction.

It is on record, too, that while injury of the vagus nerve induces contractions of the gastric muscle, injuries of the spinal accessory nerve are attended by spasms of the trapezius or sterno-mastoid muscles. (Bryant's "Surgery," p. 208.) Other examples of a similar kind are not lacking.

One might imagine that Dr. B. W. Richardson, F. R. S., intended to endorse the theory of these pages when he wrote as follows regarding the convolutions of the drowning. He says: "The convulsive movements that are seen are unconscious movements; they are the same as those which mark the period of stupor, in death by hanging, by noxious vapors, by concussion; and they are simply the results of action of muscles from which *the controlling power of the nervous centres has been removed.*" (Braithwaite, July, 1871, p. 255.) [Italics mine.] Dr. Henry M. Lyman, A. M., M. D., would appear also to have had a commendable distrust, if not an entire disbelief, in the theory of the text-books, when, in referring to "a temporary increase of muscular movement directly caused by the abolition of some special source of nervous impulse," he says: "Witness the tremendous *liberation of muscular movement* which follows a *paralysis* of the influence of the brain, by the sudden decapitation of a fowl, for example." ("Anæsthetics," Wood's Lib., p. 26.) [Italics mine.]

One of Dr. Ferrier's experiments is so much in point here, that, at the risk of being tedious, I cannot forbear a brief reference to it. The right brain of a monkey had been exposed and subjected to faradisation. Next day the animal "was found perfectly well." "Toward the close of the day following, on which there were signs of inflammatory irritation and suppuration, it began to suffer from choreic spasms," which rapidly assumed an epileptiform character. Next day hemiplegia became established, with the usual symptoms of "paralysis of the left arm and partial paralysis of the left leg." "On the day following paralysis of motion was complete over the whole of the left side, and continued so till death, nine days after." Dr. Ferrier says, "In this we have a clear case of vital irritation producing precisely the same effects as the electric current, and then destruction by inflammatory softening resulting in complete paralysis, etc." ("Functions of Brain," pp. 200, 202.)

On Dr. Ferrier's view, the stage of apparent inflammatory action was accompanied by increased production and discharge of nerve energy, as seen in the choreic and epileptiform spasms. But "Recent studies show that the inflammatory process is a destructive and depressive one, so far as the tissues are concerned; that it does not irritate and kindle into increased activity the protoplasm of the cells, but rather the reverse." (Editorial, *N. Y. Medical Record*, January 30th, 1886, p. 128.) So that it is now definitely understood that the inflammatory process in brain tissue does the reverse of Dr. Ferrier's view, and paralyzes rather than excites nerve energy.

Observe here, that the spasms of the muscles, on Dr. F.'s own showing, began to

Digitized by Google

occur contemporaneously with the "signs of inflammatory irritation and suppuration," and as this term "irritation" (on so good an authority as the able editor of the *N. Y. Medical Record*), must now be interpreted to mean depression and lowering of cell activity, it follows that the spasms referred to occurred from the absence or failure of nerve energy, and not from its undue excitation. Observe, too, that Dr. Ferrier held that this "vital irritation," as he saw it, but which we now know is depression or paralysis, produced "precisely the same effects as the electric current." Another evidence of the paralyzing character of electricity!

The Epileptic Paroxysm.—With the experiments on the cervical sympathetic and splanchnic nerves before us, how can we say that the anæmia, or rather ischæmia, of the brain, which ushers in the epileptic seizure, is due to "excessive action of the spinal centres," compelling the spasm or contraction of the arterial muscles on which this ischæmia depends? Have we not had proof that the arterioles contract best when their vasomotor nerves are cut, or are paralyzed, or dead; and if so, are we not bound to hold that not excess but failure of nerve power is the proximate cause of the epileptic paroxysm? And is not the question of such excess or failure of nerve force a most practical one in determining the treatment?

How far is our comparative failure to cure this terrible disease due to our approaching it under the ægis of an erroneous theory—that nerve force here needed to be depressed rather than exalted? It is well for mankind that in this, as in some other instances, our practice has sometimes been directly at variance with the theory of the day. Thus we find Dr. Anstie assuring us that "our best anti spasmodics are stimulants": and that "alcohol is one of the best remedies possible in the convulsions of teething in children." ("Stim. and Narcot.," pp. 113, 129.)

No "Morbid" Nerve Force.—Spasms and convulsions frequently take place in the very act of dying, and under circumstances in which nerve force ought to be regarded as at a low ebb; as, for example, in uræmic blood poisoning. It is customary in some quarters to attribute these or other spasms to "a morbid irritability" or "a morbid nerve force"; as if the central nervous ganglia were capable of producing two kinds of nerve force, one normal and the other "morbid," and the spurious variety of attaining extraordinary power just in proportion to the complete failure of nerve force proper. A little reflection, I think, will show that this is untenable. Nerve force may be increased or diminished: its condition may be one of excess or of failure, but that it may present a duplicate of itself, and its *alter ego* produce effects for which nerve force proper is inadequate, and yet is responsible, is surely yielding too much to the exigency of an erroneous theory.

Medical literature presents numerous examples of this appeal to a "morbid nerve action," and it is rather surprising to find such a writer as the late Dr. Anstie referring to "the explosive disturbances of nerve force which give rise to the convulsions of tetanus" as "something quite different in kind" from healthy nerve action. ("Neural.," p. 8.) Now, if a nerve centre be thrown into action otherwise than by the exercise of its normal activity, then it is no longer the nerve centre which is acting, but a power extraneous to itself; a modern Archæus for which scientific medicine ought to have no place. And if tetanus be really due to an explosive activity of the nervous centres which are discharging nerve force with unwonted activity, surely to administer stimulants in such a case ought to be injurious, if not fatal! And yet we find that Dr. W. A. Hammond, of New York, has produced statistics in which "stimulants" stand at the very head of the list of curative agents in tetanus. ("Dis. Nerv. Syst.," 4th Ed., p. 541.) Here again the theory of the day is surely out of joint with the clinical facts.

Chloroform and Relaxation of Anæsthesia.—I have been asked how the rigidity, at first, and subsequently the relaxation, of the muscles during anæsthesia are to be

Digitized by Google

accounted for on this theory. The answer is easy. The rigidity is due to the partial paralysis of motor nerve influence, setting the contractile power of the muscle free to act. This occurs at a comparatively early stage of the process. The relaxation which attends complete anæsthesia is due to the loss of contractile power on the part of the muscle, owing to the absence of oxygen in sufficient quantity in the blood; for chloroform tends to prevent the oxygenation of the blood (Ringer's "Ther.," p. 286), and renders it venous in character. In this way the chemical processes on which the generation of contractile force in the muscle depends are retarded. (Lyman's "Anæsthetics," p. 28; Bryant's "Surgery," Amer. Ed., p. 318.) Dr. M. Foster states that "blood is not only useless, but injurious, unless it be duly oxygenated." And again, "if venous blood be driven through a muscle the irritability of the muscle is lost even more rapidly than in the entire absence of blood." ("Phys.," pp. 126, 883.) This, I think, will be accepted as a satisfactory explanation, in strict accord with physiological facts. The relaxation, however, is not so great but that faradisation of the muscle will induce a further degree of contraction; showing that the contractile energy of the muscle, though weakened, is not lost. That the contractile power of the muscle is thus lowered offers a bar to the prolonged or complete administration of chloroform during parturition, for obvious reasons.

The mode in which anæsthetics induce arterial contraction, as explained by Dr. Henry M. Lyman, may be quoted as follows: "Chloroform acting through the blood upon the nervous apparatus in the walls of the vessels, tends to paralyze the sensory endings of the nervous fibrils. This means a diminution of the normal impulses, which should continually reach the central intraparietal ganglia," in consequence of which "the motor cells no longer experience the inhibitory influence which they should receive from the periphery of their territory, and a liberation of a motor impulse excites muscular contraction, and we have vascular spasm," etc., as the result. ("Anæsthesia, etc.," p. 27.) This, of course, is purely hypothetical. The motor nerve fibrils in the muscular bands are ignored altogether, while a purely imaginary "inhibitory" system is invoked to meet the exigency of the occasion. How much better to hold that the motor nerve fibrils also are more or less paralyzed, and the arterial muscle directly set free to contract; thus dispensing with the inhibitory apparatus altogether.

The Nerve-Muscle Preparation.—It is impossible here to enter on a critical analysis of the experiments on nerve and muscle, which a careful examination will show to be wholly consistent with the views here advocated. When in a nerve-muscle preparation the muscle is made to contract by applying to the nerve trunk the shock of electricity, the corrosion of a chemical agent as a quick stroke, what is there to show that the effect on the nerve is not to cause a temporary cessation of nerve influence, rather than the production of a stimulus? There is really nothing, and the character of the impulse is merely a matter of inference. Even in what is called the rheoscopic frog, where contraction in one muscle imparts an influence whereby another muscle is made to contract, the molecular or electrical wave may as well be paralyzing as stimulating.

This Theory not New.—In hastening to conclude, let me state that, whether this theory of the antagonism of nerve and muscle be true or false, I am not entitled to the praise—or blame—of originating it. It was broached so long ago as 1832, by Dr. West, an English physician, and is said to have met with some countenance from Sir Charles Bell. Dr. C. B. Radcliffe, F. R. S., in his work on "Epilepsy, Paralysis and Pain," has warmly adopted the views of Dr. West, and offers some strong evidence in support of the proposition, that "there is reason to believe that ordinary muscular contraction is associated with a deprivation of nervous influence, and not with a contrary state of things." (p. 95.) I have here endeavored to support the same thesis, but with evidence drawn from other sources.

Objections to this Theory.—1. It has been objected to this theory that "a muscle can

Digitized by Google

contract when irritation is directly applied without the intervention of nerves." Now, I am not in the least disposed, or obliged, to dispute this assertion, for reasons which will appear later on. My thesis has much to gain, and nothing to lose, by the fullest admission of the independent irritability of muscular tissue. But it is exceedingly difficult, if not at present impossible, to say when a still irritable muscle has been deprived of "the intervention of its nerves." Certainly such is not the case in the experiments cited by Dr. M. Foster, in the Handbook heretofore referred to, where the experimenter, in order to produce the ideo-muscular contraction, is to choose "a muscle which has been much exhausted by treatment or by long removal from the body," and to "wait till neither muscle nor nerve give any ordinary contraction with an electric stimulus." It cannot be held to be proven that in such a nerve muscle there is not still remaining a force in the weakened nerve sufficient to control the equally weakened muscle.

Curare and the Motor Nerve Endings.—2. It has also been objected that, while the motor nerve endings are paralyzed by curare, the muscle does not contract, as it ought to do if this theory were correct. To this I have to reply, that if the muscles are not found contracted it is partly due to the insufficiency of the poisoning of the motor nerves, and partly to the fact that curare diminishes the contractile energy of the muscle. (Rosenthal, "Muscles, etc.," p. 254.) Nicotine and conine act precisely like curare (Ib., p. 253), and in the final action of these three poisons, motor nerve paralysis and spasm, or convulsion of the muscles, occupy a prominent place. (Ringer.) The special results vary, of course, in different animals. Nicotia sometimes acts like an anæsthetic (Stille and Maisch, p. 372); and the same is doubtless true of the others. Now, anæsthetics induce muscular relaxation by deoxidizing the blood; and nicotine is known to disorganize the red corpuscles which are the oxygen carriers. It is doubtless in this way that, under the slow action of these poisons, muscular relaxation is brought about. If death be rapidly produced by curare, convulsions occur. (Stille and Maisch.) Here the motor nerves are paralyzed before time has been afforded for the poison to lower the irritability of the muscle, which passes into tonic or clonic spasms according to its freedom, thus behaving as it "ought" to do. Is not this a sufficient answer to the objection?

But more remains to be said. The experiments with curare are not so conclusive as to be beyond the reach of criticism. They were intended to prove the independent irritability of muscle, which is now generally an accepted fact among physiologists. M. Rosenthal asserts that these experiments (and those of Kuhne upon the sartorius muscle) do not prove this; which is equivalent to stating that it is not proved that curare paralyzes the motor nerve endings.

More direct evidence upon this point is that of Dr. Onimus, who, not long ago, "read a paper before the Academy of Medicine, Paris, upon electro-muscular contractility and the action of curare. Contrary to the opinions of M. Claude Bernard, Dr. Onimus believed that curare does not act on all parts of the motor nerves, but only on their trunks; the nerve centres and terminal filaments being unaffected." (*N. Y. Med. Record*, 1880, p. 73.)

In view of these authoritative opinions (and doubtless of others to which I have not access), it is evident that this objection falls to the ground and loses the weight which otherwise might attach to it.

But suppose it were established beyond doubt that the influence of the nerve were completely eliminated from the muscle in any case, and that the contractile protoplasmic masses of the muscle were left wholly to themselves, and their life being not yet extinct, that they gave token of that still flickering life when comparatively rudely assailed by a a shock of electricity or a corrosive or injurious agent,—what then? Such

Digitized by Google

signs of irritability, elicited under such circumstances, would not militate against my thesis; for such would be the behavior to be expected from still living protoplasm, wherever found, and would in no way disprove the contention that in the association of nerve and muscle in the organism the *rôle* of the nerve is to restrain or control the protoplasmic energy of the muscle so long as their mutual relations continue. For, after all, "the contraction of muscular tissue is, in fact a limited and definite amœboid movement, in which intensity and rapidity are gained at the expense of variety." (Dr. M. Foster, "Phys.," p. 63.)

Indeed, I think the rational view of the situation just depicted turns the argument the other way; and tends to show that in the joint rôle of nerve and muscle the function of the nerve is *not* to goad or stimulate the muscle to contract. To suppose this is to assign to nerve energy the relative value of the fifth wheel in the coach. Such enduring power of contractility as the muscle here exhibits evidently needs no supplementary aid from the nerve. What it really *does* need, however, is restraint, control and coordination for the purposes of the organization of which it is a part.

Other Objections.—A further objection has been suggested, on the ground that on a nervous impulse reaching a muscle, an electric current is generated during the period immediately preceding the contraction of the muscle; but this is an objection which is only of any force on the assumption that electricity is a stimulant. There is nothing in the action taking place here to show that the electric current is a stimulant rather than a paralyzer. There is simply a "freeing of the forces in the muscle," just as the spark of electricity frees the forces bound up in gunpowder, and so fires the train. (Rosenthal, p. 250.)

As for the additional plea that nerve force and muscle force are too much alike for us to consider one a paralyzing and the other a contracting agent; that is merely begging the question. Nothing whatever is known regarding the nature of these forces; and the intimate structures of nerve and muscle are so widely different as to justify the idea that the product, so to speak, of each, is equally diverse.

This theory has been objected to as a proposed addition to the inhibitory system of the text-books. This is a mistake. If the views here enunciated were adopted, the huge incubus of the present inhibitory hypothesis could be in great part swept away, to the great advantage both of physiology and therapeutics.

If it be claimed that on the cutting of the spinal cord or of a nerve trunk, the "irritation" set up at the point of cutting, or the generation of electrical currents as the result of chemical change in the transverse section, act as a stimulus, and that contraction of the corresponding muscle is thus produced, such a claim must be regarded as untenable, for the following reason: The acts just referred to cannot be stimulating acts, because they are attended by precisely similar effects as are produced in the muscle by death from any cause, in which condition, it is needless to say, nervous activity is not increased. The proof of this has already been sufficiently vouched for, and need not be repeated here.

Of course, I do not pretend that all difficulties vanish in the light of the theory here advocated. There are very serious, if not insurmountable, difficulties in the theory of the text-books, as the facts of the foregoing pages fully show. What I claim is, that the view here presented rests on a rational basis, and, though presented very inadequately, and under many disadvantages, has the merit of furnishing a key to many obscure phenomena in the organism, and is entitled to the fair and candid consideration of the members of our profession.

Digitized by Google

DISCUSSION.

Dr. LOVE, of Atlanta, Ga., said:—Mr. President, I had hoped that some other member of this Section would have taken the lead in this discussion. I thank Dr. Poole for presenting that paper—thank him in my own individual behalf, and thank him particularly for presenting it in printed form, as he has. I have examined the copy I hold in my hand, and it shows no little research—research on a subject of much interest to physiologists—yes, to the profession. I hold that when a man has taken the trouble—and it is no little trouble—to make the research, gather the points, weigh the issues, draw the deductions and compile the conclusions and suggestions that Dr. Poole has in this paper, he deserves an expression of opinion from the members of the Section, but I had hoped that some one other than I would have been the first to lead out in the discussion.

To attempt to take up such a paper and discuss the points *seriatim* is simply impossible, for want of time; hence my remarks must be of the paper, and not its special points. The manner in which he handles his subject—with gloves off—makes me think of Laymon Blanchard's "*Quarrel with Old Friends.*"

But to the paper: I shall appreciate this paper, because on many of the points considered in it I have been frequently asked, by leading members of my classes, about these inconsistencies; and for myself I will say, too, that in practice I have found myself at a loss in pathology, because the teachings of physiology and the facts of pathology present such a want of harmony that, in my mind, they produce confusion, and I am at a loss to decide satisfactorily to myself. For instance, in one of the sections of the paper, treating of the relations of the nerves and non-striated muscles, I have come up with cases to which we have been taught to apply the term spasmodic stricture of the œsophagus, found at or above the cardiac orifice of the stomach. Is that condition of contraction a *spasm* of the circular fibres or a *paralysis* of the reflex inhibitory power of the nerve that inhibits the normal condition of the muscle in the performance of its function of keeping the cardiac orifice closed? What is it, and where is the fault? Is it in the nerve-increased action that closes this muscular end-band—or is it in this nerve-decreased action—*paresis*—that gives the "*open sesame*" to this gastric portal? I have met with cases of this character about which I could not clear up my pathology, hence could not feel clear in my therapeutia.

Again, in cases of spasmodic croup I have been at a loss. We have two sets of muscles there regulating the movement in these crico-arytenoid cartilages, with nerve supply from the motor filaments of the inferior laryngeal of the pneumogastric—one set holds the glottis open, the other closes it. Now in spasmodic croup, so called, what is the action? Is it *spasmodic contraction* in the one set, *or nerve paresis* in the other? These are some of the questions which the doctor's "quarrel with old friends"—as Laymon Blanchard would express it—is calculated to clear up, by springing the issues and settling the physiological questions upon which the solution of the pathological problems must be based.

Still another case in point pops up in my mind just here, to which, if my time will permit, I will refer. It is that condition of the bronchial capillaries, the minute ramifications—bronchioles—which we so often find so annoying to patient and practitioner, that comes up reflexly from uterine disturbances or menstrual derangement. Now, I am frank to admit that in many of these nervous phenomena, upon the teachings of the physiologists, when they are brought side by side with the positive pathological conditions we find to exist unmistakably then and there, the physiological

Digitized by Google

explanations are more confounding to me than the pathological facts. Look, if you please, to the explanation given by Brown-Séquard in his theory of the cause of the *rhythmical* action of the heart. Stating that he believes that its rhythm depends upon the action of carbonic acid, then instituting a series of experiments to prove his position, by vivisection and otherwise—the details of which I have no time to give—proves much more conclusively that the heart's rhythmic action depends upon oxygen than upon the carbonic acid for which he is contending.

Digitized by Google

THIRD DAY.

Dr. Wm. Abram Love, of Atlanta, Ga., U. S. A., presented a paper—

ON THE FUNCTION OF THE RED BLOOD CORPUSCLES AND THE ORIGIN OF NERVE FORCE FROM THE MAGNETIC CONDITION OF THE OXYGEN OF THE ATMOSPHERIC AIR.

SUR LA FONCTION DES CORPUSCULES ROUGES DU SANG, ET L'ORIGINE DE FORCE NERVEUSE DE LA CONDITION MAGNÉTIQUE DE L'OXYGENE DANS L'AIR ATMOSPHÉRIQUE.

ÜBER DIE FUNKTION DER ROTHEN BLUTKÖRPERCHEN UND DEN URSPRUNG DER NERVENKRAFT AUS DEM MAGNETISCHEN ZUSTANDE DES SAUERSTOFFES IN DER ATMOSPHÄRISCHEN LUFT.

In our search after truth it will be conceded that demonstrable facts will be accepted in evidence, whether such facts have been established as the results of recent research, or were the fruits of investigations in the distant past, provided they remain uncontroverted or are still susceptible of demonstration.

The inquiring mind of man has, in all ages, led him forward in his inquiries into the unknown. Such investigations have often led to the discovery of facts in the laws governing forces, regulating conditions or establishing principles. These things may be recognized as facts, and still may serve no special purpose until such time as they may be applied for the accomplishment of certain ends. It was a *fact* observed from the beginning that the application of fire to water would result in the production of steam, but many ages elapsed before that steam was harnessed up and made to bear our burdens and do our bidding. Michael Faraday, in the earlier part of our century, demonstrated, *in fact*, the principles in electro-magnetism upon which the electro-magnetic telegraph and the telephone were constructed. Still, it was many years before these principles were applied in the development of the apparatus that annihilates time and space, and brings mind and mind together from the uttermost ends of the earth.

Facts are established, principles are discovered, but their value and application is often delayed awaiting other discoveries. This same Michael Faraday, about the same time, discovered and pointed out the relative *magnetic* and *dia-magnetic intensity* of some of the gases and their binary compounds.

It serves my purpose to give you just here his figures. They are—

Magnetic Intensity.		*Dia-Magnetic Intensity.*	
Oxygen	17.5 to 3.4	Hydrogen	0.1
Air	3.4	Ammonia (NH_3)	0.5
Olefiant gas (C_2O_2)	0.6	Cyanogen (NC_2)	0.9
Nitrogen	0.3		

It will be readily seen by this table that oxygen is an electric or magnetic bearer, varying in its tension from 17.5 to 3.4.

Digitized by Google

Researches have demonstrated that oxygen, among the gases, takes the place or bears the same relation to electro-magnetism that iron does among the metals—capable of being charged with electricity, varying in intensity, as given above.

The well-known experiment of passing a current of electricity through a volume of oxygen, converting it into a condition of nascent oxygen or ozone—a state of the highest intensity, under the pressure of one atmosphere -is too familiar to you all to require further allusion. But what is the effect of the condensation, or the rarefaction, of oxygen, or of the atmospheric air? We estimate the amount of oxygen of the air—in round numbers, for the sake of convenience—at one-fifth, 20 per cent., with a pressure of 15 pounds to the square inch. A very little calculation will exhibit the effect upon its tension under an increase or decrease of pressure.

Thus, for example, we have—

	Per cent. of Oxygen.	*Pressure.*		*Tension.*
One atmosphere	20	1	=	20
Two atmospheres	20	2	=	40
Three "	20	3	=	60
Four "	20	4	=	80
Five "	20	5	=	100
Oxygen	100	1	=	100
One-half atmosphere	20	½	=	10
One-fifth "	20	⅕	=	4

(*Vide* Draper's "Medical Physics.")

From this table it will be readily perceived that the tension of the atmosphere is dependent upon two factors: *1st. The amount of pressure. 2d. The amount of oxygen.*

With these premises, I shall pass to the question:—

The Function of the Red Blood Corpuscles.—In treating this subject it is desired to present more particularly some thoughts upon the manner in which the red blood corpuscles seem to be the floating medium for transporting the electric or *solar force* that is necessary for supplying *nerve force* in sustaining and maintaining *vital action.*

These thoughts have resulted from experimental investigation, close observation of the results of experiments in the hands of others, and the correlation of acknowledged facts and established principles, and should they not be considered orthodox, I beg you, for the cause's sake, to bear with me patiently and give me the benefit of your better lights in this search after truth.

I will not insult your intelligence by repeating here what is well known to you all, of the action and function of the red blood corpuscles; but, as a basis and a starting point, quote from a text-book acknowledged in this country and Europe.

Kirke says, "The important use of the red blood corpuscles is in relation to the absorption of oxygen in the lungs and its conveyance to the tissues. How far the red corpuscles are actually concerned in the nutrition of the tissues is quite unknown."

That the red blood corpuscles are the carriers of oxygen from the atmospheric air through the lungs to the tissues, is a fact admitted by all, but when we consider or attempt to consider their further function, it is essential that we extend our investigation—that we go back and investigate that oxygen; what is it, how does it support life, sustain the vital forces and bring about the phenomena of the nervous system resulting from its introduction into the lungs?

In anticipation of the question, What is oxygen? I have given you in the outset of this paper, from the researches and discoveries of England's great electrician and chemist, that particular magnetic condition of it with which we have to deal in the discussion of the subject under consideration. It has been shown that it is magnetic, bearing a tension varying from 17.5 to 3.4 under a pressure of one atmosphere, and, as demonstrated of atmospheric air, with the increase or the decrease of the density there

Digitized by Google

was, in a corresponding ratio, as under Marriott's law, an increase or a decrease of this *magnetic tension.*

Floating out through our southern balmy air at the evening twilight, in their season, may be seen sailing, like meteoric showers, lighting up the way, multitudes of fireflies —*lightning bugs*, as they are called. These little insects interested me in my childhood, as they interest all children in the chase after them, and they interest me now, for they serve me a purpose in illustration. Placed under the lens, it is found that, with each inhalation, their almost transparent tissues become luminous. The light is, when first emitted, quite bright and not unlike an electric aura, and, as it fades a little, there is seen leaping through the deeper tissues little electric-spark-like lights, as it fades away after these intermittent illuminations. I have often fancied that the arterialization, the oxygenization, the electrification, if you please, of the blood must be like this—an electrification of a free corpuscular tissue on the one hand, and of transparent insectivorous connected tissues on the other. I am not prepared to say that such is not the case; be this as it may, it will serve my purpose for illustration, and I will beg that you bear it in mind as such while we proceed.

One function of the red blood corpuscle is recognized to be, to bear oxygen from the lungs to the tissues, and Draper says, "Although oxygen is necessary for the maintenance of life, it must be used in the diluted condition in which we find it in the air. When breathed in the unmixed state, *it stimulates the nervous system strongly, and finally causes death.* [Italics my own.] The experiments of Mr. Broughton showed that rabbits died in from six to twelve hours when kept in pure oxygen. Despite the noxious action of pure oxygen, animals will live in an atmosphere of that gas three times as long as they will live in an equal volume of atmospheric air." (Draper's "Med. Physics," p. 158.) It is well known to you that a dog poisoned by the inhalation of condensed oxygen becomes as rigid as if carved from wood. It is equally well known to you that the effects of this substance on the human system assimilates very closely the convulsions of tetanus and epilepsy, as well as those of the toxic action of strychnia and of phenic acid. As pointed out above, with the increase of pressure on or the increase of oxygen in the atmosphere, we have *increase of magnetic tension.*

There may be found a very striking and impressive illustration of this, and its effects on combustion as well as on the human system, in the results to the workmen who worked in the caissons sunk for laying the foundations for the railroad bridge across the Mississippi at St. Louis, a few years since.

As the workmen descended with their work, the pneumatic pressure in the caissons increased apace, until they were working under the pressure of two, and then on to three atmospheres. Then the *tension* was increased from twenty—the normal—up to *sixty.*

What was the result of this increase of pressure and of tension? The candles used by the workmen burned out with an increasing rapidity until, at last, under the higher pressure, they were consumed in one-fourth the time. On the men, as they descended deeper and deeper, with the continually increasing pressure and tension, the effects were most deleterious—a train of nervous symptoms were developed, closely resembling cerebro-spinal meningitis, from which many of them died in a few minutes or a few hours. Here the combustion in the candles increased fourfold—was there a corresponding increase in the *physiological* (or, if you please, *pathological*) *combustion in the systems of the workmen?* Not at all; their temperatures did not indicate it—it was from a train of symptoms indicating disturbances in the nervous system that their death arose. *Apropos*, M. Bert says, "When I for the first time saw a sparrow under the influence of condensed oxygen, and suffering violent convulsions, I thought that the intra-organic oxidation must produce exaggeration in the temperature. Imagine my surprise when the thermometer showed exactly the opposite result." (Quoted by Draper, "Med. Physics," p. 205.)

Digitized by Google

I will not further multiply these illustrations. Sufficient has been presented here to direct your minds to this one point, and that is, that when the blood corpuscles bear the oxygen of the atmospheric air from the lungs to the systemic capillaries, and thence to the tissues, they bear that oxygen *charged with that magnetic force* first pointed out by Faraday, and that that charge is greater or less, according to the per cent. pressure or magnetic tension of the oxygen inhaled. Not only this, but when that oxygen is given off in exchange in the systemic capillary system, then and there is discharged at the same time *that magnetic or electric* force which is taken up by the end organs or peripheral terminals of the nerves, and becomes to the nerve system its source of supply—a feeder force, so to speak—to be modified and stored in the nerve centres—the storehouses of the nervous system—for future use. Furthermore, when the tension of the atmosphere is increased by any of the causes alluded to above, these nerve centres receive an over-charge that may lead to the pathological conditions and a train of nervous symptoms that prove more or less speedily fatal. What should this teach us as to the function of the red blood corpuscles?

If the surcharge from increased tension of the atmospheric air or oxygen breathed produces the surcharge in the nerve centres, inducing these pathological effects, must we not, on the same line, and by the same process of reasoning, arrive at the conclusion that, *at a normal tension*, the same changes and charges take place, giving a physiological or *normal charge* to these nerve centres?

Physiologists have taught that a nerve centre is a collection of ganglionic cells, capable of *originating* nerve force, and of receiving and of transmitting impressions. I am frank to say that I have never been satisfied with that word—*originating*—in that connection.

So far I have alluded more particularly to the effects of increased pressure and tension. What is the effect of rarefaction and reduction of tension? The answer to this may be summed up into just this—*it is equivalent to a reduction of the red blood corpuscles.* Either of these conditions lead to *a reduction of nerve force.* This is illustrated on a large scale by the rarefied air of the great Mexican plateau, where the diminished tension brings an anæmic and neurasthenic type to all forms of disease, as their physicians will testify.

There are many symptoms of *mal des montagnes* and of *balloon sickness* in the disturbances in digestion, respiration, circulation and locomotion, resulting from rarefaction and loss of tension, that would be exceedingly interesting to review and apply in support of the argument, did time permit. There are some points in connection with the study of *malaria* that may serve to illustrate this idea, and with your permission I will strive to make them in as brief a manner as possible. In doing this, I must quote again from Draper. He says (p. 134): "When it is deemed proper to drain a marsh, arrangements should be made to carry the operation out as late as possible in the fall of the year, and still leave sufficient time to give the soil an upturning before it freezes. Thus, the winter air, charged, as it is, with ozone, may have an opportunity to come in contact with the material from which malarial emanations arise and exert its destructive action upon it. In no case should a marsh be either drained or its bed exposed to the intense heat of the summer sun."

Now this quotation will serve to direct your mind to a point I desire to make plain. This question of *malaria* is one of no little importance to the people of this country. Dr. Draper gives us good advice on the question of drainage, etc., but let us follow up the idea. If the marsh is drained in midsummer, a process of vegetable decomposition is set up, that is carried on at the expense of the oxygen, and *this magnetic force—this vital force of the atmosphere*—within a given space. In that area a contest arises; on the one hand is a mass of decomposing vegetable matter, demanding this oxygen of the atmosphere to oxidize, and the magnetic force to carry on the work; on the other

Digitized by Google

hand is the man whose blood tissue is demanding the oxygen to oxygenize its corpuscles, and his nerve system demanding the magnetic force to keep up its supply of nerve force; and so the contest continues, resulting in the devitalization of the air, by reason of the superior affinity of the decomposing vegetable matter for the oxygen. The blood corpuscles reduce in vitality, the nerve forces are depressed, and, as a result, you have the effects generally attributed to malaria. The vital force of the atmosphere has been reduced apace with the reduction of the magnetic intensity of its oxygen, as is illustrated in the figures of reduction from 17.5 to 3.4, according to the demand made upon it. Why leave the drainage for the winter? For two reasons: One, the decomposition, with the reduced temperature, would go on with less rapidity; the other because the atmosphere in winter contained more ozone, was of higher magnetic tension, and could furnish the *oxygen and magnetic tension* in sufficient quantity to carry on the slow process of combustion in the vegetable matter and supply equally the demands for the chemico-vital combustion in the man.

This will account to you for the broken-down blood tissue, the neurasthenic condition of the nervous system, and the vitiated condition of functional action in the secreting glands and membranes in the malarial regions of this and other countries.

It may not be out of place to contrast, just here, this state of things with that found in midwinter, when the eastern winds come to us charged with ozone in a condition of the highest magnetic tension. Mark the difference in the effects upon the system. The red blood courses its way fully charged, bearing a tension that, within bounds, buoys up the system, giving strength of nerve and muscle—that of perfect health. But when the surcharge comes we find the nervous system exhibiting evidences of tension often terminating in formidable disturbances, as influenzas, cerebro-spinal meningitis, rheumatism, neuralgias, and other diseases *neurasthenic* in character.

I may allude *en passant* to the experiments going to show that the power of muscular contraction is dependent upon and regulated by the condition and the quantity of arterial blood supplied to the muscular tissue. There are some questions pertaining to the correlation of physical and vital forces, presented by those who delight to make experiments and observations upon such subjects, that, as I conceive, may be more satisfactorily solved by the adoption of these views regarding the functional action of the red blood corpuscles than by any other line of investigation it has as yet been my good fortune to strike, with all the researches that years and their opportunities have enabled me to make.

And now, in conclusion, allow me to say that in presenting this paper, I do so being myself in search of truth. In tracing, as I do, the origin of nerve force, or rather, I should say, the means for its supply and perpetuation, back through the red blood corpuscles to the electricity of the atmospheric air and, for that matter, to the solar rays, if you please, I do so with becoming reverence; for in doing this there is shown not only the relation, but that the same power, the same force, that propels the corpuscle regulates the solar centre and system from which the force may emanate—all dependent upon the fiat of the power that spoke it into being—and brings us into closer relation with that power whose many mysteries and mysterious ways we are not yet permitted to know. In the great beyond they may not be mysteries. And now, in conclusion, I will say "*Lex scripta manet.*" And if what I have written shall meet your approval, and you shall in the end, after thorough investigation, adopt these ideas, finding them correct, I will have accomplished my object and be glad. But should you see fit to differ with me in these views, I will only say that I will be most happy—while prosecuting this search after truth, in which we are all engaged—I will be most happy to have you point me to the error of my way, and show me the better.

Digitized by Google

THE PHYSIOLOGY OF ELEMENTARY FIBRES.

DE LA PHYSIOLOGIE DES FIBRES ÉLÉMENTAIRES.

ÜBER DIE PHYSIOLOGIE DER ELEMENTAREN FASERN.

BY J. H. WYTHE, M.D., F.R.M.S.,

Professor of Histology in Cooper Medical College, San Francisco, California.

In the physiology of a century ago fibres played a very important part. Thus, Haller, in his "First Lines of Physiology," declares that the solid parts of the body consist of fibres, arranged in parallel lines or in layers, and an unorganized gluten from which the fibres were originally formed. Others regarded the tissues as made up of granules, globules, fibres and membranes. In 1835 Dujardin discovered that the lower animals were characterized by the possession of a contractile substance, which he termed "sarcode." Max Schultze afterward, in 1861, showed that this substance formed not only the bodies of the Infusoria, but also the contents of animal cells. He called it Protoplasm. For this substance, in its living state, Dr. Beale proposed the word Bioplasm.

In 1838 Schleiden and Schwann taught the origin of all tissues in cells, and from that time the cell doctrine has been considered to be the basis of minute anatomy. Those authors considered the origin of the cells to be from a fluid plasma, or blastema, but Virchow, in 1852, discovered that the cells have primary vitality, and that each cell originated from a preceding one. Hence the aphorism, "*Omnis cellula e cellula.*"

The cell, as described by these investigators, was a vesicle composed of a transparent membrane, within which was a fluid or semi-fluid substance containing a more solid body, which was called the nucleus. Within the nucleus, the nucleolus, a still smaller body, was sometimes found. Max Schultze showed that an external membrane was not an essential part of the cell, but that the individual elements of organization are masses of a living, jelly-like substance. Brücke (1861), and afterward Stricker (in 1868), proved that the nucleus was often absent, and that living matter in the organism exists as a pellucid jelly, in which nuclei and granules were secondary products.

The great improvement made in the optical part of microscopes since that time, as well as in microscopic technology, and especially the methods of staining tissues with carmine, logwood, aniline dyes, nitrate of silver and chloride of gold, have led to considerable advancement in histology. The cell, which was formerly considered to be a mass of structureless jelly, is now known to be a network of fine fibres of semi-fluid, living matter, having at the junction of the fibres nodules, or large masses of the same matter, which give it a granular appearance. In the meshes of this network, fluid nutriment and effete matters may freely circulate. A similar living network is found in many tissues and organs which are generally described as of more simple composition. Heitzman, Elsberg, and others claim that this network is continuous through all the tissues and organs of the body.

In the proliferation of nucleated cells it has been shown by Flemming, Klein and others that the fibrils of the nucleus show complicated movements and changes previous to the division of the cell itself.

This general outline of the progress of research is sufficient to suggest that the cell doctrine in physiology, based upon former ideas of cell structure, requires considerable modification. This will appear more evidently when we consider the number and variety of tissues in which elementary living matter exhibits itself in the fibrillar or reticular form.

Digitized by Google

However important to the animal organism may be the functions of separate masses, or particles, of living matter—the so-called cells—microscopical observation reveals to us that the tendency of such particles is toward the formation of fibres, and to the weaving together of such fibres in various ways, according to the differentiation of function. In every tissue a fibrous arrangement is the rule, and not the exception, and as we increase the magnifying power or the delicacy of our methods of preparation, this morphological fact becomes more evident. It must necessarily have a physiological signification. The production of fibre is the highest morphological expression of living matter.

Fibrillar and reticular structure may be found in red and white blood cells; in epithelial cells and their nuclei; in endothelial cells; in every form of connective tissue; in voluntary and involuntary muscle, and in nerve axes and ganglia. In Klein's "Atlas of Histology," Plate xv shows cells of unstriped muscle, with a central bundle of fibrils, or contractile part, connected with an intra-nuclear network, and Melland (*Quarterly Journal of Microscopical Science*, July, 1885) has demonstrated, by his method of gold staining, an intra-cellular network in striped muscle fibre—a series of reticulated discs, connected by longitudinal fibrils. Luys says ("The Brain and its Functions," 1882), of the cells of nerve ganglia, "that this substance, which we call the protoplasm of the cell, is formed by a true tissue organized in a special manner; that this tissue, consisting of very delicate fibrillæ interlaced like the wicker-work of an ozier basket, has a tendency to agglomerate toward the nucleus of the cell, which thus becomes a true point of concentration; that the nucleus itself is not homogeneous; that it is endowed with a special structure, radiated in appearance, and that, lastly, the nucleolus, considered as the final expression of the unity of the nerve cell, is, in its turn, divisible into secondary filaments." These references suffice to show the general prevalence of fibrous reticulation and its physiological importance.

Bioplasm, or living protoplasm, which is the basis of living fibre, is a semi-fluid, homogeneous substance, having peculiar motions and properties. In addition to its changes of shape, and movement from place to place, seen by the microscope in every amœboid particle, there are inherent motions of its molecules witnessed by means of the flow of accidental granules, or vacuoles, which are carried along by its current. Thus, in the hair cells of vallisneria and other plants, the streaming movement may be seen in thread-like masses circulating around the cell cavity. What is known as contraction or expansion of living fibre, is simply the relative distance of its molecules from each other, and although so elementary a function of living matter, it differs so much from ordinary vibration, or contraction in inorganic substances, that no proper comparison can be instituted between them. The molecular movements of living fibre vary according to the character of the reticulation. Thus, in the axis cylinder of nerves there is room only for a simple oscillatory motion, while in the reticular fibres of the sense organs, and in the reticulation of living matter in the ganglia, the movements are more complex and varied. We can readily conceive that a movement excited in the peripheral network may be transmitted by the nerve axis to the ganglionic reticulum, and thence through other axes to the muscular fibre, producing what is called reflex nervous action. The nodular reticulum of the so-called discs of striated muscle also (shown by Melland), when excited to vital contraction by the molecular changes in nervous fibres, will, of necessity, shorten the longitudinal fibres between the discs. We may thus explain the mode of action in voluntary muscle. In a similar way, Klein's description of involuntary muscle fibre shows how its action may be readily conceived. The present status of histology thus tends to remove from the sphere of mystery what has been known as nervous influence and muscular action, and resolves them into simple physical contractions of an elementary fibre in which the power of life is incarnated. This elemental

Digitized by Google

bioplasmic fibre is so generally distributed in the various tissues as to suggest the thought that its properties constitute the true physiological unit of organic life. Its power of vital contractility will account for nervous and muscular motions, and it needs only to consider it endowed with selective power in order to understand the process of secretion also. It may be that in the physiology of the future we shall no longer speak of a mystical nervous influence, or of muscular contraction, but acknowledge the power of living matter to do that which the non-living can never do.

Digitized by Google

FOURTH DAY.

Dr. W. Hutson Ford, of St. Louis, Mo., presented a paper—

ON THE NORMAL ACCOMPLISHMENT OF THE ALCOHOLIC FERMENTATION OF HEPATIC SUGAR IN THE ECONOMY.

SUR L'ACCOMPLISSEMENT NORMAL DE LA FERMENTATION ALCOOLIQUE DU SUCRE HÉPATIQUE DANS L'ORGANISME.

ÜBER DIE NORMALE AUSBILDUNG DER ALKOHOLISCHEN GÄHRUNG DES LEBERZUCKERS IM ORGANISMUS.

More than a hundred years ago (1764) Willis announced that the urine of diabetes was saccharine. This malady, not yet distinguished from simple polyuria, was known to Celsus, Aretæus and Galen. In confirmation of the statement of Willis, the existence of sugar in the urine in grave and lethal forms of diabetes was demonstrated by Pool and Dobson, and three years later by Cowley. After this (1778) much attention was devoted to the subject, and valuable contributions relating to it were published by Rollo, Prout, Bell, Nicolas, Guedeville, Dupuytren and Thénard, and Contour (1844). From 1841 to 1846 M. Bouchardat contributed several papers, in which he endeavored to enlighten the pathology of the affection by an ingenious theory of its origin. Bouchardat maintained that the sugar in the urine is derived exclusively from the starchy matters of the food, and is formed by a pathological process in the stomach, too rapidly for abstraction in the blood, while normally the saccharification of starch is effected throughout the length of the small intestine, so that the resultant glucose is absorbed gradually into the blood and commensurately destroyed in that fluid.

Bouchardat's theory fails to take into account a well-known and characteristic feature of diabetes, viz., that when the disease is severe or advanced, the quantity of sugar voided is considerably greater than can be accounted for by the starch and other carbohydrates ingested. This important consideration, plainly showing that Bouchardat had not grasped all the facts, perhaps not even the fundamental ones, furnished the earliest motive for the masterly investigations of Claude Bernard, which date from 1843.

It was not, however, until 1848 that Bernard published his first results, viz., that,

1. Sugar may exist in the organism independently of alimentation, and that,
2. The liver is an organ which produces sugar.

In 1853 a formal monograph upon the subject appeared, entitled, "Nouvelle fonction du foie," and a volume of lectures delivered at the College of France in 1854 and 1855, was published in the latter year. Some years later (1857) Bernard announced the isolation of the glycogenic matter.

The cardinal facts established by Bernard, were as follows :—

1. The blood of the hepatic veins always contains sugar.
2. The blood of the vena portæ of a fasting animal, or of one fed upon a purely

Digitized by Google

animal diet, never contains sugar, except as due to accidental reflux from the liver after opening the abdominal cavity, or to muscular movements of the animal, etc.

3. Sugar always exists normally in the tissue of the liver, and only disappears a short time before death by inanition, or in certain morbid states.

4. The systemic blood contains no sugar during abstinence, but does so during digestion.

5. The blood of the liver, hepatic veins, ascending cava and right ventricle is always normally saccharine.

6. Sugar disappears in the lungs in fasting animals; and in animals in digestion, almost entirely in those organs, and eventually, in the blood mass; no sugar being excreted in the normal condition.

7. The highest temperature of the animal body exists at the point of entrance of the hepatic veins into the ascending cava.

8. The temperature of the left ventricle is slightly lower than that of the right.

These statements were shortly (1849–1852) verified by Vanden-Bröck, Lehmann, Frerichs, Bawmert, Gibbe, Mitchell, and afterward by many other experimentalists, up to the year 1858. Previously to 1853, Bernard had greatly extended his studies. He investigated the cardinal facts of the glycogenetic theory throughout the animal kingdom, elaborating his views and recording the results of his experiments in the monograph above mentioned, which remains to us as an almost perfect model of physiological research. Some years afterward (1857) he published his paper on the isolation of glycogen, although the principal facts relating to this substance had been exposed in his lectures delivered in 1854 and 1855, already mentioned.

Glycogen is closely allied to starch and dextrine; it is a normal constituent of the liver tissue, being found in all animals at all times. During the earlier periods of gestation it exists in the muscles and lung tissue, but not at first in the tissue of the liver. When it appears in this organ, its quantity declines in other parts of the organism, from which it eventually disappears. It accumulates in the liver in especial abundance after starchy and saccharine food. In experiments upon dogs, Pavy found that a diet of vegetable food augmented very greatly the quantity of glycogen present in the liver, also notably increasing the weight of the liver itself. In the present state of our knowledge, we are authorized to believe that glycogen represents the entire quantity of carbohydrates ingested as food in herbivora and in omnivora, and likewise, all that part of proteinoid food which is not transformed into urea, and the biliary acids, with some unimportant exceptions, for glycogen is formed as well in carnivora as in the fasting state of other animals. The formula of glycogen is $C_6 H_{10} O_5$, being apparently formed from the dehydration of starch. The liver contains from 7 to 17 per cent. of this substance, according to the nature of the diet. It is continually transformed in the liver tissue into hepatic sugar, by the action of the liver cells themselves most probably, subject to the influences of innervation; nevertheless, this transformation continues after exsection of the liver and the drainage of the liver tissue of its blood, so that it must be attributed to inherent properties of the liver stroma, essentially independent of the circulation and nervous system, though greatly modifiable by these influences, as Bernard has demonstrated. The liver must thus be regarded as a depôt for carbohydrates, whence they are constantly poured into the circulation through the hepatic veins, at an uninterrupted, nearly uniform rate.

Prior to 1858, Bernard published his quantitative determinations of hepatic sugar in the liver in various communications; he had executed them with extraordinary diligence and scope for a great variety of animals and physiological conditions. He affirmed that the liver contains, on an average, one gramme and a half to two grammes of sugar to one hundred grammes of its tissue, in mammals and birds, and in man, from analyses of the livers of two criminals, 1 gr. 966 of sugar to 100 of liver tissue. Quan-

Digitized by Google

titative statements of this kind, undoubtedly accurate for the conditions under which the facts supporting them were determined, were continually made by Bernard, and are to be met with in all his writings prior to 1877. His assertions were corroborated by experimenters everywhere who operated as they had seen him do, by leisurely slicing off a portion of a liver, extirpated without haste, grinding it in a mortar with animal charcoal, boiling and filtering. A clear extract so made always contained sugar in the proportions asserted by Bernard. The glycogenetic theory, as Bernard had formulated it, seemed in all its aspects as firmly established as the laws of gravitation itself.

In the midst of this security (1858), Pavy published certain experiments, to all appearance, wholly subverting it. Pavy claimed to have demonstrated that the liver contains *no sugar during life*, this substance making its appearance *after death only*, by a change of the glycogen which does not occur normally or during life, and consequently, that no glycogenetic function, properly so-called, exists as an attribute of the liver. He operated by injecting into the liver, through the portal vein, a solution of caustic potassa, and by rapidly slicing off bits of the liver during life, and casting them immediately into a freezing mixture, in both ways designing to prevent a formation of sugar subsequent to the operation, the argument being that if, under these circumstances, sugar was not to be found, it could not have been present during life. The presence of sugar under these conditions could not be demonstrated, or only in what *was said to be* an insignificant amount, and even this was claimed by Pavy to be of post-morten formation. These experiments, with some modifications, were repeated by Meissner, Ritter, Schiff and Eulenberg, who accepted Pavy's explanation of the phenomena. They regarded the *traces* of sugar sometimes found as due to post-mortem or abnormal influences. In 1860, Harley showed in four experiments that when the liver tissue was cast into a freezing mixture, and after freezing, sliced into boiling acidulated water, sugar was *always determinable*, though in small quantity.

Nevertheless, Pavy's views gained a wide foothold; it was generally assumed that the so-called glycogenetic function of the liver had no real existence, and that Bernard's conceptions of the sequences of chemical transformation occurring in the liver were essentially at fault. The presence of sugar in the excreted liver was regarded as a post-mortem phenomenon.

While, during a period of eight years, the physiological world thus went astray, those who had witnessed Bernard's cardinal experiments of the absence of sugar from the portal blood of fasting or carnivorous animals and its universal presence in the blood of the hepatic veins, as well as in the right side of the heart, as I myself, with Hardy and other pupils of Bernard, had fortunately been able to do, remained unshaken in their convictions, although it was plain that the subject needed at least quantitative revision.

Appreciating this necessity, Flint, of New York, in 1868, endeavored to reconcile these conflicting views. He operated by cutting out bits of the liver in dogs during life, slicing them into boiling water and examining them by Fehling's test; the time occupied was very short, viz.: from 10 to 28 seconds only. Flint was unable to demonstrate the presence of sugar in such bits of liver, but he detected sugar in the hepatic venous blood within one minute after one of his exsections, concluding in favor of a normal hepatic glycogenesis; the sugar being derived from glycogenic matter during life and rapidly washed out of the liver by the blood stream.

In 1870, Lusk obtained results opposed to those of Pavy and others, corroborating Bernard's assertions. He found, by repeating Bernard's aspiration of the right ventricle, that sugar in very definite quantity exists normally in the blood passing through it.

In 1871 Dalton reviewed previous experiments of this kind after a carefully planned method. He determined, in a series of twenty experiments, that the liver contains

Digitized by Google

glucose in quantities varying from 1.510 to 4.375 parts to 1000 parts of liver, the time occupied in extraction of the liver morsel, in pulping it and in heating it to 212°, varying from 3 to 13 seconds. The dosage found by Dalton, as above given, was about one-tenth of that determined by Bernard, and *assumed by him* to have been present during life. Dalton also took occasion to *measure* the post-mortem glycogenesis of the exsected liver tissue at ordinary temperatures. He found that the proportion of sugar determinable in the liver tissue after four or five seconds (mean of three experiments) to 1000 of tissue was 2.778; after fifteen minutes, more than twice as much, viz.: 6.792; and after one hour, 11.074 (mean of two experiments), or very nearly four times as much. He also determined that sugar did not exist in the spleen, nor presumably in any other organ, so that the sugar found in the liver could not have been derived from the arterial blood sent to that organ, the spleen being examined concurrently with the liver, in three experiments, ten minutes after the liver tissue was tested.

These results essentially reëstablished the credit of the glycogenic function, at least with those who had temporarily failed to appreciate the rigorous cogency of Bernard's cardinal experiments, and Dalton formally concluded that sugar exists in the liver at the earliest period at which it is practicable to examine the organ; that the quantity existing at this time is at least two and a half parts in a thousand of liver tissue, and that the sugar found does not belong to the arterial blood, but is normally present in the hepatic tissue. Bernard, finally, in 1877, published a paper in which this subject was reconsidered, correcting his earlier dosages of sugar, which had been altogether too high, on account of his failure to bear in mind a fact he had himself discovered and frequently spoken of, viz.: that the percentage of sugar notably increases in the liver after exsection. All of Bernard's earlier dosages, as well as the comparative conclusions derived from them, to a very considerable extent at least, are thus necessarily invalidated, while, nevertheless, the glycogenetic function has become more exactly defined and thoroughly substantiated. Sugar is perpetually formed in the liver and continuously evected in the blood stream. Had Bernard dosed his morsels of liver tissue after having first thrown the entire organ or the parts of it operated upon into boiling water immediately after exsection, these controversies could not have occurred. He was himself the originator of the method of arresting the progressive formation of sugar in the liver after exsection by plunging it into boiling water. I quote from his "Leçons," 1854 and 1855, p. 366. Speaking of the increase of sugar in a portion of the uncooked liver of a rabbit killed after section of the spinal cord above the brachial enlargement, Bernard observes: "It may thus be seen, that by exposing liver tissue pulped with water to a temperature of 40° to 50° C., the return of saccharine matter is singularly hastened," proceeding to explain how some sugar might thus be formed, even while making a decoction, before the liquid is heated to 212° F. "It would," says he, "be more appropriate to arrest this process immediately by plunging the liver into boiling water, so that it might not pass through an intermediate temperature capable of determining the production of a certain quantity of sugar which would not otherwise have become manifest."

Our knowledge of the glycogenetic function of the liver may be fairly summed up, in brief, as follows:—

1. The incursion of dextrinoid, amylaceous and saccharine matters, from the digestive tract, effects a prompt increase of glycogen in the liver. Under these circumstances a freshly made decoction of the liver is opalescent. Even in fasting carnivora, glycogen exists in the healthy liver, and must therefore be formed from proteinoid matters, furnished by the blood.

2. Glycogen is more nearly allied to dextrine than to starch, and is converted into sugar by all agencies capable of converting dextrine into glucose, but with a facility far

Digitized by Google

greater than is observable for dextrine of vegetable origin. In this facile convertibility into an ulterior product, glycogen resembles hepatic sugar itself.

3. Some zymogenous agency proper to the liver substance may be invoked as the cause of this transformation, but even systemic blood is able to saccharify glycogen at the temperature of the body. This conversion, normally in continual progress, so that the blood escaping by the hepatic veins always contains a notable and very constant proportion of sugar, takes place in the liver at the temperature of the body with great ease and rapidity, but is speedily checked and eventually arrested by a decline in temperature.

4. The glucose thus formed, the resultant product of all the amylaceous and saccharine elements of the food, as well as all of the proteinoids, excepting such portions of these last as are represented by urea, the biliary acids and some bodies of secondary importance, is unceasingly washed away by the blood, so that during life the liver very probably never contains more than one or two parts of sugar in a thousand of its own substance. If the animal is in digestion, the quantity of sugar thus evected by the blood is considerably greater than during the fasting state, and much of it passes beyond the lungs into the general circulation. This appears to be habitually the case with herbivorous animals, which are more or less in digestion at all times; but in fasting carnivora, most of the sugar is destroyed in the lungs, though a certain small portion may be regarded as normally present in the systemic blood, having escaped the peculiar decompositive influences of the pneumocardiac transit. Arterial blood contains more glucose than venous, so that glucose would seem to disappear in the systemic capillaries, although mainly decomposed while passing through the lungs; nor does it normally escape by any of the secretions.

As this sugar represents by far the greater weight of the colloidal elements of the food, its ultimate destruction, its physiological significance, the mechanism by which it disappears, and the phenomena attendant upon the disappearance, have been appreciated as fundamentally important problems since Bernard first established its invariable presence in the liver and blood, and its conversion within the economy into other products. Among the non-nitrogenous matters of the food, starch, dextrin, pectin, cane and fruit sugar are undoubtedly metamorphosed into glycogen and through this into hepatic sugar. Gelatine also subserves the formation of glycogen, probably giving rise to crystalloidal bodies containing nitrogen at the same time; it thus ranks as intermediate between the carbohydrates and albuminoids, since it cannot administer to the formation of tissue. Fibrinogen disappears as such, at least in the liver, and albumen diminishes in the blood passing through the organ, so that we are compelled to admit that among the various acts of the liver one function exists of primary importance, by which the proteinoids of the economy, most probably after subserving the purpose of tissual nutrition, or when actually exceeding the immediate wants of the system or its power of storing up nutritive matters, are broken up into the biliary acids, uric acid and urea, and *glycogen*. Thus we find that hepatic sugar and the acids of the bile, with urea, together account for all the food absorbed by animals. A recognition of the numerous physiological conditions whose concurrent action is indispensable to the steady outflow of hepatic glucose into the blood, of the existence of the glycogenic function in all animals hitherto examined, of the fact that this function is formally elaborated during fœtal life in obvious preparation for an independent existence, compels us to regard the disappearance of sugar in the organism as a phenomenon replete with significance, underlying, as it obviously does, the process of alimentation, and necessarily, therefore, all the functions of animal existence. For indeed, this hepatic sugar constitutes a temporary halting place in the transformation of the colloids of the food into crystalloidal matters, a most significant change in which the forces of animal life originate.

Digitized by Google

The physiological importance of hepatic glucose cannot, therefore, be over-estimated; it must be regarded as the eventual hydrocarbonous resultant of the digestive and alimentary processes of hepatic elaboration, and as designedly prepared or harmoniously utilized for purposes directly subservient to life itself. Is this importance to be recognized in the conditions attendant upon the destruction of glucose in the economy or surrounding its origin and elaboration? Are the purposes for which animals consume starch, sugar, vegetables, and the greater weight of the flesh they devour, subserved by the act of hepatic glycogenesis, or by the fact of the conversion of the glucose so carefully elaborated into further and ultimate products? Is the destruction of sugar in the economy merely a sidelong excretory process, by means of which it may be speedily eliminated, and easily separated from the blood, or must we regard it as essentially an active one, fundamentally indispensable, and administering to organic necessities upon which all cell activity is inherently dependent?

Soon after oxygen was discovered by Scheele and Priestley, its relations to the act of respiration were investigated by Lavoisier, in some of the most admirable experiments recorded in science. Priestley also pursued his studies in the same direction, with similar results, so that physiologists announced before long that a hydrocarbon was consumed in the organism, in the lungs, or, perhaps, elsewhere, and that such a combustion, though slow and occurring at low temperatures, must nevertheless evolve as much heat as if attended by visible inflammation. Liebig finally, by his ingenious methods, demonstrated that this hydrocarbon must be derived from the food.

Hepatic glucose is a typical hydrocarbon, directly combustible, easily broken up by dry distillation; more rapidly fermentescible than any known sugar; why, therefore, should it not constitute the source of the larger share of animal heat by its slow combustion within the economy? After the publication of Bernard's discoveries, it was affirmed that the long-sought hydrocarbon was found at last. Mialhe framed his theory of the direct destruction of hepatic glucose by the alkalinity of the blood in explanation of the phenomena of diabetes, and Alvaro Reynoso published a series of experiments, proving, as he affirmed, that this sugar is directly oxidized in the lungs.

Bernard, however, rejected these theories. He affirmed persistently that hepatic sugar was not "consumed" in the lungs, directly or indirectly; that it was not a prime source of animal heat by oxidation in the blood-stream, but was changed in the lungs, or at any rate in the blood, into new matters, by *whose formation heat was not evolved.* He says "It has been supposed by certain theories, that sugar is destroyed for the production of the heat requisite to maintain the proper animal temperature; I think that sugar has other uses to fulfill, of a nature entirely different, and of a much greater importance." He regarded the formation of sugar as the significant epoch of its existence, supposing that certain phenomena of what he termed "animal germination" were accomplished in the liver at the moment when the animal matter which furnishes sugar is decomposed for the production of this substance. "Should we consider the chemistry of the adult organism only," he observes, "we must place the principal part which sugar plays, in its destruction, as has already been imagined by some. On the other hand, by the theory of development, we are led to conclude that the most important objects for which this substance is present are to be recognized in phenomena which occur, not when it is destroyed in the blood, but when it originates in the liver." "It is at the moment of its formation in this organ, that the organic elements are likewise created which are destined ulteriorly to complete their evolution for the renovation of the animal tissues." (*Loc. cit. p. 256.*)

Bernard never relinquished these views, maintaining that sugar is formed in the liver or in fœtal tissues in order that "animal germs" may be simultaneously evolved and developed. He attached little or no importance to the disappearance of sugar in the organism, admitting, however, that this disappearance was by a fermentative change

Digitized by Google

of some kind, and inclining, though not very strongly, toward the hypothesis according to which glucose is destroyed by conversion into lactic acid. He peremptorily dismissed all hypotheses of direct combustion or oxidation of glucose in the lungs or elsewhere, like those of Mialhe or Reynoso, "We have seen" says he, "that oxidation does not explain the phenomena; is it so, however, for a fermentation? The theory of oxidation as an explanation of the disappearance of sugar, from considerations already urged (loc. cit. p. 232) as well as from new facts which offer themselves to strengthen this affirmation, is," he says, "more than insufficient." "Direct experiments have proved to me that the quantity of oxygen absorbed, when compared with the quantity of carbonic acid disengaged, is greater in the non-saccharine blood of the jugular vein of a fasting animal than in the blood of an animal sacrificed during digestion and containing sugar; or more plainly, non-saccharine blood absorbs more oxygen, and extricates, relatively less carbonic acid than saccharine blood. The experiments of MM. Regnault and Reiset, made upon living animals, have furnished identically the same results; they have shown that the ratio of the oxygen exhaled in the form of carbonic acid to the oxygen absorbed is greater during digestion than during abstinence. It results from these experiments that the quantity of oxygen introduced into the economy is not proportional to the amount of carbonic acid formed." It has been demonstrated that a certain amount of carbonic acid is yielded up by the blood under the air-pump, as in the experiments of Magnus and Bernard, or is found to have been exhaled by the lungs and skin, as in the bell-glass experiments of Regnault and Reiset, which does not owe its origin to any systemic oxidative processes; this fact I shall speak of further on, as it is a necessary corollary of the views I first proposed in 1859, and which form the motive of this communication.

Bernard, in continuance of the subject, proceeds to detail some experiments, proving very clearly that oxygen possesses no especial influence upon the disappearance of sugar in saccharine blood. He found that in two samples of the same blood normally saccharine, through one of which a current of oxygen was passed for five or six hours, while carbonic acid was passed through the other for an equal period of time (evidently at the temperature of the laboratory, although the fact is not noted) sugar *did not disappear*. The gaseous currents being pretermitted, after a repose of twenty-four hours, sugar was found to have *disappeared*. Bernard continued the inquiry in the following desultory but suggestive experiments:—

Samples of saccharine blood were placed in contact with various gases (evidently at the temperature of the laboratory) confined under bell-glasses, as follows:—

From the peripheral extremity of the jugular vein of a healthy dog in full digestion, some blood was drawn containing sugar in small quantity.

1st. Twenty-four grammes of this blood was passed into a flask reversed over mercury, containing 500 cubic centimetres of oxygen.

2d. The same quantity of warm blood was passed into a similar flask containing nitrogen.

3d. A similar quantity of the same blood was placed in contact with carbonic acid.

After these arrangements the presence of sugar was again determined in the original sample of blood, in undiminished quantity. The blood rapidly coagulated, and was commingled with the overlying gas by agitation, becoming, of course, rutilant with oxygen, red with nitrogen, and very black with carbonic acid. After two hours, sugar had disappeared under the nitrogen, while a portion still remained under the oxygen and the carbonic acid.

Repeating these experiments (at the temperature of the laboratory), he operated as before: 1st, with pure oxygen; 2d, with carefully prepared hydrogen; 3d, with arseniureted hydrogen; 4th, with carbonic acid, and 5th, with air. The blood coagulated rapidly in all the flasks; it was shaken up with its overlying gas in each flask, remain-

Digitized by Google

ing red in all the flasks excepting that containing arseniureted hydrogen, where it became very black. After two hours, sugar had entirely disappeared under the arseniureted hydrogen while it was perfectly preserved under the pure hydrogen.

The next day, after twenty hours of contact, sugar was found to have disappeared completely under the arseniureted hydrogen and under the pure hydrogen, while traces of it remained under all the other gases.

Bernard comments briefly upon these experiments, though unable to deduce any definite conclusion from them as to the influence of the gases employed upon the disappearance of the sugar. One main fact was, however, plain enough, viz.: that in both sets of experiments the sugar had disappeared in from twenty to twenty-four hours, at the temperature of the laboratory. In view of this fact and of other considerations of similar significance, he declared that the disappearance of the sugar is not effected in the organism by a combustion or a direct oxidation, but by a *fermentation* "which cannot be arrested while life lasts," proceeding (p. 288) to explain that this cannot be by the fermentation of the sugar into alcohol and carbonic acid. "Under the influence of beer yeast sugar is transformed into these latter products, while if the yeast is disorganized by grinding in a mortar, the sugar is converted into lactic acid." He further states that it is impossible that the alcoholic fermentation can occur within the organism, because the yeast of beer, its proper ferment, is absent. "Should we endeavor," he observes, "to induce such a fermentation artificially, grave disorders would ensue, inducing death," going on to relate the history of an experiment in which a mixture of sugar and beer yeast was injected into the veins of a dog, the animal shortly dying of a grave malady, in which the blood became black, decomposed and viscid. "Sanguinolent diarrhœas supervened, death ensuing with symptoms analogous to those of the typhoid state." He then rejects the possibility of the destruction of hepatic sugar within the economy by the alcoholic fermentation, and still further remarks: "The destruction of sugar does not, therefore, occur in this way, but upon the arrival of the sugar at the lungs, *it may*, under the influence of the extreme division of the blood, be changed into lactic acid by a transformation involving only a molecular modification with the separation of two equivalents of water, in which oxygen would play but a secondary part. According to this view, the oxidation by which the carbonic acid originates would only occur, perhaps, in the general capillary system. I do not, however, think that this combination of oxygen with carbon takes place at the expense of matters directly poured into the blood, whether they be the resultants of digestion or of elaboration in the liver. I believe, on the contrary, that these new matters which, so to speak, have not yet lived, enter first into organic combinations, displacing the older matters, which are excreted in gaseous, liquid or solid forms. I do not believe, in short, that any of the phenomena, either of composition or of decomposition, are accomplished in the organism in a direct way" (pp. 239 and 240).

Bernard then commits himself, though not with any special emphasis, to the lactic acid theory of saccharine destruction, but without offering any further considerations in favor of it, or any experimental proofs of its occurrence. It is true that while experimenting upon the presence of sugar and glycogen in the muscles and lungs of fœtal calves, he found that if these organs were left in water at a temperature of from 59° F. to 68° F., after a little while the liquid became very acid, in consequence of the development of a considerable quantity of lactic acid, determined to be such by crystallization of its salts, and evidently derived from the glycogen and sugar which he demonstrated in such tissues.

Lactic acid is always formed where sugars are present in fluids containing albuminoid matters undergoing putrefaction. "The various kinds of sugar," says Regnault, "dextrine and sugar of milk, yield a large amount of lactic acid when they are mixed with a solution of diastase which has been exposed to the air for some time."

Digitized by Google

The diastase of germinating seeds and tubers converts their starch into sugar; if, when this is effected, further development of the grain is arrested by soaking in water, bruising and exposure to the air, the diastase soon begins to putrefy, and converts the sugar already existing into lactic acid, and, still later, causing the formation of a certain quantity of butyric acid. The lactic acid fermentation of the chyme, so frequently observed, appears to be due to a tendency toward putrefaction of the contents of the stomach, rendered possible by a vitiated condition or deficiency of gastric juice, naturally antiseptic, and the high temperature of the animal body.

The conditions attendant upon the lactic acid fermentation, as far as we know them, are certainly not present in the living organism. Bernard has suggested that extreme division might be one of the factors inductive of this fermentation in the lungs; but as this implies no more than an increased exposure to the action of oxygen, albuminous bodies, in the stages of change in which they are always found during the lactic acid fermentation, must be demonstrated as coexisting with this condition before admitting its validity. Indeed, if, as Bernard states, the alcoholic fermentation cannot occur in the organism because its proper ferment, yeast of beer, must be absent, by the same logic, the lactic acid fermentation cannot take place in the normal blood or liver, though possibly occurring as a pathological phenomenon.

Lactic acid is commonly formed in milk by a modification of its lactose, attendant upon exposure to the air at moderate temperatures in the presence of albuminoids. For its manufacture on a large scale, sugar is dissolved in boiling water, to which sour milk and tartaric acid are added ; rotten cheese is then thrown in as a ferment, and a quantity of chalk to combine with the acid as soon as formed. The whole is exposed to a temperature from 86° F. to 95° F. for ten days, and the lactic acid afterward separated by treatment with sulphuric acid.

Free lactic acid normally exists in the muscular tissues—enough, it is said, to neutralize the entire alkalinity of the blood ; what is its source? Lactic acid exists in the chyme as a normal constituent, according to some observers, being due to a conversion of glucose formed from starch by the saliva. If not over-abundant, it is neutralized by the alkaline secretions poured into the intestine below the pyloric orifice, like the acid phosphates of the gastric juice, and absorbed as a lactate. The lactates, like the malates, citrates, tartrates and acetates, are easily converted into carbonates by oxidation in the blood ; lactic acid, consequently, cannot exist as such in that fluid, at least in the normal state and while the oxidizing power of the corpuscles is undiminished. It has never been demonstrated in the liquor sanguinis. "All attempts to detect lactic acid in the blood have signally failed," says Lehmann. Nevertheless, I am inclined to agree with Ziemssen, when discussing the relations of lactic acid to *polyarthritis acuta*, that we may readily conceive that lactic acid may effect certain changes in the tissue before it is oxidized. With this admission, a momentary presence of lactic acid in the circulation may be imaginable, although it would be more reasonable to suppose that through faulty hepatic action, which indubitably exists in rheumatism, the glycogen of the liver is abnormally converted, giving rise to lactic acid to some extent only as a pathological product. This will be presently considered more carefully.

It does not, therefore, seem at all probable that the lactic acid of the muscle juice can be derived from the stomach or intestinal canal.

A small proportion of hepatic glucose is now admitted to be present even in centripetal venous blood. This quantity is notably increased during digestion, and it might be affirmed that the muscular lactic acid is derived from the glucose. It must not be forgotten, however, that the muscles contain a small quantity of sugar more nearly allied to lactose than to glucose or hepatic sugar. Inosite does not ferment into alcohol and carbonic acid, nor does it reduce the salts of copper. Glucose exists in the

Digitized by Google

cephalo-rachidian fluid, but we do not find that it exhibits any tendency toward a conversion into lactic acid.

Although the hypothesis of the lactic acid fermentation of glucose in the blood has generally prevailed, apparently in deference to the views of Bernard, I have been quite unable to discover any experimental or pathological foundation for it. Cantani's treatment of diabetes was based upon this hypothesis, and its temporary success would appear to substantiate it. Cantani assumed that by supplying lactic acid to the economy as an oxidizable substance, for purposes of calorifaction, in quantity sufficient for its needs, the progressive emaciation due to the incessant consumption of fats and albuminates might be arrested, and the characteristic subnormal temperature of diabetes elevated to its normal standard. He therefore proposed to administer lactic acid in quantities sufficient to accomplish these objects, but found himself limited, by the intolerance of the stomach and system to this remedy, to from 77 to 154 grains per diem. This was all that could be taken, a quantity wholly inadequate to accomplish the results arrived at, representing not more than five per cent. of what is normally assimilated in the form of carbohydrates, and quite insignificant as a calorifacient addition to the food. Cantani, however, simultaneously administered small *quantities of pure alcohol*, and rigidly excluded starchy and saccharine food, allowing his patients only an exclusive meat diet, a mode of treatment long ago established as fundamental, and, with the alcohol allowed, quite adequate to account for the improvement of his patients. The skim-milk treatment was also based upon the same theory, and, like all systems of treatment where starch and sugar are withheld, has been of decided though usually only of temporary benefit. In the hands of Balthazar Foster a trial of this lactic acid treatment, after Cantani's method, produced in two patients most sharply defined attacks of acute rheumatism, in one of them so constantly ensuing upon repeated administrations of the acid, and so constantly disappearing after its suspension, that it was impossible to ignore so plain an etiological sequence. The patient was able to take only two drachms of the acid daily after persistent attempts, a quantity utterly unworthy of notice as a pabulum for the systemic heating processes. Indeed, Foster remarks that the facts developed in these two cases had dispelled the "last lingering doubts" in his mind as to the truth of the lactic acid theory of rheumatism.

Ziemssen, in commenting upon this hypothesis, alludes to Foster's cases, and states that "Küelz has quite recently observed the same phenomenon in a diabetic patient under treatment for diabetes with lactic acid." Ziemssen satisfactorily invalidates the negative experiments of Richardson upon *healthy* rabbits, cats and dogs, as well as other objections to the causative influence of lactic acid in the development of acute rheumatism, by calling attention to the obvious necessity of the simultaneous existence in the economy of various predisposing influences, and strongly endorses this lactic acid hypothesis of rheumatism, as follows: "The above considerations appear to indicate that the theory of an accumulation of lactic acid in the system deserves more attention in relation to the pathology of rheumarthritis than has been hitherto accorded to it. It is well worthy of being tested further, both by experimentation and by clinical observation."

The presence of products of the uric acid type in abnormal abundance in the system during rheumatic fever indicates that the functions of the liver are seriously perverted, and it becomes quite possible that the glycogenetic function may be simultaneously modified, so as to cause a deviation from the normal processes by which glycogen and glucose are transformed, so that a formation of lactic acid may become established as a pathological process, in concurrence with the acute lithæmia. The successful employment of alkalies alone, and of these in conjunction with colchicum and the salicylates, points to the effective neutralization of *acid substances* in the *blood*, and to an abatement and modification of morbid trains of action occurring in the *liver*, for the salicylates

Digitized by Google

undoubtedly control the processes of heat formation which are known to occur in that organ, and must, therefore, affect the secretory acts of the liver, both qualitatively as well as quantitatively.

It may be also remarked that there is no natural desire among animals or in man for lactic acid, and there exists, on the contrary, a very marked intolerance to it. Lactic acid is a very powerful acid, capable of decomposing the chlorides even with the aid of heat, and some chlorides at common temperatures. When injected into the blood, it acts like other acids, destroying life long before the alkalinity of the blood is overcome. (Bernard, "Leçons," p. 236.) While, therefore, physiological considerations fail to substantiate the occurrence of the lactic acid fermentation of glucose within the economy, pathological and therapeutical observations furnish us with good reasons for regarding this process as a morbid one, which may be set in motion by certain functional perversions of hepatic action.

From *a priori* considerations, on the other hand, the possibility of the occurrence of the alcoholic fermentation of hepatic glucose is very reasonably admissible. As has been stated, Bernard emphatically rejected this hypothesis because "beer yeast, the proper ferment of sugar, must, necessarily, be absent from the economy." I have carefully verified this statement, also observing that the animal may be saved from death by an almost instantaneous thrombosis of the vein into which the mixture of yeast with water is injected; sugar alone may be injected into the veins with impunity. Bernard is, however, in error in defining beer yeast as the proper ferment of sugar, as yeast ranks only as the most potent form of ferment yet discovered for the induction of the alcoholic fermentation. It is not employed in nature for this purpose, and we may observe numerous instances in which this fermentation is accomplished without it. "Muscular flesh," says Regnault, "urine, gelatine, white of eggs, cheese, gluten, legumin, extracts of meat and blood, left to themselves for some time, exposed to air and moisture, cause sugars to ferment and convert them into alcohol and carbonic acid." In distinct cells of the ripened grape are contained a saccharine fluid and an albuminous material, which are kept apart and exert no influence upon each other as long as the fruit is fresh and uninjured. When, however, the envelopes of these juices are broken, they become commingled, and enter into regular fermentation, sometimes within *thirty minutes*. If a bunch of ripe grapes be expressed, one by one, under a bell glass filled with mercury and reversed over it, the juice rising into the glass remains unchanged for any length of time, but if a globule of air be admitted, the liquid begins forthwith to ferment. Wine of all kinds, therefore, is produced by the fermentation of sugar without yeast. Indeed, the juice of any vegetable which contains sugar and an albuminous matter will take on the alcoholic fermentation by simple repose at moderate temperatures. The *chica mascada* of South America is made by the action of the saliva upon partly masticated maize. When a certain quantity has been duly ground between the teeth of the natives, it is placed under ground, and by the agency of the albuminous matter of the grain, and some further change, probably of the salivary ptyalin, is converted into an intoxicating liquid. The juice of the agave furnishes the Mexican *pulque;* that of the sugar cane the *guarapo* of the West Indian negro; of the spadix of the palm, the East Indian palm wine or toddy; of apples, cider, and of pears, perry, etc. In these and in other instances yeast is not required; yeast is employed in the arts because it is the most effective agent known for the induction of a prompt and complete alcoholic fermentation in saccharine fluids prepared by artificial processes, as malted infusions of grain or solutions of molasses or honey. Such liquids will, however, ferment spontaneously at a later period if left unyeasted. I have found that saccharine fluids made by boiling finely-ground grain with dilute acids ferment quite as well without yeast as with it, though at a later period. Thus, such a mixture

Digitized by Google

when yeasted will begin to ferment within five or six hours, at a temperature of 75° F., but only after from twenty-four to thirty-six hours if left without yeast.

Yeast, therefore, is not necessary for the accomplishment of the alcoholic fermentation; albuminoid ferments of various origin are capable of inducing this fermentation. The truth may be, perhaps, more exactly expressed by saying that many different albuminoids seem to be able to assume a polar condition during a progressive molecular change tending toward putrefaction, by reason of which they become capable of exciting the alcoholic fermentation, which polar state is similar or the same in all of them. It may be said that this fermentation cannot occur without the presence of the *micrococcus cerevisiæ;* to this may be answered that while the unfailing attendance of the micrococcus upon the alcoholic fermentation is indisputable, so far as the process has been observed in *saccharine fluids out of the body* or even in diabetic urine, the *relationship* of the microscopic plant is not yet established to the alcoholic fermentation. The question here touches upon the germinal doctrine, which I prefer to disregard in this connection, because, in the first place, it is very distinctly upon trial in the scientific world as to the facts supporting it; and secondly, because in this place the question is between the existence in the organism of one or the other of two modes of the fermentation of sugar, *one of which* undoubtedly occurs, while both are supposed to be induced by germinal influences, by those who accept the germinal hypothesis, although no one has ventured to assert that a micrococcus of any kind is present or active in the alleged transformation of glucose into lactic acid in the general blood mass. As it is, therefore, out of the question to argue with respect to either of these hæmal fermentations from a germinal point of view, I have preferred to do so from a Liebigian standpoint, which I beg leave to say, to my mind, affords us incomparably broader and more scientific views of these intricate and little understood phenomena. We must bear in mind that there are several stages in the conversion of starchy matters either into lactic acid or into alcohol and carbonic acid. Any given ferment is competent to guide the amylaceous matter through more than one of these stages; so, likewise, are acids, which not only cause starch to pass into dextrine, but also convert the dextrine into sugar. There exist abundant reasons for believing that the ferments which change starchy matters into dextrine, and thence into sugar, may, likewise, perhaps progressively changed themselves, cause a still further conversion of the saccharine matter formed either into lactic acid or into alcohol and carbonic acid, under conditions which we may note, but which we certainly do not comprehend. Such a progressive action of the hepatic or blood ferment must be invoked under the lactic acid hypothesis of saccharine conversion in the blood, and equally so under that of the alcoholic fermentation.

If alcohol is a physiological constituent of the blood, why, it might be asked, do we not find it, like starch, the sugars and the vegetable acids, in fresh saps and living plants and seeds? The reason is one, apparently, which underlies the relationship of the animal to the vegetable kingdom. Alcohol is the product of a metamorphotic change by which force, in the form of heat, is extricated. The liberation of force within the animal organism is fundamentally subservient to its functions; it would, therefore, be a plain violation of natural harmony, if this force-giving process (fermentation) should occur before the matters used as food are taken into the animal body. It is desirable that this fermentation, with its attendant heat and the production of an easily oxidizable matter, capable of furnishing still further supplies of heat, should occur within the animal body, for the sustentation of its proper temperature. Consequently, the alcoholic fermentation does not occur in the vegetable world as a normal process, but only in such fruits and juices as are exposed to the air, or whose investing textures are broken. The unfermented ripe fruit or grain is sought by animals, invited, so to speak, by nature to partake of it, and so aid in the continuation of the plant species by

Digitized by Google

distributing the seeds imbedded in saccharine pulp. The alcoholic fermentation is a process of destructive metamorphosis, of the conversion of colloids (starch and dextrine) into crystalloids (sugar, alcohol and carbonic acid), and therefore belongs essentially to the animal kingdom, and should naturally occur in animal bodies. The functions of plant life, on the other hand, are constructive, crystalloids being compounded into colloids, and thereby strictly opposite and complemental to the processes of animal nutrition. While, therefore, the starch and sugar, and eventually even the nitrogenous matters of the food of animals are the source of glycogen and glucose, and of alcohol, in the destruction of this last-named body, alcohol itself is nowhere found in living plants.

I have adverted to Bernard's studies of the glucose and lactic acid found in the lungs and muscles of fœtal calves. It would seem that the sugar formed from the glycogen of these structures is as distinctly prone, to say the least, toward the alcoholic fermentation as toward the lactic, especially if the temperature be somewhat high. We find in the "Leçons" the following important note upon this point: "The muscles and lungs of fœtal calves (not older than the fourth month of gestation) are washed and immersed in water enough to cover them. In order to prevent a rapid lactic fermentation, so that the intermediate presence of sugar might be recognized, a fourth or a third of alcohol was added to the water. (Alcohol possesses the power of preventing the lactic acid fermentation, while it does not arrest the alcoholic.) By the next day the liquid becomes charged with sugar. We find then, upon collecting the air imprisoned in the flask, especially if the temperature be elevated, that a disappearance of oxygen and formation of carbonic acid has taken place. This gas cannot come from the lactic fermentation, which consists simply in the fixation of an equivalent of water by the sugar. Does it arise by the saccharine (*glycosique*) fermentation at the expense of the organic matter? We cannot determine this at the present time." (*Loc. cit.*, p. 382.) It is very plain that the carbonic acid in question must have been derived from the alcoholic fermentation, the oxygen having been absorbed in the process of acetification ensuing at the elevated temperature named. (I am now experimenting upon this point, and will report the results in this paper before its publication.) The lactic acid fermentation coincided with the later stages of the alcoholic, and the acidity observed at the end was probably due both to acetic and to lactic acid.

Bernard has shown that the lymph of the thoracic duct at its entrance into the subclavian vein is normally saccharine, in consequence of the addition of lymph containing sugar brought by the hepatic lymphatic vessels. The large lymphatics belonging to the deep set, which emerge from the transverse fissure of the liver to join the lacteals, always contain sugar in healthy animals, derived by direct absorption from the hepatic stroma. Cruveilhier observes, with regard to those vessels, "They are often filled with yellow lymph, and are sometimes found distended with gas in cases of commencing putrefaction." This gas can be no other than carbonic acid, and of course implies a previous alcoholic fermentation of the hepatic sugar under the influence of ferments present or originating in the body. I shall detail further on some experiments showing that the alcoholic fermentation arises spontaneously in the liver and fluids derived from it long before putrefaction begins.

Finally, we must notice the extraordinary appetite for alcohol which exists in the animal kingdom when once a knowledge of this substance is acquired. Moreover, a most remarkable tolerance for alcohol naturally exists, and is easily acquired for very large quantities. Such large quantities, within certain limits, have been shown, by Binz, Anstie and Dupré, to entirely disappear in the organism. A remarkable appetite exists, likewise, for saccharine substances, which are especially craved and sought after by all classes of animals, the dissemination of seeds and fructification of flowers being dependent more or less upon this instinct for sweets. A similar craving for acetic

Digitized by Google

acid, in man at least, is very widely diffused. Sugar in many forms and several varieties, alcohol in numerous combinations, and acid preparations containing vinegar are so commonly used as food that they form items in the diet allowed to soldiers by all civilized nations. It is evident that so marked an inclination and tolerance must be based upon the occurrence of intra-systemic processes in which these matters play some important part.

On the other hand, lactic acid is badly borne by the stomach ; tolerance of it is difficult to acquire, except in small quantities, and wholly impossible in quantities adequate to administer to the processes of heat formation. Its presence in the stomach in notable quantity, as in acid dyspepsia, is followed by chronic inflammation of the organ, while great pain is experienced as long as the food remains in the stomach and often during the transit of the imperfectly neutralized mass through the small intestine.

I have cited Bernard's experiments upon the disappearance of sugar in samples of blood submitted to the action of various gases, from which he concluded that sugar disappears in the blood by some kind of fermentative movement, at the same time rejecting all hypotheses of direct oxidation. I determined to review these experiments, and to extend them by ascertaining how sugar comports itself in blood and liver tissue maintained at the temperature of the animal body, whence it was derived. Experimenting in this direction, I have obtained the following results :—

Sugar gradually diminishes in quantity in samples of mixed thoracic ox blood, which always contains it at ordinary temperatures, though it may be recognizable after several days.

The sugar disappears more rapidly as the temperature is more elevated. Thus, at a temperature of 53° sugar may not be destroyed after ninety-six hours, while, at a temperature of 55.5° it may totally disappear after ninety hours. In another sample sugar disappeared at 63° almost entirely, after forty-four hours. The disappearance of the sugar was seldom complete before signs of incipient putrefaction occurred, though by far the greater portion of it had disappeared. Thus, sugar may quite disappear at a mean temperature of 55°, while as yet, even after four days, no signs of putrefaction have become manifest.

The disappearance of the sugar is due to influences which antedate putrefaction ; whether they are intrinsically similar or not it is not possible to affirm positively.

Sugar progressively increases during twelve hours or more in liver tissue, at temperatures between 50° and 66° ; it is then destroyed by a slow fermentation, with the formation of a considerable quantity of acetic acid. At higher temperatures, from 80° to 90°, an unmistakable alcoholic fermentation sets in within the substance of the tissue when no water has been added to it, after a lapse of from twelve to fifteen hours. Sugar does not promptly disappear in liver tissue, even after putrefaction is fairly established ; the liver first becomes highly acid, and contains a large amount of acetic acid.

When we add enough water to cover a mass of liver tissue, at a temperature of about 84°, the following changes are noted : After two or three hours the liver tissue changes color, becoming whitish, and bubbles begin to form upon its cut surfaces. These increase in number, the water becomes covered with froth, and after four or five hours a tumultuous alcoholic fermentation is established, which completes its course with extraordinary rapidity, in not more than three hours. The liquid soon becomes acid from the development of acetic acid. Alcohol can be recovered from it by distillation, in small quantity only, while the presence of much acetic acid is demonstrable. All these phenomena occur before the least development of putrescence takes place. The conversion of the original glycogen evidently continues, and the resulting sugar dissolved partly in the water covering the mass, and also remaining in the substance of the liver, passes rapidly into a most tumultuous alcoholic fermentation, and almost simultaneously into the so-called acetic fermentation of the alcohol formed. These phenomena seem to

Digitized by Google

occur in virtue of a special proclivity toward the alcoholic fermentation proper to the solids and juices of the liver.

When sugar-bearing blood is maintained at its somatic temperature, sugar disappears in from three to five hours. Thus, in one case, at a temperature of 105°, it disappeared in about three hours; in another at the same temperature, after four hours; in a third at a temperature of 105.3°, within five hours; in a fourth case, at temperatures between 103° and 105°, after four hours; in a fifth experiment, in which sulphureted hydrogen had been passed through the blood, the sugar disappeared in three hours and fifty minutes. The disappearance of the sugar was in no obvious way hastened or retarded by the treatment with sulphureted hydrogen.

I found, as Bernard had done, that the disappearance of the sugar was evidently due to a fermentative movement, unaffected by the oxidative qualities of the blood, occurring, therefore, without perceptible modifications, whether the blood was treated in closed vessels, with frequent agitation, in contact with oxygen, sulphureted hydrogen, carbonic acid or air. The disappearance of the sugar at somatic temperatures I have invariably found complete before anything like putrefaction had begun; in blood containing sugar so treated, the reaction of a clear extract, made after the disappearance of the sugar, is neutral or faintly acid.

Proceeding to study the behavior of exsected liver tissue at its somatic temperature, I observed that the phenomena already noted as occurring when the tissue was immersed in water enough just to cover it, at an atmospheric temperature of 80° to 90°, became still more marked and set in earlier. As this is a typical experiment, I quote it entire.

Object: *To test the phenomena of the disappearance of sugar in liver-tissue, maintained, as soon as practicable after exsection, at its somatic temperature.*

February 28th, 1876, 2.20 P.M. Ox struck down; a thermometer plunged into the centre of the liver at the moment of its extraction registered 106°. Temperature of the liver when received, forty minutes later, 80°. *Sugar abundant.*

3 P.M. Eight ounces of this liver tissue was placed in a covered beaker with an equal quantity of distilled water, the whole being set in a water bath. The liver was in one piece.

3.10 P.M. Temperature of the liver 106°. A portion tested is very rich in sugar. As 106° was the somatic temperature of this liver, it will be maintained at that temperature.

3.45 P.M. The reaction of the liver is neutral; *it sinks in the water.*

5.15 P.M. Two hours after elevation to the somatic temperature have elapsed. The superjacent liquid is already distinctly acid.

6 P.M. Sugar is abundant in a morsel of tissue taken from the middle of the mass. The temperature is steadily maintained.

9 P.M. Sugar is far more abundant. (These comparative estimates were reached by using equal portions of the solid or fluid matters, and comparing the color after boiling with Fehling's test and the depth of the sediment.) The supernatant liquid is still more acid. (The following remarkable phenomena were then there observed.) A fermentative movement is in full progress. The liver masses have swollen greatly. The circumnatant fluid, at first sanguinolent, though transparent and without flocculence, is now full of large flocculi, which are driven about by the commotion of the liquid. The surface of the liquid is covered with a whitish scum, like that seen on the surface of a brewer's vat. The bottom of the beaker is strewed with masses of coagulated matters and liver débris, some of which rise quickly to the surface, borne by entangled bubbles of gas, and fall again when this escapes. The masses of liver have quite changed color, being now whitish, and they yield a whitish froth when a cut surface is squeezed. The contents of the hepatic vessels are forced out spontaneously as a similar whitish foam, from the recesses of the tissue, by the pressure of the gas generated within it. Where

Digitized by Google

a cut surface lies in contact with the wall of the beaker, it is seen through the glass to be covered with innumerable fine bubbles, evidently springing out of the liver tissue itself. The liver is plainly the seat of a tumultuous fermentation advancing in the recesses of its tissue.

10.15 P.M. The fermentative commotion is still more marked. A lighted match put under the glass plate covering the beaker is suddenly extinguished. The liver is now quite acid to test-paper. It crepitates between the fingers, showing the presence of gas, and the advance of the fermentation in its interior. Sugar was not tested for again.

The liver masses and circumnatant fluid were placed in a distillatory apparatus over a bath of common salt and distilled. The first few drops of the primary distillate were strongly acid, and this acidity continued for some time as the distillation progressed. Acetic acid was present in the distilland. The final distillate assumed a bright green tint when tested with chromic acid dissolved in sulphuric acid, but the alcohol was in very minute quantity, having evidently been lost by acetification. In a second experiment, a portion of liver which had been submitted to a temperature of 61° on an average, for sixteen hours, was found to be still richer. At this time its reaction was neutral or faintly alkaline; it emitted no odor whatever, nor were there any bubbles or scum upon the liver itself or on the distilled water in which it was immersed.

Feb. 29th, 1876, 10 A.M. Under these conditions about three ounces of this liver tissue, with most of the superjacent liquid, was placed in a beaker set in a water bath and maintained with great care at 106°, which was the somatic temperature to the liver.

12.45 P.M. Froth begins to form on the edges of the liquid.

1.10 P.M. Fermentation plainly commencing.

2 P.M. Fermentation in full progress.

4 P.M. Liver in full fermentation, which also advances in the circumnatant fluid quite as actively as if it had been an artificial solution of sugar to which yeast has been added. Reaction strongly *acid*. The temperature (106°) is steadily maintained. By a small pneumatic contrivance three-fourths of the capacity of a large test-tube of the gas evolved was collected. *It instantly extinguished a blazing splinter of wood; it was carbonic acid.*

5 P.M. The fermentation is over.

8 P.M. Upon trial, sugar has entirely disappeared. A portion of the circumnatant fluid which had not been boiled was treated with freshly calcined animal charcoal; filtered; boiled in a test-tube firmly closed by the thumb; filtered anew through animal charcoal; came through perfectly clear; tested with the chromic acid alcohol test, the fluid became at once of a vivid emerald green without further heating than that attendant upon the addition of the test (which contains sulphuric acid) to the filtrate. Alcohol was, therefore, present in considerable quantity. By distillation the acidity was shown to be due to a volatile acid coming over with the very first portion of distillate; it was consequently acetic acid. This distillation was not, however, pushed further. The fermentation observed in these experiments, and since in similar ones, was evidently the alcoholic; not to any appreciable extent the lactic, which evolves no gas, and is never accomplished with the tumultuous vigor noted in these cases, where the fermentative movement was almost explosive in violence. The gas evolved in such large quantities was carbonic acid. The presence of alcohol in the masses of liver and in the fluid around them was determined by the chromic acid test; but I must call *particular attention to the fact* that the quantity of alcohol was very small, greatly less than would have been recovered from a decoction made with an equal weight of the same liver tissue and fermented with yeast at a *moderate temperature*, *e. g.*, about 65° to 70°. The

Digitized by Google

diminution of the alcohol must have been owing to acetification at the high temperature maintained, the extent of the alcoholic fermentation itself having been shown by the very large amount of carbonic acid thrown off.

The sugar did not disappear, as in saccharine blood, but became more and more abundant, in virtue of the conversion of glycogen into glucose, the latter substance, even in the midst of the liver, passing at once into the alcoholic fermentation.

The presence of a ferment or of a fermentative disposition in the blood of the liver capable of converting glycogen into glucose at somatic temperatures, had been affirmed by Bernard; he took no note, however, of the effects of temperature upon these processes. The experiments detailed, which I have frequently repeated, illustrate the great influence which temperature exerts upon them. They also show that the alcoholic fermentation in the liver ensues directly upon the saccharification of the glycogen; if the temperature is high enough, with tumultuous violence; but, as I shall presently show, without any specially marked phenomena, if the temperature be low; in both cases, however, with the formation of some acetic acid. At high temperatures the formation of acetic acid is excessively rapid in these liver masses, and the change would appear to be induced by the same ferment which caused the conversion of the glycogen into glucose and this latter into alcohol and carbonic acid, in a more advanced and consequently modified stage of molecular change, and, consequently, of catalytic efficiency.

It must be borne in mind, though not generally known except as a scientific fact, that the alcoholic fermentation will progress regularly and with extraordinary rapidity at temperatures even of 120° to 130°, high enough for the fermenting fluid to boil, when the pressure of the atmosphere is removed. Processes have been devised for removing alcohol from fermenting liquids by the air pump, as fast as it is formed, in order to avoid the influence of the alcohol itself in the fermenting liquid in arresting the conversion of the last portions of sugar. This plan has proved perfectly feasible, but cannot be conducted except in conjunction with the vacuum pump, which delivers the alcoholic vapors over to a condensing apparatus, because at the high temperature employed, if air were admitted, a concurrent acetification would destroy the alcohol as soon as formed. Whenever the alcoholic fermentation is accomplished at a temperature above 60° to 65°, there is a concurrent acetification, which becomes more marked as the temperature rises, and becomes most effective at 100° and upward. When we ferment large masses of wort made by boiling grain with dilute acids, the nitrogenous principles being thoroughly dissolved, and in some way rendered exceedingly oxidizable by the prolonged action of the dilute mineral acid, the process of fermentation, once kindled, runs a violent course, often in a few hours, if it is not restrained by cooling apparatus. I have often seen such masses rise in temperature from 60° to 80° or 90°, with the inevitable loss by acetification of three-fourths of the alcohol. At lower temperatures the alcoholic fermentation proceeds more slowly and the sugar is more completely converted, while the oxidative process we term acetification is entirely checked below 60°. Even at high temperatures, this process of acetification is one which experience has shown to be very promptly affected by a slight decline in temperature, a fact always carefully considered in the conduct of processes for making vinegar.

The accomplishment of the alcoholic fermentation with a concurrent acetifying process in perfectly fresh liver tissue, at the temperature of the animal body, determined in the foregoing experiments, affords us strong reasons for inferring that similar phenomena take place during life as normal manifestations, for we know that many fermentative movements occurring soon after death, as the action of the ferments of the intestinal canal, the post-mortem digestion of the stomach, etc., are really continuations of proper vital processes. In the liver this has been proved to be true beyond any sort of doubt, for the conversion of glycogen into glucose occurs in the most marked manner

Digitized by Google

after exsection of the liver, which hour by hour becomes more charged with sugar up to a certain point, the post-mortem conversion being plainly a continuation of that in continual progress during life. Hence, it appears safe to conclude, that inasmuch as soon after death and before decomposition begins the glucose is fermented and its product acetified within the substance of the liver, the same phenomena occur in the liver and in the blood, which bears away the hepatic glucose and is impregnated with the hepatic ferments or retains the fermentative proclivities imparted to it in the liver.

I must still further direct attention to facts which I have long since ascertained, viz., that the tissue of the liver possesses remarkable powers in exciting the fermentation of saccharine solutions and in saccharifying starch. If to a mass of dough a small quantity of pulped, uncooked liver mixed with a little water be added, and the whole maintained at about 80° F., after seven or eight hours the dough is filled with bubbles, becoming excellent yeast, capable of making porous and wholesome bread. When, again, a little pulped liver tissue is added to a solution of sugar, at the same temperature, in about four hours the liver tissue is seen to undergo that intrinsic alcoholic fermentation which I have described. This passes by in a few hours more, but after the lapse of from twenty to thirty hours from the beginning of the experiment, the alcoholic fermentation sets in vigorously in the saccharine solution, and advances as regularly and as satisfactorily as if yeast had been added. Nothing of this kind occurs with the muscular tissue or with the tissue of the lungs. The following comparative experiment illustrates these remarkable properties of the tissue of the liver:—

August 15th, 1887, temperature air, 88°, 2.15 P.M.

No. 1. Into a solution of glucose (18° Baumé) a cubic centimetre of fresh compressed yeast was placed, and mingled with it.

No. 2. Into a similar solution of glucose two cc. of calf's liver pulped with a little water was placed.

No. 3. Into a solution of sugar of milk a similar quantity of liver tissue was stirred.

No. 4. Into a solution of sugar of milk a similar quantity of yeast was placed.

No. 5. Into a solution of cane sugar two cc. of liver tissue was placed.

No. 6. Into a solution of cane sugar two cc. of the same compressed yeast was placed.

No. 7. In an equal quantity of water two cc. of pulped liver tissue was placed, as for control.

No. 8. In an equal quantity of water two cc. of the same compressed yeast was placed, as for control.

No. 9. Four cc. of the same liver tissue was pulped with a little water (two teaspoonfuls), and incorporated with a certain quantity of dough, enough to half fill a glass goblet.

No. 10. Four cc. of the same yeast was mixed with the same quantity of water, and incorporated with another portion of the same dough in equal quantity.

No. 11. A similar quantity of the same dough was set aside, as for control.

No. 12. Into a solution of glucose (18 Baumé) two cc. of the muscular tissue of beef, first pulped, was placed.

No. 13. Into a solution of glucose (18 Baumé) two cc. of ox-lung tissue, as well triturated as possible with two drachms of water, was placed.

All of the fluids were in equal quantity and contained in tall champagne glasses of the same set. All the samples were placed together, side by side, upon a mantel-piece. The parcels of dough were placed in glass goblets, which they half filled.

After forty minutes the yeasted dough had risen to the top of the glass goblet, and the experiment was discontinued.

After one hour and a quarter the yeasted glucose solution was in full fermentation,

Digitized by Google

and soon afterward the yeasted cane-sugar solution, and both of these experiments were discontinued.

After four hours, the temperature of the air having remained unchanged, the pulped liver masses lying at the bottom of the glasses containing (No. 2) the glucose solution, (No. 7) the water, (No. 5) the cane-sugar, and (No. 3) the sugar of milk, began what we may term their intrinsic fermentation. The liver masses changed color, developing numerous bubbles in their midst, and rising and falling through the liquids or floating for a time on the surface of the saccharine solutions. A very active fermentation of these masses, evidently *not propagated* to the solutions in which they lay, and plainly due to ferments originally present in the liver tissue, continued for two or three hours and then ceased entirely, the liver masses subsiding to the bottom of the glasses in all cases.

Nothing more was observed until thirty hours had elapsed; the temperature had been meanwhile very constant, ranging between 86° and 90° F. The glucose solution now began to ferment in mass, with complete regularity, and ran its course in forty-eight hours, the liquid having become attenuated to density of 6° Baumé. During the fermentation the mycoderma cerevisiæ was found to be very abundant upon microscopic examination.

The solution of cane-sugar treated with liver tissue began to ferment twelve hours after the glucose solution (No. 2), and pursuing its regular course, reached an attenuation of 8° Baumé.

At the eightieth hour the glucose with beef muscle had not fermented at all, although it showed an attenuation to 16° Baumé, or two degrees of the saccharometer.

The sugar of milk with yeast did not ferment, nor did the sugar of milk with liver tissue; nor was any change observable in the solution of glucose with lung tissue, or in the control experiment of the yeast placed in water.

By 10 P. M. of the evening of the first day, *i. e.*, after about eight hours, the dough to which the liver pulp had been added was observed to have become fluid, and was filled with frothy bubbles throughout its entire mass, possessing an agreeable odor of yeast. The other dough remained unchanged for three days, becoming acid and acquiring a disagreeable smell.

We observe that the liver tissue behaved like a true yeast, acting in all respects like the beer yeast employed, exciting the saccharine fermentation in dough, and inducing the alcoholic fermentation in glucose and cane sugar. Like the yeast, it failed to evoke any trace of alcoholic fermentation in the sugar of milk. Neither the lung tissue nor the beef muscle appeared to possess any power whatever, even after the lapse of eighty hours, at a temperature of 86° to 90°, to excite the alcoholic fermentation in either glucose or a solution of cane sugar.

The odor of the saccharine fluids fermenting under the influence of liver tissue is yeast-like and pleasant, while the saccharine fluids into which the portions of lung tissue and beef muscle were placed, refusing to ferment, soon acquired a disagreeable odor of putrefaction. Hence the liver is remarkable: 1st, for the singular development of its intrinsic fermentation as described, sooner manifested and completed, and accompanied with a more intense concurrent acetification of the alcohol produced, as the temperature is higher and approaches that of the animal body; and 2d, for the fact that its tissue is invested with an ability to excite the alcoholic fermentation comparable to that of the best brewers' yeast in intensity and regularity, though by no means so promptly active. It is also evident that the ferment which produces the intrinsic alcoholic fermentation of the liver is a different one from that which eventually induces this fermentation in saccharine fluids in which the liver is immersed; for the first is accomplished and finished twenty or more hours before the second begins. If, therefore, it should be affirmed that the micrococcus cerevisiæ was the cause of the later

Digitized by Google

fermentation, *i. e.*, of the mass of the saccharine solution, it could not have been the inducing agent of the intrinsic liver fermentation, else the first fermentation would have been continuous with the second, while in reality there was a distinct interval of repose of twenty hours between the two fermentations.

Considerations like those I have detailed, led me long ago to believe that a fermentation of hepatic glucose into alcohol and carbonic acid normally progresses in the animal body. It had been my privilege to attend the courses of lectures given by Claude Bernard in 1852 and 1853, and after my return to America I began to consider this question of the disappearance of sugar in the economy, which he had not fully analyzed. Such reasons as I have given led me to regard the lactic acid hypothesis as quite insufficient to explain the facts, and I soon determined to test the matter experimentally by submitting the blood and tissues of animals to careful concentrative distillation, hoping to procure alcohol in definite form thereby, if this substance is really present in the economy. The working hypothesis under which I proceeded, accordingly, to conduct the series of experiments involved, was the one already suggested in this paper, viz., that hepatic glucose is fermented in the blood into alcohol and carbonic acid; that the carbonic acid is exhaled by the lungs and skin, and that the alcohol is oxidized by a process akin to that occurring when the vapor of alcohol, mixed with air or oxygen, is acted upon by platinum black; the phenomena of oxidation being also similar to the one known in the arts as the "quick vinegar process," while beyond this point the oxidative sequence was probably the one chemically normal to alcohol destroyed by slow combustion, viz., the successive formation of formic acid, oxalic acid, and carbonic acid and water.

Considering the normal high temperature of the body, the formation of alcohol and its acetification and further oxidation must be parallel processes, simultaneously active, the alcohol scarcely *existing as such in the blood, or only in very minute quantity*, being consumed by oxidation as fast as formed by fermentation. It would still, however, be possible to imagine that the alcohol might exist in a recoverable form for a moment of time, during a portion of the blood transit, or possibly a round or two of the circulation, for, although sugar is destroyed in the blood, it may, nevertheless, be detected even in the venous blood derived from the systemic capillaries, as Bernard has demonstrated. Especially might it be possible to procure alcohol from the blood, if the unceasing oxidative process in which it was assumed to be destroyed could be artificially abated or arrested; and as this occurs by the arrest of respiration accompanying the slaughtering of animals, and also by the mere fact of withdrawing the blood from its vessels, the function of the lungs being thereby pretermitted, I thought it not improbable that in the blood of the lungs remaining in those organs after death, as well as in masses of blood collected from the throats of slaughtered animals, I might reasonably expect to recover a certain small quantity of alcohol which had been formed by normal processes, but which had not been destroyed, as it normally should have been, in consequence of the arrest of the oxidative processes under the conditions designated.

At the same time I became aware, from certain calculations, that even if oxidation were in complete abeyance the quantity of alcohol existing at any given moment of time in the blood must be very minute indeed, as the rapid motion of the blood must quickly disseminate whatever might be present throughout the system or through extensive networks of capillary vessels, and that this must be the case, although this minute quantity of alcohol really represented the entire quantity of these proximate elements of the food, which, as we know, are subservient to the glycogenetic function

Moreover, as it is out of the question to test the fluids or solids of the animal body for alcohol within a few minutes or even seconds, as we may do for saline bodies or for sugar, and as the organs and blood must first be extracted carefully, and then, after

Digitized by Google

some inevitable delay, brought to the boiling point, some time must elapse, during which the vital chemistry of the organs concerned may most reasonably be assumed to remain unchanged. This period of inevitable delay, therefore, would allow of the formation of a small quantity of alcohol under conditions adverse to the accomplishment of that elaborate process of oxidation to which we know that substances are subjected in the circulating blood stream. I might reasonably expect, therefore, to obtain by distillation a certain quantity of alcohol formed after death and before exsanguination or exsection of the organs, and also an additional quantity formed by a continuance of normal action during the period of time elapsing between extraction of the matters operated upon and their elevation to a temperature capable of arresting all fermentative movement. It is not to be supposed, however, that the quantity of alcohol can ever equal what is assumed to be normally formed in a given time, for the arrest of oxidation, in the form of acetification at least, cannot be supposed to be complete, as the blood remains charged with oxygen and retains its temperature for a considerable period after extraction, as both the liver and lungs, in man, do likewise.

But first, it is worth while to determine what this theoretical quantity of alcohol present in the blood at any given time may be, oxidation being supposed absent.

"From the accurate determination of the quantity of carbon daily taken into the system in the food, as well as that proportion of it which passes out of it in the fæces and urine in some form in which it is not combined with oxygen," says Liebig, "it appears that an adult taking moderate exercise consumes 13.9 ozs. of carbon daily." The carbohydrates ingested pass directly into the forms of glycogen and glucose, but it must be recollected that the ingested albuminoids are to be conceived of as resolved by ultimate conversion after ministering to the nutrition of the tissues (mainly) into glycogen, urea and the elements of the bile. The quantity of carbon noted above may, within certain somewhat narrow limits, be regarded as derived from hepatic sugar destroyed in the blood, indicating therefore, according to the formula of hepatic sugar ($C_{16}H_{12}O_6$), 35.30 ozs. (Troy) of this substance daily converted into its final products. Now, if this amount be fermented into alcohol and carbonic acid, it would correspond to no less than 18 ozs. (Troy) of alcohol, equal to 8640 grains.

Liebig estimates the carbon consumed by each prisoner in the House of Arrest at Giessen at nine ozs. Hessian (one oz. Hessian = 482 grains Troy). This amount of carbon would be contained in 22.5 ozs. of hepatic sugar, which would represent a daily consumption of 11.5 ozs., equal to 5520 grains of alcohol. As all of this must be supposed to pass through the heart, there would be present during one minute in the pulmonary blood the 1440th part of it, or exactly six grains by the last estimate, and 3.83 grains by the present one. According to Müller, ten pounds of blood = 76,800 grains, traverse the lungs in the same time. The pulmonary blood, therefore, cannot contain more than $\frac{3.83}{76,800}$ to $\frac{6}{76,800}$ grains of alcohol during any given minute, equal to 0.4857 to 0.7812 of a grain of alcohol to 10,000 grains of blood.

We may denominate these calculations by "ingested carbon." Let us compare them with a similar one based upon the "expired carbon."

Scharling estimates the carbon given off from the lungs of a man in twenty-four hours at 325.30 grammes. Supposing that all of this carbon (which is not strictly true) results from the destruction of hydrocarbon, and that this hydrocarbon is hepatic sugar, 813.26 grains of sugar would be converted, with a production of 415.65 grammes of alcohol, equal to 6415 grains. As this quantity represents the daily consumption or supply of alcohol, $\frac{6415}{1440} = 4.45$ grains of alcohol would pass through the lungs in one minute, and by Müller's estimate of the velocity of the blood through the lungs, we would have $\frac{4.45}{76,800} = 0.5794$ of a grain of alcohol present in the pulmonary blood during any given

Digitized by Google

minute. The mean of these three calculations is 0.6154 of a part of alcohol to 10,000 parts of pulmonary blood.

While, therefore, no less than 13.77 ozs. of alcohol is poured into the circulation within twenty-four hours—a quantity which represents all the carbonous constituents of the food capable of combining directly with oxygen, and which is evidently capable of subserving phenomena of primary importance, it would be impossible that more than .6 of a grain could exist in 10,000 parts of the blood passing through the lungs at any given time. This calculation assumes that the conversion of glycogen into glucose and the fermentation of this latter body are uniform ; but this is not strictly true, glycogenesis being stimulated by digestion and modified by various other conditions. Nor would it be reasonable to assume that either the hæmal fermentation of sugar or the oxidation of alcohol is uniformly active. What I desire to illustrate, however, by the foregoing calculations is, that whatever the importance of the glycogenic function may be, and although as much as 13¾ ounces of alcohol may be consumed in the blood in twenty-four hours, it cannot be possible to obtain by distillation more alcohol, upon an average, than this small quantity, viz.: .6 of a grain to 10,000 of blood, and this even under the supposition that oxidation is in complete abeyance, and that every atom of alcohol present is recovered. Consequently, all procedures addressed to this end must be surrounded with every possible safeguard, and large quantities only of organic material operated upon, if we desire to obtain measurable quantities of alcohol.

The only available method of separating alcohol from aqueous organic fluids is *distillation*. This is, moreover, the proper method for experimentation of this kind, on account of its prompt arrest by heat of fermentative changes going on in the material employed, and the completeness with which the alcohol is separated, this substance being entirely recoverable when one-half or a little more of the distilled liquid is brought over. I have distilled blood and the tissue of the lung, the liver, and the pancreas, under various conditions, but have always avoided any previous admixture of substances designed to separate or coagulate albumen, dissolve blood corpuscles, destroy sugar or arrest its fermentation. Tedious filtrations, unavoidable when anything of this kind is attempted, would occupy too much time, while the admixture of foreign substances would complicate the conditions of the experiment. I have regarded the greatest attainable simplicity as most certain to furnish the best results, but I may remark, in passing, that in the distillation of blood it may be advisable to add an equal weight of sulphate of soda, which has the property of crisping the albumen and of preventing it from becoming so solidly coagulated by heat as to risk an imprisonment of the vapors. For these reasons, except in a few instances, I have invariably submitted the organic matters to the distillatory procedure without prior admixture, a bath of chloride of sodium being interposed between the vessel containing them and the source of heat. In some cases, where the main facts had been already established in a suite of experiments, I have placed the vessel containing the organic material directly over the source of heat.

For the distillation of the organic matters, either copper balloons, holding from two to sixteen quarts, or cylindrical vessels horizontally placed, surmounted by a dome terminating in a copper pipe leading by a ground joint to a metallic worm, were employed. These vessels were set in larger ones containing the saline bath. None of these organic matters can be distilled in glass, for the vessel is infallibly broken by the generation of vapor between the wall of the vessel and the contained substance. The secondary distillations were always conducted in glass balloons varying from one fluid drachm to four quarts. The connections were made with glass tubing and corks. Liebig condensers, traversed by glass tubes of various sizes, were used, the tubes in all the later distillations reaching by a double bend, without joints, from the vessel submitted to heat to the centre of the receiving vessel, which was usually the balloon for the succeeding distillation. The sources of heat were charcoal and gas; no variety of coal oil was

Digitized by Google

kept or used in the apartment. From six to ten successive distillations by halves are necessary for the proper concentration and purification of the final distillate. The quantity of material operated upon varied from a pound or less to twenty or more pounds, each experiment occupying several days. Before every operation all the metallic and glass vessels were carefully washed in pure water, rinsed repeatedly in distilled water which had been evaporated to half its bulk, and then thoroughly dried over a furnace, air being driven into them with a bellows through a metallic or glass tube attached to its nozzle until all vapor whatever was entirely expelled. The vessels were then corked and set aside. All tubes, corks and funnels and the tubes of the condensers were carefully washed, rinsed as described, and thoroughly dried over the furnace, after carefully wiping with tissue paper and raw cotton. *In no reported experiment whatsoever* were *any* of these precautions neglected; the preparation of all necessary pieces of apparatus being made immediately before the institution of an experiment. For washing precipitates nothing but distilled water evaporated to *half its bulk* and freshly cooled was used. Chemically pure reagents only and different samples of these in successive operations were used. Solutions of salts were made with evaporated distilled water, and the solutions themselves were afterward filtered and boiled so as to drive off any volatile substance accidentally present. With such precautions I believe it was impossible that any alcohol or other volatile matters from without could by chance have become mixed with the distilled fluids or with the substances operated upon. All distillates not directly under manipulation were closely corked and locked up. Whenever a distillation was commenced it was invariably completed at the earliest possible period. If fluid or froth (under the conditions of these experiments possibly containing sugar) escaped from the balloon into the tubes or receiver, as occasionally happens when the balloon or cylinder is too full, the experiment was at once abandoned; and so, likewise, for accidental admixture of any sort whatsoever, *e. g.*, as when a few drops of the water used for cooling in the condensers accidentally fell into the distillate. If vapor escaped through a joint or passed uncondensed through the tubes, thereby involving perhaps a loss of alcohol, the experiment was abandoned. Transference of distillates from vessel to vessel was avoided as much as possible, and when filtrations were required the funnels were covered and the receivers, narrow-mouthed flasks, if possible, the ones to which heat was to be applied in the ensuing distillation. The balance was of the best German make, with knife edges resting on cornelian, and platinum weights, according to the metrical system. It turned readily with the ten thousandth part of a gramme when lightly loaded. The thermometers employed were carefully corrected by comparison with a standard one.

The final distillate seldom exceeding thirty grains in weight, and great accuracy for quantitative determinations being desirable, a special method, though similar in principle to that of the specific gravity, was employed as follows:—

A common thermometer with a small tube but medium bore, and a bulb about a quarter of an inch in diameter, was selected and cut off two and a half or three inches above the bulb, and the end ground and polished. The mercury having been expelled the weight of the bulb and tube was ascertained to four places of decimals. The bulb was then rinsed out two or three times with boiled distilled water, and, finally, when filled with distilled water by the procedure described below, brought very carefully to a temperature of 60° F., and its weight again determined. To take the specific gravity of a final distillate, the liquid in the bulb tube was entirely driven out by boiling, and its extremity immersed in the final distillate. After it had again filled itself by aspiration, its contents were a second time returned into the final distillate by heating the bulb, and it was allowed to fill itself again. This manœuvre was practiced three times successively in order to secure a uniform density in the fluid contained in the bulb tube and the bulk of the final distillate. Care was taken to expel all the bubbles

Digitized by Google

of air which are apt to linger in the bulb tube, and after their complete expulsion, the point of the tube was immersed in the distillate while the contents of the bulb tube were expanding; the tube was then allowed to cool and fill itself completely at the temperature of the air. A jar of water being provided with a thermometer suspended in it, the temperature of the water in the jar was brought to 62° F. if the air was below 60°, and to 58°, if the air was above 60°, the specific gravity being always taken for this latter temperature. The bulb tube completely filled with final distillate was now attached to a thread long enough to allow its bulb to rest in the same horizontal stratum of water as that in which the bulb of the standard thermometer was situated, when immersed in the water of the jar for the entire length of its stem except about a quarter of an inch. The thread being tied around the tube near its extremity, just before its final immersion in the jar, the bulb was quickly passed over a flame so that its contents might expand sufficiently to cause a minute globule of the liquid it contained to appear by expansion upon the surface of its polished end; to this, a small drop of the final distillate was added from the tip of a glass rod, and the bulb tube at once lowered to the proper depth in the jar of water. It was allowed to attain the temperature of 60°, as shown by the standard thermometer, a small quantity of distillate being again added to the globule on its tip if this appeared in danger of entire aspiration into the tube by contraction, as would be apt to happen when the air was above 60° and the water in the jar, in consequence, cooling through two degrees as explained. When the standard thermometer showed exactly 60°, the tube was quickly raised out of the water, the drop of distillate upon its extremity suddenly wiped off with a single application of a linen cloth, the tube itself carefully and quickly wiped, and immediately weighed. In this way the following weights were obtained: 1st, The weight of the empty tube; 2d, The weight of the tube filled with distilled water at 60°; 3d, The weight of the volume of distilled water contained in the bulb tube at 60°; and 4th, The weight of the volume of final distillate contained in the tube at 60°. These data enable us to determine the specific gravity of the final distillate by simple calculations. The weights of exactly equal bulks of very small quantities of liquid can be determined after this method with an accuracy equal to that of the specific gravity bottle, without appreciable loss by evaporation, or gain in weight in warm weather by the deposition of dewy particles, if the air within the balance-case be kept dry by chloride of calcium, as it always was during these determinations. The table of drink water furnishes the percentage by weight of alcohol in any mixture of alcohol and water, corresponding to its specific gravity, up to ten per cent., a range amply sufficient for experiments of this nature.

Organic matters should always be distilled over a bath of chloride of sodium; a bath of simple water is not sufficient. When heat is applied blood becomes solid, and vapors being generated between the wall of the vessels and the coagulated mass, a spongy texture is induced which is very unfavorable for the conduction of heat to the contents of the vessel. The material should be spread out in a shallow layer not more than one or two inches thick, and when the cylinder is used, it must be provided with a revolving spiral rake turning in a hole at one end which does not penetrate the end of the cylinder, while at the other its shaft passes through a tight stuffing box and also traverses the end-wall of the vessel containing the saline solution. This rake must be frequently rotated back and forth during the distillation, whatever be the contents of the cylinder. In my earlier experiments, copper balloons with flattish bottoms were employed, placed in larger copper vessels holding the solution of common salt.

The organic matter having been prepared, weighed, and its physiological and physical conditions noted, it is passed into the cylinder, to which the dome is carefully fastened. The bath is covered, and a hood adjusted reaching upward so as to enclose the dome and the bend of the eduction pipe. The steam of the boiling bath is thus

Digitized by Google

caused to ascend in contact with the dome and pipe, maintaining their temperature at a point which prevents premature condensation of the vapors arising from the substance distilled, which would otherwise trickle back, in the fluid form, into the mass of material. These first portions of vapor are always the richest in alcohol. Similar precautions must be observed, in all the succeeding distillations, against the premature condensation. The eduction tubes of glass should be heated with the flame of a Bunsen burner directed by a blow-pipe just before the distilland begins to boil, so that the first and most volatile portions may be separated from the distilled fluid at the earliest practicable period. After they have passed over, and watery drops appear, the bend of the tube from the boiling vessels toward the condenser should be wrapped in cotton wick.

The first distillate ought always to be equal in weight to one-half of the weight of the water contained in the matters distilled, and when fresh material is operated upon it comes over transparent or slightly cloudy, containing carbonate of ammonia and several volatile products. Its reaction is neutral or slightly alkaline. It must be distilled down by halves, and the distilland must be kept slightly acid with hydrochloric acid. After two or three distillations the distillate is treated with freshly calcined animal charcoal, and this must be repeated as long as the first drops of fluid distilled are not absolutely transparent. At about the sixth distillation some crystals of nitrate of silver are thrown into the distillate and dissolved. A decomposition is thereby caused of certain volatile substances which come over before the alcohol, and which behaves in all respects like aldehyde-acetic ether, and acetal. The distillate is again passed through animal charcoal and well washed with distilled water evaporated to half its bulk. This takes away almost all the peculiar odor which the distillates of animal substances always possess, nor is the volatile substance above described again seen.

When the organic matters have been allowed to remain for some days before distillation, inasmuch as putrefaction is beginning, carbonate of ammonia comes over in greater quantity and sulphureted hydrogen is found in the first distillate. In the process of rectification, therefore, acetate of lead must be added to decompose the sulphides, and hydrochloric acid as usual, animal charcoal and the nitrate of silver being used as before, the treatment of the distillate with these substances being repeated until the volatile product ceases to play along the tubes before the alcohol itself comes over, as it does in greatest quantity soon after the distilland begins to boil. Care must be taken to bring over a quantity of distillate in every operation at least equal to one-half the fluid submitted to heat, even where, by washing filters, a considerable quantity of water has been unavoidably added to the liquid in process of concentration, and that the first portions condensed in the tubes be not allowed to trickle back. After sufficiently numerous distillations, eventually in a very small apparatus, where the quantity distilled is often not more than one or two drachms, a final distillate is procured.

This final distillate should weigh from fifteen to thirty grains or more if alcohol is evidently very abundant, for its quantity can be approximately estimated by the eye as it plays along the tubes; its reaction must be neutral, and it must possess little or no odor. A peculiar odor always attaches, however, to these products of the distillation of animal substances, as Bernard has observed with regard to the alcohol obtained by distillation from decoctions of the liver of different animals.

The tests upon which I have relied for the qualitative determination of alcohol are the following:—

1. The optical appearance within the conducting tubes, during the final distillation, when the distilland began to boil.

Alcohol comes over in greatest strength during the first moments of a distillation, and if present, may always be seen to play in a mobile manner just in advance of the less volatile and more sluggish watery drops, as both progress rapidly along the con-

Digitized by Google

ducting tube when the distillaad just begins to boil. To observe this peculiar play of the successively condensing and vaporizing alcohol, it is essential that the dewy drops of volatile matters given off from the liquid, as well as of humidity deposited from the air of the flask in the bend and first portions of the conducting tube, should have been dissipated by a gentle heat from the flame of a lamp directed by a blow-pipe with large orifice. The conducting tube must now be allowed to cool again before general ebullition. This appearance is, of course, observable only between the balloon and the condenser. It is a trustworthy indication of even very minute quantities of alcohol, and should be studied by the distillation of mixtures of a few drops of alcohol with a quart of distilled water. During the final distillations this appearance becomes more and more distinct, and if not seen, alcohol is assuredly absent, at least in any determinable quantity. As I have said, in all these distillations some still more volatile substances are present, whose "play" in the tube precedes that of alcohol. Of these substances I believe *aldehyde* to be the most abundant, a body which ought to coexist with alcohol in organic fluids, as the result of the progressive oxidation of the alcohol. But after treatment of the later distillates with nitrate of silver, which decomposes aldehyde and acetal, the "play" due to these substances no longer occurs, while that proper to alcohol becomes more pronounced.

2. *Chromic Acid Dissolved in Sulphuric Acid.*—By adding a quarter of a gramme of crystallized bichromate of potassium to 100 grammes of concentrated and chemically pure sulphuric acid (as Bernard recommends from Leconte), the bichromate is decomposed, chromic acid liberated, and a rich, brownish-yellow liquid is obtained. When to any aqueous liquid containing the feeblest trace of alcohol an equal bulk of this reagent is added, much heat is evolved, the chromic acid is deoxidized by the alcohol and sesquioxide of chrome set free, imparting to the fluid its brilliant emerald green color. This is an exceedingly delicate and most reliable test for alcohol; indeed, the very best we have for distilled fluids. Nothing similar occurs with other volatile fluids obtainable from animal matters. Sugar, dextrine, gum, uric acid, urea, albumen and gluten equally effect this reaction, but they must be absent in distilled fluids. I have found that the oils distilled from petroleum reduce the chromic acid in this test, but they must be absent from a final distillate, such as I have described, and were never kept or used in the apartment where these experiments were conducted. Empyreumatic matters generated during the distillation of organic materials do not affect the test, even when strongest, as I have repeatedly and carefully determined.

3. *Inflammation of the Vapor of Alcohol in a Test tube upon Ebullition of the Final Distillate.*—This most striking and satisfactory test I have found applicable when not less than two or three per cent. of alcohol exists in an aqueous fluid; below these percentages the alcoholic vapor is too much diluted with steam to burn even at the first moments of ebullition. Some care is requisite in the use of this test. The quantity of the liquid should be about one-tenth of the capacity of the test tube; the flame over which the tube is held should be very moderate and steady, and should give but little light; the room should be darkened; a wax taper ready lighted should be held just above the upper edge of the mouth of the tube; the entire length of the tube should be heated before the liquid is made to boil, and the tube should be held in an oblique position and closely watched, as the least discomposure may prevent our observing the inflammation of the alcoholic vapor, which is sometimes a mere flash. When all is properly done, if alcohol is present in sufficient quantity, a flash of flame is seen to descend into the tube at the moment of ebullition, or a little after it, and if more abundant the alcoholic vapor continues, for a longer or shorter time, to burn continuously at the mouth of the test tube, with a characteristic flame, as the fluid in the tube is made to boil.

No other substance met with in the distillation of animal matters is capable of

Digitized by Google

inflammation in this way. The primary distillate, and all successive ones, until the final one is reached, constantly refuse to burn as above described. Some organic acids, as formic and acetic, are inflammable in the state of vapor, but only when pure or nearly so. The greater part of the final distillate obtained by these processes is water, and *acids* are not present, as the fluid is neutral or must be made so by the rules which I have followed.

Such, then, are the qualitative tests upon which I have relied. Whenever a final distillate, whose specific gravity is less than that of water, fulfills the requirements of these tests, alcohol unquestionably exists in it. By long experience in this kind of experimentation, during which I have had constantly recurring opportunities of observing the behavior of alcohol either normally present in the organic matter acted upon, or designedly mingled with it and finally recovered in the distillate, and moreover, by the determination of the ratio of expansibility of some strong distillates, the reasonable skepticism I long endeavored to maintain has been entirely dissipated.

The following considerations seem to me important in regard to the action of the tests employed, and better illustrate their cogency than a mere mention :—

1st. In the fermentation of liver decoctions, after about twenty-four hours at moderate temperatures (65° to 75°), so much alcohol is obtainable that it may be concentrated and purified, and recognized by its specific gravity and boiling point, etc., as well as by the taste and smell. When diluted with water the fluid burns in the test tube, in identically the same manner as the final distillates prepared from liver tissue, lung tissue, or blood set aside at ordinary temperatures for forty-eight hours or so, and also like those obtained from fresh organic matters. The vapor of all these fluids is inflammable and burns in a test tube at the first moments of ebullition, with a faint blue flame descending like a flash into the tube; they all contain the same substance which is known in the case of liver decoctions to be alcohol.

2d. Liquids obtained by distillation from fluids in which sugar has disappeared, as by direct fermentation with yeast, and which are, therefore, known to contain alcohol, are inflammable in the test tube. and when purified, color the chromic acid test green; liquids, therefore, obtained by distillation from fresh organic matters known (by direct testing) to contain sugar originally, and in which sugar has finally disappeared or diminished in quantity, and which are equally inflammable with the first named, and similarly decompose chromic acid, must likewise contain alcohol.

3d. The vapor of empyreumatic substances does not become inflamed upon ebullition of the fluid in which they may be present, in the test tubes, even when most abundant, as in the primary distillate, or after any degree of concentration. The distillates become more inflammable as the purification is more complete, which would not be the case were their inflammability due to empyreumatic contaminations, for successive digestions in the cold and with heat, with hydrochloric acid and animal charcoal, continually reduce any quantity of them originally present; and besides, when the saline bath is employed, no empyreuma is generated.

4th. Final distillates from the same kind of material have been found to be more inflammable in proportion to the quantities of organic matter operated upon. This has been especially marked, as will be detailed, when the product of several successive and similar operations have been mingled and concentrated together; which shows that the substance obtained and certified by the tests to be alcohol existed originally in the matters treated by distillation, and that its presence was not due to any incompleteness in the methods followed or to any accidental contamination.

5th. The specific gravity of the final distillates has always been found to be less than that of water, and the distillate is more inflammable in proportion to its lightness, and the depth of the tint produced with the chromic acid test, which should obviously be the case with alcohol.

Digitized by Google

6th. The quantity of the substance obtained has always been extremely small, which, on theoretical grounds, and from the calculations detailed, must necessarily be the case for alcohol.

7th. The expansibility of the final distillate is always greater than that of water, and is universally proportional to its specific gravity. In view, therefore, of these facts, and by the direct authority of the tests enumerated, of which the chromic acid test is obviously the only one of perfect scientific rigor, the presence of alcohol in the fluids obtained by distillation has been affirmed. For ultimate analysis in the combustion tubes enough alcohol might be obtained from liver tissue allowed to remain for forty to sixty hours at moderate temperatures, but what we wish to determine is the existence of alcohol in the fluids or solids within an hour or two after death, and before their normal temperature has appreciably declined. The quantity of alcohol existing in fresh liver tissue is so exceedingly small—only one part in 400,000 of liver tissue—that it is hopeless as the source of a sufficient quantity of this substance for the above purpose. The same may be affirmed of the lung tissue, but from the blood it might be possible to obtain an adequate quantity, especially as the material can be obtained in great abundance.

Having now described the methods followed in the distillation of organic matter, and the modes of determination employed, I proceed to devote a more particular attention to the experiments themselves. In some preliminary studies, I determined—

1st. That it is possible to distill such matters as liver tissue, blood and lung tissue, and to separate from the distillates obtained all substances foreign to the aim of the experimentation, so as to obtain a reasonably pure mixture of alcohol and water, the alcohol having been designedly added to the organic material.

2d. In a quantitative sense, that a few drops of alcohol thrown into a quart of distilled water could be entirely recovered, burning in the test tube, and reducing chromic acid.

3d. That when one part, by weight, of alcohol is poured into three or four thousand of fresh ox blood it can be likewise recovered.

4th. That in blood set aside for three or four days, to which alcohol is added in the same proportion, the substance is not destroyed, but can be almost wholly recovered from it by distillation.

Fortified with these results, I determined, in the first place, to investigate the conditions of Bernard's experiments upon the disappearance of sugar in newly drawn blood. Undoubtedly, if the disappearance was due to the transformation of the sugar into lactic acid, no alcohol could be recoverable. But, if the sugar had disappeared in consequence of its conversion by the alcoholic fermentation, inasmuch as in cool weather (53° to 65°) rectification advances but slowly, the alcohol, if formed, would remain, at least in part, unconverted into acetic acid, and should be recoverable by distillation.

I determined, therefore, to employ the mixed thoracic blood of the ox, which always contains sugar (from 0.00195 to 0.0074 parts to 1000 parts of blood, according to Carl Schmidt, quoted by Bernard), setting it aside after determining the presence of sugar at the temperature of the air until sugar had nearly or wholly disappeared. Upon such a basis the following experiments were conducted:—

No. 1.—622 grammes (1.37 lb. avoir.) of ox blood containing sugar by Fehling's test, as it does invariably, was agitated with air in a large flask, and set aside at a mean temperature of 62° F. for forty hours. At the expiration of this period sugar had almost entirely disappeared. The blood was distilled over the salt bath. The final distillate did not burn in the test tube, but struck a bright green tint with the chromic acid test. The quantity of alcohol present was 0.0162 gramme.

Digitized by Google

No. 2.—622 grammes (1.37 lb. avoir.) of similar blood, abundantly charged with sugar, was set aside as before, at a mean temperature of 60°F., for 90 hours. The blood was occasionally agitated with the air in the flask, which was renewed from time to time with a bellows. Although sugar was still traceable at the expiration of the above period of time, the blood was distilled over the salt bath. The final distillate weighed 1.2940 gramme. It struck a bright green with the chromic acid test but did not burn in the test tube. The quantity of alcohol was 0.0155 gramme.

No. 3 —930 grammes (2.05 lbs. avoir.) of similar blood was introduced into a large glass-stoppered flagon. The blood contained sugar, upon examination. The vacant two-thirds of the flagon was filled with carbonic acid, the blood well agitated with it becoming almost black. The flagon being securely stoppered, was carefully sealed with wax, and the blood set aside at a mean temperature of 66° F. for 42 hours. Though sugar was still traceable, it was distilled over the bath of common salt. The final distillate weighed 1.6173 gramme; alcohol burned in a lively manner in the test tube, and struck an emerald green hue with the chromic acid test. The quantity was 0.0647 gramme.

No. 4.—1120 grammes (2.46 lbs. avoir.) of similar blood, containing sugar by examination, was introduced as before with a large stoppered flagon, securely shut off from the air, and set aside at a mean temperature of 60° F. for 72 hours. The blood was well shaken with the air in the flagon from time to time, but no fresh air was allowed to enter. After the above lapse of time, the blood was distilled over the bath of common salt. The final distillate weighed 1.4880 gramme. The vapor of alcohol burned in a lively manner in the test tube, and the fluid reduced the chromic acid test as before. The quantity of alcohol was 0.0446 gramme.

No. 5.—930 grammes (2.05 lbs. avoir.) of similar blood, containing sugar by examination, was well agitated with air and set aside at a mean temperature of 58° F. for six hours. It was then distilled over the bath of common salt. The final distillate weighed 1.0352 gramme. The vapor of alcohol burned in a lively manner in the test tube, and the chromic acid test was reduced as before. The quantity of alcohol was 0.0414 gramme.

In these experiments the blood was distilled before the establishment of putrefaction, only a faint odor being emitted; it changed in color from red to deep black within two days. The mean quantity of blood operated upon was 845 grammes; the mean temperature was 61° F.; the mean duration of the experiments was 62 hours; and the mean quantity of alcohol obtained was 0.0365 gramme. Alcohol was obtained in every case, although the quantities of blood employed were comparatively small; they were, however, on an average, thirty-five times as great as those experimented upon by Bernard (24 grammes).

In blood left to itself, therefore, sugar is not destroyed, at a mean temperature of 61° F., by transformation into lactic acid, but by the alcoholic fermentation, and moreover, for the occurrence of this fermentation under the conditions named, *yeast of beer* is not indispensable.

Having determined these points, I proceeded to inquire whether the hepatic glucose, so abundant in exsected liver tissue, is similarly decomposed by the alcoholic fermentation, at ordinary temperatures. I therefore experimented as follows :—

No. 6.—750 grammes (1.65 lb. avoir.) of fresh ox liver was comminuted in a meat mincer (used only for these purposes, well washed and dried at a high heat over the furnace) and set aside at a mean temperature of 75° F. for 90 hours. At the end of this period sugar was no longer discoverable, and putrefaction was fairly established. While making a decoction of a portion of the tissue in testing for sugar, acid vapors were given off, the mass itself being strongly acid and effervescing vigorously with bicarbonate of soda. This free acid, capable of passing off as vapor during the first moments of ebul-

Digitized by Google

lition, and strongly reddening litmus paper, was undoubtedly acetic acid. The mass of liver was now distilled over the salt bath and yielded 0.3235 gramme of alcohol. The vapors of alcohol burned continuously at the mouth of the test tube upon ebullition of the final distillate, which struck a vivid emerald green tint with the chromic acid test. Much of the alcohol, indeed nearly all of it, must have been lost by acetification, on account of the high temperature at which the liver tissue was set aside.

No. 7.—190 grammes (0.42 lb. avoir.) of fresh ox-liver tissue was laid aside, sugar being present by examination, without comminution, at a mean temperature of 60°, for 120 hours. When distilled over the bath of common salt, it yielded a large quantity of alcohol, the ultimate distillate, when boiled in a test tube, giving off vapors which burned continuously at the mouth of the tube as long as heat was applied, but little of the alcohol formed having been lost by acetification.

The sugar, therefore, of liver tissue is largely converted into alcohol by the maintenance of a morsel of liver at a temperature of 60° to 70°. At the lower temperature decomposition is longer postponed and acetification less active, so that larger proportional quantities of alcohol are recoverable. I have already shown with what tumultuous violence hepatic glucose is fermented in a mass of liver submitted to its somatic temperature. The alcoholic fermentation advances, however, at temperatures even as low as 45° F., but a rise of temperature very rapidly exalts the inherent tendency of the alcohol formed toward oxidation, especially in the presence of a ferment, so that at the somatic temperature, as I have proved by direct experiment, the alcohol is acetified nearly as fast as it is formed.

I proceeded next to inquire whether the lung tissue might not ferment alcohol when left to itself at moderate temperatures. As the blood contained in the pulmonary capillaries is derived from the saccharine blood of the right ventricle, sugar being borne constantly from the liver through the heart into the lungs, it must necessarily exist in the lung capillaries. Bernard formerly affirmed that sugar disappeared *totally* in the lungs in the fasting state, and in greater part during the digestive overflow; nevertheless, he emphatically denied that the lungs contain sugar. "We boil," says he, "and treat with the test liquids, decoctions of the spleen, kidney, pancreas, lung, muscular tissue and brain, derived, like the portions of liver already found to be so rich in sugar, from the ox. Not one of these decoctions reduces the test liquid; not one of them ferments with yeast. We may, therefore, boldly declare that no organ of the body, except the liver, contains sugar in the physiological state" (*Loc. cit.*, p. 53). I have already noted that in his later researches Bernard modified this statement, admitting that a certain small portion of sugar is normally present even in the systemic venous blood. But his original declaration was inherently contradictory to his own hypothesis, for if it be affirmed that sugar is destroyed in the lungs, it must of necessity exist there, although the lungs be the site, mainly, of the processes in virtue of which it disappears before reaching the left side of the heart. Sugar, therefore, or its representative, alcohol, must exist in the lung capillaries, at least transiently. I therefore determined to submit ox-lung tissue to the same conditions of time and temperature under which the blood and liver tissue had been examined. The following four experiments exhibit the result of this inquiry:—

No. 8.—500 grammes (1.12 lb. avoir.) of fresh lung tissue from the ox was comminuted to the size of an acorn, with scissors. The pleural portions of the tissue alone were used. All vessels and bronchial tubes, as well as fluid blood, were rejected. The lung tissue was set aside at a mean temperature of 66° for 72 hours. It was then distilled over the bath of common salt. The vapor of alcohol burned in a lively manner upon ebullition of the final distillate in a test tube. The quantity of alcohol was 0.0323 gramme.

No. 9.—750 grammes (1.65 lb. avoir.) of fresh lung tissue from the ox, similarly

Digitized by Google

prepared, was set aside at a mean temperature of 66° F. for 96 hours. At the end of this period the odor exhaled was faintly disagreeable. It was then distilled over the salt bath. Alcohol was seen to pass over during the final distillation, and when the final distillate was boiled in a test tube, burned vigorously at its mouth. Its quantity was 0.0485 gramme.

No. 10.—2180 grammes (4.80 lbs. avoir.) of fresh ox-lung tissue was set aside at a mean temperature of 76°, for 17 hours. It was then distilled over the bath of common salt. Alcohol was seen to pass over during the later distillations. Its vapor burned in the test tube upon ebullition of the final distillate.

No. 11.—5160 grammes (11.37 lbs. avoir.) of similar tissue was set aside at a mean temperature of 76° F. for 65 hours. At the end of this time it was distilled over the bath of common salt. The final distillate weighed 3.8820 grammes, and contained 0.1640 gramme of alcohol. The vapor of alcohol burned continuously at the mouth of the test tube after removal of the taper, upon ebullition of the final distillate, which also struck a vivid green tint with the chromic acid solution.

These results were in complete accord with those obtained by distillation of the blood and liver tissue; alcohol existed in lung tissue which had been submitted to ordinary temperatures during periods of time varying from 17 to 96 hours. It is certain that sugar had existed at some period antecedent to the distillatory process in the lung tissue employed, although this period may have been previous to the death of the animal or have been extended somewhat afterward. From the fact that Bernard failed to find notable quantities of sugar in the lungs, and that by my own experiments alcohol can be obtained from these organs, as well as by considerations adduced further on, it is reasonable to conclude that the sugar conveyed to the lungs is promptly decomposed there by the alcoholic fermentation. In this way it becomes possible to reconcile Bernard's declaration that sugar does not exist in the lungs, or, at least, cannot be demonstrated in the lung tissue of the ox, some time after death, with his inferential statement that this substance is mostly destroyed in the pulmonary transit.

The conditions of the disappearance of sugar in the blood, the liver, and the lung tissue, at ordinary temperatures had now been determined, and it had been established that this disappearance was effected through the mechanism of the alcoholic fermentation, presumably with a concurrent acetification, which, however, could be actually observed only in the liver tissue, normally so much richer in sugar than the other matters operated upon. While by no means denying the possibility of the occurrence of the lactic acid fermentation in these substances, I cannot believe that alcohol would have been recoverable from them, if the lactic acid fermentation had occurred as a continuation of the normal processes, while the converse of this might readily happen, as the alcoholic fermentation antedates the lactic, this latter fermentation being accompanied usually with changes initiatory of putrefaction. Thus in the preparation of bread, an alcoholic fermentation is first kindled in the dough by the yeasting, the carbonic acid evolved causing the dough to swell. If the temperature be too high, acetification of the alcohol very quickly supervenes, the acetic acid formed at the expense of the alcohol liquefying the gluten; so that the carbonic acid is allowed to escape, the dough falls and turns sour. As yet, however, there is no lactic acid formed; this substance appears later, when the dough begins to putrefy, and is found to be associated with butyric acid.

The foregoing experiments were instituted as the first stage of an inquiry as to the normal occurrence of the alcoholic fermentation within the economy of living animals. The physiological mind of the present day is fully prepared to admit that the fact of somatic death, as I have already hinted, does not necessarily arrest all nutritive transformations occurring in the several organs and fluids, some of which are independent of the circulation, of innervation and of respiration, although through certain mechanisms controlled by these functions. The converse of this proposition, properly guarded,

Digitized by Google

ought likewise to be true, viz., that the chemico-vital processes observable within limited periods after death, may, with prima facie reason be supposed continuous with those occurring during life. For, indeed, the moment of death is a very uncertain one, and the period at which the organs and fluids become unable to continue or to resume their normal trains of action varies greatly in the solids and liquids, as it does also in different animals and even individuals. It is reasonable, consequently, to infer that the modifications of sugar observed soon after death, or even before the establishment of putrefaction, are continuations of the modes of action normal to the organism, or at least chemically allied to them. We may conclude, in accordance with such an inference, and with the satisfaction of knowing that it equips us with the best attainable working hypothesis, that the liver evinces its remarkable power of causing the sugar it contains to ferment into alcohol, in what I have termed its "intrinsic fermentation," because it possesses the same faculty during life; that sugar disappears in blood within a few hours after death by a change into alcohol, because it does so normally within the system, and that alcohol is obtainable from lung tissue not yet in process of putrefaction, because it naturally exists in the blood passing through the lungs. I engaged, therefore, as follows, in this important question, by submitting the above hypotheses to the test of direct experiment.

No. 12.—750 grammes (1.65 lb. avoir.) of normal lung tissue was comminuted in a meat mincer and raised over the salt bath to 212° F., in 90 minutes after the animal was struck down. Alcohol was seen to pass over during the later distillations. The final distillate burned within the test tube upon boiling. The quantity of alcohol was 0.0259 gramme.

No. 13.—870 grammes (1.92 lb. avoir.) of ox-lung tissue, while still quite warm, was comminuted as above, and raised over the salt bath to 212° F. in 55 minutes after the animal was struck down. During the later distillations, alcohol was seen to pass over. The final distillate weighed 0.8411 gramme, and upon ebullition in a test tube the vapor of alcohol burned satisfactorily. The quantity of alcohol was 0.0168 gramme.

No. 14.—1240 grammes (2.75 lbs. avoir.) of ox lung tissue, comminuted to the size of a pigeon's egg, was raised over the salt bath to 212° F. in 75 minutes after the animal was struck down. During the final distillations alcohol was seen to pass over. The final distillate weighed 0.6470 gramme, and contained 0.0453 gramme of alcohol. It struck a vivid emerald green tint with the chromic acid test. It was not burned in the test tube, being reserved for demonstration before the Elliott Society of Natural History, of Charleston, South Carolina, on May 15th, 1858.

The total quantity of lung tissue operated upon was 2860 grammes, or 6.32 pounds (avoir.). The total quantity of alcohol obtained was 0.0880 gramme. Alcohol can be recovered from fresh lung tissue, though only in exceedingly minute amount, if the weather is warm and the lung tissue allowed to retain the heat proper to the animal body. The lung tissue contains so much air that, in conjunction with heat, all the conditions are present necessary for the most rapid acetification. In warm weather, therefore, but little alcohol can be recovered from lung tissue, but if the air be cold, so that the lungs are rapidly lowered in temperature to the neighborhood of 50° or 60°, the amount of alcohol recoverable is proportionately larger. On account, however, of the volume of the lungs in proportion to their weight, they contain but little blood, and large masses must be distilled to obtain very moderate quantities of alcohol. Lung tissue is far easier to distill than blood, as it shrinks on heating to 212°, and when cut in strips from the peripheral portions of the organs, not more than half an inch thick, readily yields up its vapors. But on account of the great variability of the density of the lung tissue, and the small quantity of blood it contains, it is necessary to employ very large quantities, to obtain only minute quantities of alcohol, even in cold weather.

Digitized by Google

However, as the fermentation process advances everywhere in the blood, and as this fluid represents several times its weight of lung tissue for the purposes of these experiments, in order to obtain alcohol in larger quantities I determined to submit the slaughter-house blood itself to distillation, procured with the least delay possible, and with every precaution against the admixture of foreign matters, except, as will be detailed, when designedly added.

The mixed thoracic blood of the ox had been already employed for observing the disappearance of sugar at somatic and atmospheric temperatures. By distillation, alcohol was obtained from blood of this kind which had been set aside at ordinary temperatures for two or three days; it remained to inquire whether it could be procured from perfectly fresh ox blood. This mixed blood can be obtained in large quantities, from ten to twenty pounds escaping from each slaughtered animal. In the process of slaughtering the animal is first struck upon the head, and soon after it has fallen the throat, just above the sternum, is longitudinally incised, and the large venous trunks opened with the point of the knife. By the Jewish method the animal is not struck down, but fastened to the floor by means of a windlass, and the throat transversely incised just behind the angle of the jaws. Bernard details the French method as follows: "In ox blood obtained from the slaughter houses, sugar is always present while the blood is fresh, and for the following reasons: In order to bleed (exsanguinate) the oxen just struck down, the butcher plunges the knife even into the right auricle; the blood escaping is therefore partly derived from the hepatic veins; and if we notice, moreover, that in order to evacuate the blood that the animal contains, the butcher applies his foot strongly to the region of the liver, so as to press as much as possible of it out of that organ, it can be understood how it happens that the blood issuing from the neck wound, thus commingled with that from the hepatic veins, contains notable quantities of sugar; and whenever I have examined blood from the slaughter houses, I have determined the presence of sugar in it. Besides, it must be observed, that the animals may be in digestion, a condition which augments the quantity of sugar in the organism, and that they struggle violently if the throat is cut without a previous blow upon the head." From extended observation, I can likewise affirm that sugar is always present in such blood, though in variable quantity, the variations being caused by different methods of slaughtering. When the internal jugular veins are both fully divided, and especially when the innominate venous trunks at the base of the neck are opened, which is the usual practice in this country, the maximum amount of sugar is present in the escaping blood. While conducting the subjoined experiments the slaughtering was frequently superintended in person, the blood being received into a flat cylindrical vessel of tinned iron, constructed for the purpose, twelve inches in diameter, and four and a half inches in altitude. In its upper side, an orifice four inches wide, with a low rim, and fitted with a cover, was placed as near the edge as possible. Handles were attached to the vessel, so that the wide mouth could be applied directly to the wound in the neck of the animal, as the butcher made the opening into the great veins. The blood was thus caused to pass directly from the animal into the receiver, no intermediate vessel being used. The receiver itself was washed with evaporated distilled water and dried over the furnace, all vapor being driven out of it with a bellows, while still hot, just before it was sent to the slaughter house.

All the apparatus being prepared, as already stated, the temperature of the blood when received was noted, and it was passed out of the receiver through a wide-mouthed funnel into the distilling balloon or cylinder, and as soon as the conducting tube was securely adjusted the saline bath was poured on.

The following ten experiments exhibit in detail the results obtained in the distillation of blood fresh from the animal:—

No. 15.—697 grammes (1.53 lb. avoir.) of ox blood was raised to 212° F. in 60

Digitized by Google

minutes after the animal was struck down. When received the blood was quite warm, coagulated and of a dark red color. Alcohol was seen to pass over during the final distillations, and its vapor burned in the most satisfactory manner in the test tube. The final distillate assumed a bright emerald-green tint with the chromic acid test. The quantity of alcohol obtained was 0.0650 gramme.

No. 16.—9734 grammes (21.46 lbs. avoir.) of ox blood was raised to 212° in 56 minutes after the animals, two in number, were struck down. The temperature of the blood when received was 101°. During the final distillations alcohol was seen to pass over. The final distillate weighed 0.8416 grammes, and struck the usual green tint with the chromic acid test. The quantity of alcohol obtained was 0.0198 gramme.

With the object of counteracting or abolishing post-mortem oxidation, and thus preventing a possible loss of alcohol by its transformation into aldehyde and acetic acid at the high temperature of the blood, it was determined to add a certain quantity of a solution of sulphureted hydrogen to the blood as it passed into the receiver. This solution was made by passing a stream of *pure* and *dry* sulphureted hydrogen through distilled water previously evaporated to half its bulk.

No. 17.—9137 grammes (20.10 lbs. avoir.) of ox blood, from two animals, was raised to 212° in 70 minutes after the oxen were struck down. 250 grammes of a saturated solution of sulphureted hydrogen was added to the blood during its collection. The blood coagulated loosely and became very dark or even black. Notwithstanding the addition to the blood of so much liquid at about 70°, its temperature when received was 100.3°. Alcohol was seen to pass over during the later distillations. The final distillate weighed 1.6218 gramme, and struck the usual green tint with chromic acid. The vapor of alcohol burned within the test tube and at its mouth when the final distillate was boiled. The quantity of alcohol was 0.0605 gramme.

No. 18.—9286 grammes (20.36 lbs. avoir.) of ox blood, from two animals, was raised to 212° in 77 minutes after the animals were struck down. During the collection 250 grammes of a saturated solution of sulphureted hydrogen was added to it, as before. The blood coagulated loosely and was nearly black. Its temperature when received was 99.5°. Alcohol was seen to pass over during the later distillations. The final distillate weighed 3.6130 grammes, and struck the usual tint with the chromic acid test; when boiled in a test tube the vapor of alcohol burned within it satisfactorily. The quantity of alcohol obtained was 0.0444 gramme.

No. 19.—8988 grammes (19.81 lbs. avoir.) of ox blood, from two animals, was raised to 212° in 60 minutes after the animals were struck down. 250 grammes of the usual solution of sulphureted hydrogen was added, as before. The temperature of the blood when received was 99°. Alcohol was seen to pass over during the later distillations. The final distillate weighed 2.6092 grammes. The vapor of alcohol burned within the test tube in a most satisfactory manner upon ebullition of the final distillate, which gave the usual reaction of alcohol when the chromic acid test was added. The quantity of alcohol was 0.1357 gramme.

No. 20.—8854 grammes (19.52 lbs. avoir.) of similar blood, from two animals, was raised to 212° F. in 60 minutes after the oxen were struck down. 255 grammes of the solution of sulphureted hydrogen was added, as before. The temperature of the blood when received was 98° F. Alcohol was seen to pass over in the later distillations. The final distillate weighed 1.7320 grammes, and gave the reaction of alcohol with the chromic acid test. The quantity of alcohol obtained was 0.0760 gramme.

No. 21.—9423 grammes (20.77 lbs. avoir.) of similar blood, from two animals, was raised to 212° in 45 minutes after the oxen were struck down. No sulphureted hydrogen was used in this experiment. The temperature of the blood when received was 96°. Alcohol was seen to pass over. The final distillate weighed 1.8722 gramme, and gave the alcoholic reaction with the chromic acid test. Upon ebullition, the vapor of

Digitized by Google

alcohol burned within the test tube, and remained burning at its mouth for some seconds after the removal of the taper. The quantity of alcohol was 0.0708 gramme.

No. 22.—9122 grammes (20.11 lbs. avoir.) of similar blood from two animals was raised to 212° in 61 minutes after the animals were struck down. The temperature of the blood when received was 98°. No sulphureted hydrogen was used in this experiment. Alcohol was seen to pass over during the later distillations. The weight of the final distillate was 0.9552 gramme. The chromic acid test gave the emerald-green reaction of alcohol. The vapor of alcohol burned satisfactorily in the test tube upon ebullition of the final distillate, remaining inflamed at the mouth of the tube after the removal of the taper, and being relighted several times, as the liquid in the tube was made repeatedly to boil. The quantity of alcohol obtained was 0.0350 gramme.

In the preceding eight experiments, 1500 grammes of first distillate was brought over in each case from about 9000 grammes of blood. As a portion of alcohol might still remain in the organic material, all the alcohol of a spirituous liquid being driven over only by the evaporation of somewhat more than half its bulk, it was determined to bring over 4650 grammes of first distillate in a series of operations. To do this, about seven hours of distillation was necessary.

No. 23.—27.330 grammes (60.25 lbs. avoir.) of ox blood was distilled in three operations, in portions of about 9000 grammes each, the respective intervals between death and elevation to 212° being 50 minutes, 45 minutes and 50 minutes. About 4700 grammes of first distillate in each case was brought over. The first was purified and concentrated to 120, 130 and 500 grammes. The three fluids were then mixed and treated as usual. The conjoint final distillate weighed 10.6883 grammes. It struck a bright green tint with the chromic acid test; when boiled in a test tube the vapor of alcohol burned satisfactorily, remaining lighted after the removal of the taper. Not more than a gramme was used; the remainder was set aside. The quantity of alcohol was 0.2928 gramme.

No. 24.—36.300 grammes (80.03 lbs. avoir.) of ox blood was distilled in portions of about 9000 grammes each. The interval between death and elevation of the blood to 212° F. was in each case 55 minutes, 56 minutes, 50 minutes and 45 minutes. 4900 grammes of first distillate was brought over in each operation. Each first distillate was purified and distilled down to about 120 grammes. The four resulting fluids were then mixed, as in the last experiment, and concentrated by distillation. The conjoint final distillate weighed 14.0606 grammes, manifesting the characteristic reaction of alcohol with the chromic acid test; the vapor of alcohol burning, as before, in the test tube and at the mouth. The quantity of alcohol obtained was 0.5652 gramme. What remained of the final distillate of these two last experiments (Nos. 23 and 24) were mixed and set aside, a few crystals of nitrate of silver having been thrown in for the purpose of destroying any remaining traces of the volatile body supposed to be aldehyde, and kept in the dark for eleven months. At the end of this time the liquid contained a slight black precipitate, owing, doubtless, in part to reduction of some dissolved nitrate by diffused light, and upon concentration by two distillations to a specific gravity of 0.986 (which is specific gravity favorable to the satisfactory application of the test), the vapor of alcohol burned at the mouth of the test-tube and within it, as before, though with far greater intensity.

The mean quantity of alcohol obtained for 10,000 parts of blood when sulphureted hydrogen was not added at the moment of its collection, was, 0.0567 of one part; and when sulphureted hydrogen was so used, 0.1028 of one part, or about twice as much. The assumption, therefore, that this body arrested or impaired the oxidative processes by which alcohol was destroyed in the blood operated upon after its escape from the vessels of the animals, appears to have been confirmed. Carbon protoxide should act in a similar manner, as, according to the investigations of Bernard, it renders the red

Digitized by Google

corpuscles of the blood incapable of absorbing or of parting with oxygen, but on account of the difficulty of using it at the slaughter house, due to its gaseous form and slight solubility in water, sulphureted hydrogen was preferred. In all the later experiments the solution of this latter substance was placed in the blood receiver, so that the blood was immediately subjected to its influence as it gushed from the incised veins.

The mean quantity of alcohol obtained in these ten experiments was 0.0797 gramme to 10,000 grammes of blood. Alcohol is always present in this mixed blood. I have never failed to find it. Its quantity is necessarily variable; for, as I have explained, the blood cannot be supposed to be derived at all times from the same vessels, owing to variations in the process of slaughtering by which the quantity of sugar and its resultant alcohol must be notably affected. I have not found it practicable, so far, to surround the collection of this blood with any physiological conditions whatsoever;it must be used as it pours from the neck of the animal. Practically, however, as the slaughtering is usually performed by experienced persons, in any one establishment, in the same way, so as to exsanguinate the animal as speedily and as completely as possible, considerable uniformity ought to pertain to parcels of the blood collected at the same place.

Having now determined the normal existence of alcohol both in the lung tissue and in the mixed blood of the slaughter house, I determined to search for it in the tissue of the liver. Here a new difficulty arose. The lungs, being exceedingly vascular, and collapsing by their proper elasticity after the thorax is opened, necessarily imprison a certain quantity of blood retained in their capillary vessels. It was from this capillary blood that the alcohol obtained by the distillation of the lung tissue must have been derived. The liver, however, is vastly less vascular than the lungs, and is very thoroughly drained of its blood, not only by the practice of the French butchers, at least, of applying the foot to the hepatic region, but also because the larger trunks of the hepatic veins are not occluded by the weight of the organ when excised, their walls being directly continuous with the hepatic stroma; their channels, consequently, remain open for the escape of the blood upon exsection or slicing. It is from this blood of the hepatic veins, however, that we should expect to obtain alcohol, on account of the known presence of sugar in it. Nevertheless, as the smaller vessels of the hepatic venous system and their rootlets in the acini must retain some blood, and as this is normally saccharine, though far less so during life or immediately after death than Bernard originally believed, if the alcoholic fermentation is proper to the hepatic veins and begins to advance in their blood as soon as sugar is transuded into it by the cell action of the liver, it might be possible to obtain some trace of alcohol by distillation of the hepatic substance. Impressed with such considerations, I made the following experiments:—

No. 25.—2985 grammes (6.58 lbs. avoir.) of ox-liver tissue was raised to 212° in 40 minutes after the animal was struck down. 1400 grammes of first distillate was brought over. The final distillate weighed 1.1050 gramme; its specific gravity was 0.9989, corresponding to 0.0064 gramme of alcohol. This final distillate was too dilute for the inflammation of the vapor of so small a quantity of alcohol in the test tube, but it was colored a bright green by the chromic acid test.

No. 26.—The entire liver of a dog, weighing 650 grammes (1.43 lb. avoir.), the animal having been killed by section of the rachidian bulb, was raised to 212° in seven minutes after the sacrifice of the animal. The final distillate weighed 0.860 grammes and struck a bright emerald-green tint with the chromic acid test, but was too dilute for the inflammation of the vapor of the very small quantity of alcohol present in the test tube. This quantity of alcohol was 0.0024 gramme.

These results determined me to employ masses of liver tissue many times greater than those distilled in these last two experiments, as follows:—

Digitized by Google

No. 27.—18,200 grammes (40.12 lbs. avoir.) of fresh ox-liver tissue was distilled in three portions:—

No. 1.—6000 grammes was raised to 212° in 35 minutes after the animal was struck down, 1550 grammes of first distillate being brought over.

No. 2.—6000 grammes was raised to 212° in 40 minutes after the animal was struck down, a like quantity of first distillate being brought over.

No. 3.—6000 grammes was raised to 212° in 45 minutes after the animal was struck down, an equal quantity of first distillate being brought over.

These three distillates were mixed and concentrated. The final distillate weighed 1.2304 gramme. It struck a vivid green tint with the chromic acid test. The vapor of alcohol burned most satisfactorily and repeatedly, upon intermissions of the boiling, when the final distillate was tested. The quantity of alcohol obtained was 0.0324 gramme.

The ratio of the alcohol obtained in Expt. No. 25 to the liver tissue employed was one part to 466,406; in No. 26, one part of alcohol to 270,000, and in No. 27, one part to 555,555 of liver tissue. These results are so concordant that it is impossible to doubt their accuracy, and illustrate the extraordinary efficiency of distillation when conducted with the care taken in the experiments reported in the present paper. Indeed, my confidence in the quantitative accuracy of the methods followed is such that all of the results may be numerically compared with each other. Even in the present connection I think that a comparatively greater quantity of alcohol was obtained in No. 26, where the dog's liver was distilled, because the exsanguination of the ox liver had been more complete, the liver of the dog having been rapidly cut up into several pieces and cast into the copper balloon, while the ox liver had been previously drained of its blood much more perfectly.

Alcohol, therefore, undoubtedly exists in the stroma of the liver, even in animals slaughtered by hemorrhage, and within as short a period of time as it is practicable to bring the substance to the boiling point, the shortest of these intervals having been only seven minutes. It must then be conceded that the alcoholic fermentation progresses in the liver during life.

By comparing the results of distillation of liver tissue with that of the lung tissue an interesting fact is brought into view. The ratio of the alcohol recovered in Expts. No. 25, No. 26 and No. 27, as a mean, to 10,000 parts of the liver tissue operated upon, was 0.0188 of a part to 10,000 of the liver tissue, and in Expts. No. 12, No. 13 and No. 14 I have found that the ratio of the alcohol obtained to the weight of lung tissue operated upon was 0.3084 of a part to 10,000 parts of this latter tissue employed. Fresh lung tissue, consequently, contains somewhat more than sixteen times as much alcohol as liver tissue. As in both cases the alcohol can only exist in the capillary blood left in the organs after exsection and drainage, the lung tissue must retain a far greater quantity of such blood than the liver, owing to its vascularity, and especially to its elasticity, whereby the blood at least is imprisoned. I judge this to be the case from observing that when we cut a piece of fresh lung tissue and observe the surface, although the cut seems dry when made, within a few seconds, or a minute, the cut surface is moist and bloody. I do not think, however, that a difference of vascularity alone adequately accounts for the great difference observed between the liver and the lungs as to the quantity of alcohol obtainable from them; but feel disposed to attribute the small quantity found in the liver substance to the probable inauguration of the alcoholic fermentation in the blood of the hepatic veins, which contains much sugar and but little alcohol, while the lungs contain much more alcohol comparatively, and according to Bernard, little or no sugar.

The sugar of the liver is thus to be regarded as fermented into alcohol and carbonic acid for the most part in the transit between the liver cells and lung capillaries, but as

Digitized by Google

alcohol must be the unfailing attendant of sugar in the blood, and as we know that sugar is disseminated throughout the system during the digestive overflow, and also, to some extent, at all times, as Bernard finally admitted, we should find small quantities of alcohol in the systemic blood of animals in digestion.

To determine this point I made the two following experiments :—

No. 31.—1368 grammes (3.01 lbs. avoir.) of the pancreatic tissue of the ox was rapidly and carefully isolated. The animals from which it had been derived, being ruminants, were, of course, more or less in digestion at all times, access to fodder and a field of grass being allowed to them. The tissue was raised to 212° in 60 minutes after the animals were struck down. The final distillate weighed 1.0246 gramme and struck a vivid green tint with the chromic acid test. The quantity of alcohol was very small and was not estimated, nor did its vapor burn in the test tube.

No. 32.—280 grammes (0.63 lb. avoir.) of blood was drawn from the cephalic extremity of the jugular vein of a small bitch, and raised to 212° in ten minutes after the vein was opened. The animal was in full digestion. The final distillate weighed 0.6000 gramme, and struck a bright green tint with the chromic acid solution. The quantity of alcohol was minute and could not be inflamed in the test tube, nor was it estimated, as its specific gravity was too low.

Alcohol was thus found to exist in the capillary blood of the pancreas, and in the centripetal venous blood in very minute quantity, according to the theoretical provision which governed the experiments. We must conclude that whenever we find sugar in the fluids of living animals we must likewise find alcohol in proportionate quantity, the alcoholic fermentation progressing everywhere in the circulation, though most active between the liver cells and the left side of the heart.

In accordance with the foregoing observations, direct experiment should confirm the hypothesis of a hæmal oxidation of alcohol. I will premise, however, by remarking that processes instituted by art, in imitation of natural ones, and especially of those taking place in the animal body, can be conducted under conditions only roughly similar to those imitated, even when fortunately planned and skillfully executed; numerous difficulties embarrass the inquirer and tend to complicate his results. The attempt to oxidize alcohol in blood out of its natural channels can, of course, merely illustrate the direction of the normal processes, as it is evident that for many reasons the oxidative powers of the blood must be very gravely impaired by removal from the vessels, and that no degree of agitation with atmospheric air can compare in efficiency with the marvelous exposure to oxygen realized by the pulmonary structure. Nevertheless, the following experiments, being conducted with great care, may be found to possess a certain degree of cogency:—

No. 28.—Into 1000 grammes (2.24 lbs. avoir.) of fresh defibrinated ox blood 0.3882 gramme of absolute alcohol was dropped. The blood was placed in a glass flagon holding three quarts, and the flagon, with its contents, was maintained for two hours at a temperature of 98°, by a water bath, regulated by a thermometer. The flagon was frequently removed from the bath, and its contents thoroughly commingled by agitation with the superjacent air, this air being removed every five minutes by a bellows. The blood assumed and maintained a florid arterial hue. It was then distilled over the bath of common salt. The final distillate weighed 1.9733 gramme, reacting with the chromic acid solution as usual, and when boiled in a test tube giving off vapors which burned in the manner of alcohol. The quantity of alcohol recovered was 0.0258 gramme.

I had previously ascertained that when 0.26 gramme of absolute alcohol was dropped into 1000 grammes of blood already *ninety hours old*, at a period, therefore, when the corpuscles have entirely lost their functions by disorganization, and when this quantity of alcohol was allowed to remain in such blood for forty-eight hours longer, it

Digitized by Google

could be almost entirely recovered by distillation, provided the temperature had not been above 60° F.; the legitimate inference, therefore, deducible from the results of the above experiment was that 0.3623 gramme of alcohol had been destroyed by oxidation into acetic acid, or still further in the two hours during which the aeration was continued.

No. 29.—Into 1000 grammes of fresh defibrinated ox blood contained, as before, in a glass flagon holding three quarts, 0.5823 gramme of absolute alcohol was dropped. The flagon was placed in a water bath and maintained at a temperature of 100° by a thermometer hung in the bath during two hours. Every two minutes the air of the flagon was removed by a bellows, and the flagon, closed by its stopper, for a moment withdrawn from the bath and vigorously agitated. Sugar was found to be present, by Fehling's test, both at the commencement and at the termination of this procedure. The blood was then distilled over the bath of common salt. The final distillate weighed 1.2616 gramme, reacting, as before, with the chromic acid test, and burning upon ebullition in the test tube. The quantity of alcohol was 0.1877 gramme; 0.3946 gramme had therefore disappeared.

From these results it appears that more than two-thirds of a certain quantity of alcohol added to fresh blood is destroyed in it under conditions simulating the oxidative processes of the economy; and by comparing the relative quantities added and obtained in the two cases it would seem that a certain limit exists in this respect, the quantities of alcohol which had disappeared being nearly the same in both experiments, viz.: 0.3623 gramme and 0.3946. These results, moreover, illustrate the great accuracy of the distillatory procedures.

That the oxidating process assumed was really efficient in the destruction of the alcohol lost in these two cases, would, however, be more positively affirmed in a control experiment, in which a converse effect was aimed at by the employment, instead of atmospheric air, of a substance known in the economy to be associated with hypo-oxygenation of the blood, viz.: carbonic acid. If under the use, in a similar way, of this gas alcohol was not destroyed, or was not destroyed in equal quantity, its destruction would be still more reasonably attributable to the assumed hæmal oxidation. The blood having been rendered *quasi arterial* in the last two experiments, it was determined to ascertain the effect of rendering it *quasi venous* by the use of carbonic acid instead of air.

No. 30.—Into 1000 grammes of fresh defibrinated ox blood, contained in the same glass flagon, 0.5176 gramme of absolute alcohol was dropped. The flagon was filled by displacement after the introduction of the blood, and before the addition of the alcohol, with pure carbonic acid gas. It was then closely stoppered, and maintained at 100° F., in a water bath regulated by a thermometer, during two hours. Carbonic acid was repeatedly added during the experiment, the blood being each time thoroughly agitated with it, with the production and maintenance of a very dark or even black hue. The blood was then distilled over the bath of common salt. The final distillate weighed 4.8848 grammes, and reacted as usual with the chromic acid solution; upon ebullition, likewise, in a test tube, giving off vapors which burned after the manner of alcohol. The quantity of alcohol recovered was 0.2931 gramme; 0.2245 gramme of this substance had disappeared.

The mean ratio of the alcohol lost in the two experiments in which the blood had been *arterialized* was 80.5; in the last experiment, in which the blood had been *venosed*, it was 43.0; results distinctly confirmatory of the supposition acted upon. Whatever process had been facilitated in the first two experiments, had been retarded or impeded in the third of the series. Arterial blood seems therefore to consume alcohol with far more facility than venous blood. If my object had been to *destroy* the oxidizing power of the blood corpuscles, hydrosulphuric acid might have been used, as when it was

Digitized by Google

employed with a similar purpose in the distillations of fresh ox blood. I preferred, however, to simulate as nearly as possible the normal differences of function obtaining in arterial and venous blood, especially as the use of sulphureted hydrogen had already contributed to the establishment of this hypothesis of the hæmal oxidation of alcohol, by increasing the quantities of alcohol recovered in the final distillates.

Alcohol is undoubtedly oxidized in the blood very completely, no portion of it, whatever, being recoverable from the urine, according to the experiments of Benz, Anstie, and Dupré, when moderate quantities of it are ingested, while by far the greater proportion of what is consumed is lost in the organism, even when excessive quantities are taken.

The alcohol which is produced in the blood by the *fermentation of hepatic sugar, is doubtless intended or utilized in a hæmal combustion, which is one of the prime sources of animal heat.* This alcohol must be regarded, moreover, as the hydrocarbonous element, long since divined to be destroyed by the oxidative powers of the economy, for the maintenance of the proper animal temperature. While muscular action generates heat, and all the chemical and functional actions of the body likewise extricate a certain amount of heat, I regard the two processes involved in the alcoholic fermentation of hepatic sugar in the economy as contributing by far the greater part of the heat dissipated by the skin and lungs, or lost in the fæces and urine and by radiation. The fermentation of hepatic sugar itself must originate a large share of heat, for the marked rise of temperature which occurs in fluids undergoing the alcoholic fermentation, is a matter of common observation, which must, moreover, be guarded against, at great expense, by all who manufacture fermented liquids, such as ale and beer. This source of heat, consequently, is the primary one, though, perhaps, not so great as that which is involved in the hæmal oxidation of the resulting alcohol. I have not, however, had opportunities to make comparative researches on these points, which, indeed, have not been studied carefully with the calorimeter. That alcohol should fulfill this function in the organism is certainly most harmonious with all we know of its properties and the circumstances attendant upon its generation in the economy and the elimination of its products. It is distinguished, chemically, for its ready oxidation. Its vapor is so inflammable as to be explosive, and its combustion is remarkable, both for the great heat which it evolves and the simple nature of its products. When alcohol is consumed in the air, water and carbonic acid alone result; substances evolved at all times in the air expired from the lungs. The existence of these matters in the expired air has been long known, and their relative quantities closely studied; their presence has always been referred to the combustion of a body containing carbon and hydrogen. What substance so fit for such a combustion as alcohol? A fluid miscible with water in all proportions, with the most marked affinity for oxygen; capable of destruction by direct inflammation, and by slow oxidation under the influence of albuminous matter in a state of change, by platinum black and platinum sponge, or even by the oxygen dissolved in aqueous fluids with which it may be mingled, provided the reaction of the fluid be alkaline; producing, moreover, by its rapid or slow oxidation, a gas which exists in the blood, and a fluid which is the common menstruum of the body, tolerated in almost any quantity by the organism. Alcohol is especially adapted to the function of maintaining the animal temperature.

It is easy to comprehend how alcohol may be transformed within the body into carbonic acid and water. "Aldehyde, acetic acid, formic acid, oxalic acid, and carbonic acid," says Liebig, "form a chain of products arising from the oxidation of alcohol; all these products appear to be formed simultaneously by the action of oxidizing agents." In virtue of its extreme division, of the ease with which it assumes a pronounced negatively electrical condition, and its remarkable power of condensing gases, and oxygen especially, of which it can condense and retain several hundred times its volume, pla-

Digitized by Google

tinum *black*, or metallic platinum in a state of minute division, effects a ready oxidation of alcohol into *aldehyde*, *basic* acetate of ethyl, or acetal and acetic acid. When platinum black is fully charged with oxygen, "if a drop of absolute alcohol be thrown upon it, the whole substance becomes *incandescent;* and if a capsule containing platinum black be placed under a bell-glass filled with air, the sides of which are moistened with alcohol, the vapors of alcohol undergo a slow oxidation, which converts them into acetic acid." (Reynault, II, 341.) Platinum sponge and wire also possess similar qualities, heat being always evolved.

A strong analogy exists between the particles of platinum black and the red corpuscles of the blood. Like platinum black, the red corpuscles possess the remarkable and significant property of condensing large quantities of oxygen, and of parting with it again with facility to easily oxidizable substances; their size is also extremely small, their number infinitely great, and their exposure to oxygen in the lungs wonderfully complete. The red corpuscle also acts like a minute reagent, whose sides are in states of opposite polarity, for we see it attracting its neighbor as soon as the movement of the blood stream is arrested. And if this be true, which is rendered still more probable from the fact that iron is a constituent of the blood corpuscles, we are prepared to witness the energy with which it attracts oxygen, for the same is known to take place out of the body, oxygen being the most strongly attracted by the magnet of all gases (De la Rive). The blood corpuscles, therefore, in virtue of their proper constitution, are evidently fitted to effect the facile oxidation of easily oxidizable matters. This property of easy oxidation, however, such as alcohol and other similar bodies possess, into whose constitution hydrogen largely enters, is greatly enhanced by the presence of albuminous matters in the condition of a ferment or undergoing change of the nature so closely studied by Liebig, provided a suitable temperature be maintained. The process of acetification is essentially based upon this ferment influence, and requires the coincidence of favorable temperature and ready access of oxygen. At temperature of 70° to 80° the process takes place slowly, but at about the temperature of the animal body and with the aid of a designedly extended exposure of the alcoholic fluid to the air, and the addition of some albuminous matters, the change of alcohol into vinegar is very rapid, and the amount of heat extricated considerable. The details of what is termed the "quick vinegar process," are worthy of study in this connection. Regnault describes it as follows: "In order that acetification may progress rapidly, the alcoholic liquor must be sufficiently diluted with water, and present a large surface to the oxidizing action of the air. These conditions are fulfilled on a large scale by using an alcoholic liquor containing one part of alcohol to eight or nine of water, and adding about one-thousandth of fermentable liquor, such as beet juice, potato juice, or small beer, when the liquor thus prepared is dropped into barrels filled with beech shavings. The lower part of the barrel is pierced with several holes, and the upper part with other holes, while a false bottom forms a vat, into which the alcoholic liquor is poured. The false bottom has a great number of holes, through which pass pieces of twine, having a knot on the end, to prevent them from slipping through. The alcoholic liquor flows along the twine, and dropping on the shavings, spreads into a thin layer, and presents a large surface to the oxidizing action of the air; oxidation being effected by means of the ferment contained in the liquor, and the albuminous substances in the wood, while the *temperature rises* and produces a current of air which enters at the lower holes and escapes at the upper ones. *Oxidation is so rapid that when the liquid reaches the bottom af the barrel, it frequently no longer contains any alcohol;* but if after one passage, the alcohol is not completely converted into acetic acid, it is passed through a second time." ("Elements of Chemistry," II, 544.) During the process the temperature maintains itself at from 86° to 97°; at the beginning, the alcoholic liquor must be first heated.

This process, originally noted by Liebig, strikingly illustrates the primary step of

Digitized by Google

the oxidation of alcohol in the animal body. This first stage must be through aldehyde into acetic acid; no other oxidative progress is possible for alcohol under the conditions of the living body. The substance which comes over in the later distillations just in advance of alcohol must be aldehyde, or its compound with the ethyl base of the alcohol, basic acetate of ethyl, so-called *acetal*. This latter body is one of the products of the slow oxidation of alcohol by platinum black, and may be regarded as a compound of aldehyde with oxide of ethyl. In contact with platinum black, this substance is oxidized in the air into *acetic acid*, says Löwig. Aldehyde itself is one of the most oxidizable bodies known; it dissolves in all proportions in water, boils at 71.3° F., and is converted into acetic acid by all oxidizing agents. For this reason, I have found the use of nitrate of silver, in considerable quantity, effective in destroying this mobile substance in the later and final distillates. On mixing from five to ten per cent. of its weight of crystallized nitrate of silver, in powder, with the distillate, the liquid becomes densely milky, and after an hour or two shows a blackish-brown, flocculent precipitate, while the supernatant liquid is clear. In the next distillation they no longer pass over, having been completely destroyed, if the quantity of nitrate of silver employed was sufficient. As both acetal and aldehyde produce acetic acid by oxidation, as both these matters, together with acetic ether in small quantity, are formed by the action of platinum black upon alcohol, as they are both destroyed by the action of nitrate of silver with heat, as they must necessarily exist wherever alcohol is oxidized slowly, and as the two volatile substances seen in advance of the alcohol, as it comes over beyond the first bend of the eduction tube, coexist with the alcohol found, thus, indeed, rescued from the oxidative processes of the body, I must regard them as aldehyde and acetal. The boiling points of these liquids are as follows: aldehyde, 71.3° F.; acetic ether, 165° F.; alcohol, 173° F.; acetal, 203° F., and water, 212°. Necessarily existing, in very minute quantities, in the liquid submitted to distillation, these substances will come over just at the beginning of the ebullition, which will not occur until the mean of the liquid has attained a temperature of 212°. They will then come over in the order of their boiling points, viz.: aldehyde, acetic ether, alcohol, acetal, and will continue to do so until a certain quantity of the liquid is brought over. This is what is observable during the final steps of every distillation of this kind, and as all these substances disappear under the oxidizing influence of nitrate of silver with heat, I must conclude that these bodies are really such as I have named, and consequently derivatives of the oxidation of alcohol and associated products in the acetification of alcohol. Hence, their presence I regard as very strong corroborative evidence of the actual oxidation of alcohol in the body by the stages named, viz.: aldehyde, acetic acid, formic acid, etc. The only test which I have been able to apply to these bodies is the nitrate of silver, which is immediately decomposed by them and destroys them in turn, metallic silver being left, while the alcohol is unaffected.

The conditions under which alcohol is oxidized in the blood we thus find to be wholly parallel with those existing both when it is oxidized by platinum black and in the "quick vinegar process." The process as conducted in the blood seems a combination of the peculiar features of both of these actions, while the theoretical demands, viz., minute dissemination of the oxygen, the presence of albuminoids in a state of change, a high and constant temperature, and the liberation of the oxygen in the form of ozone, are vastly better satisfied in the animal body than in the oxidative processes referred to. The great surface of the beech shavings is analogous to the great surface of platinum black, and to a great surface in the blood mass, effected by the smallness and number of the red corpuscles. It is not, therefore, the *lung*, as suggested by Liebig, but the *blood itself*, which is analogous to a barrel of beech shavings. Irrespectively of the red corpuscles, the blood containing alcohol is similar to the "*fermentable alcoholic liquor*" employed in the quick vinegar process, while the corpuscles

Digitized by Google

are analogous to the minute particles of platinum in the "black" or "sponge," especially in their power to condense large quantities of oxygen, and still further of liberating this oxygen to easily oxidizable bodies in the form of ozone.

The transformation of starch and dextrine into alcohol and carbonic acid are qualified by the extrication of large stores of heat, due to the gradual approximation of the molecules, as the original colloid becomes transformed into crystalloidal matters. We know this to be so out of the body, for the brewer's vat becomes heated, while the sugar is converted into alcohol and carbonic acid. The vinegar maker's tubs, also, where alcohol is transformed into acetic acid, become hot; the heat in this case being accredited to a loss of gaseous form by the oxygen consumed, a continued liberation of heat being observable as long as the quantity of carbon and of hydrogen are in excess of the oxygen combined with them in the train of matters submitted to progressive oxidation; all this is familiar enough to the chemist and physiologist. The existence of alcohol in the organism, as proved by the distillation of a variety of organic matters; the existence in the distilled fluids of substances significant of the progressive oxidation of alcohol into water and carbonic acid; the existence of glycogen in the liver, which, by a change into glucose, is plainly a part of the mechanism by which alcohol is set free for an instant in the blood, constitute a network of concordant facts whose dominant hypothesis is plainly the one maintained in this paper.

Dr. T. C. Stellwagen, of Philadelphia, Pa., read a paper—

ON THE TEETH AS INDICATORS OF THE NUTRITION, PAST AND PRESENT, OF THE PATIENT.

SUR LES DENTS, COMME INDICES DE LA NUTRITION DU PATIENT, AUTREFOIS ET A PRÉSENT.

ÜBER DIE ZÄHNE ALS ANZEIGER DER ERNÄHRUNG DES PATIENTEN IN DER VERGANGENHEIT UND GEGENWART.

The importance of proper regimen for development of the animal body has always been a favorite subject with physiologists; hence it is by no means a novelty to which your attention is invited in this paper; yet there are some features that would seem to bear closer investigation. There appears, at least among the laity, to be an increased interest in the study of diet, and an effort to discover that most proper for general nutrition. To judge from the elaborate and expensive advertising of different preparations of foods for infants and invalids, they at least seem to support many of the medical journals and furnish a very desirable pabulum for their publishers as well as some sort of pap for the babes. Without attempting to discuss the merits of the various and not unfrequently nauseous mixtures that, under the pretext of feeding, are forced down the throats of almost starving children, let us examine more thoroughly and discover any defects that may exist in the nourishment of the new being during its entire embryonic existence.

No one experienced in agriculture or pomology would think of taking seeds or graftings from a defective and poorly nourished plant or tree if others could be obtained, and the most abundant evidence exists, indeed the proposition seems undisputed, that by proper application of fertilizers to assist the moisture, and by the full amount of sun-

Digitized by Google

light and fresh air, the value of the crop both as to quantity and quality is greatly enhanced. The attempts at too close planting have long since had their erroneous and unprofitable results shown in the most practical ways. The agriculturist and arboriculturist both attest, by their careful avoidance of overcrowding their land, that the most favorable conditions for crops are those which allow the sun force and the atmosphere to have free access to the individual plants; indeed, in nearly all cases, both of these are absolutely essential. Likewise it must be a self-evident truth that the same conditions hold as regards the domestic animals, and although breeders of stock recognize and obey this same law, strangely enough human beings seem to have so long followed a reverse, or nearly opposite course with themselves and children as to be too often forgetful of or oblivious to its application to their own progeny. The seclusion of the child-bearing women, which has become essential in the blind obedience rendered to a fiat of society in many parts of our country, is productive of serious injury that may even prove fatal both to the mothers and their offspring. The dimly-lighted and tightly-closed apartments of the fashionable house affect the mothers' spirits, rendering them morbid and unhappy, apprehensive of danger and death. From like causes enceinte women's appetites, which at best may be subject to strange caprices, are made more unnatural and erratic. Finally it is not sufficiently well understood that innutritious or indifferent food, even if swallowed, cannot be properly digested or assimilated. Under these conditions the digestive, the nervous, the muscular and the circulatory systems are sluggish in their action. But one result may be anticipated as following the semi-invalid and inactive conditions of the patients. Hearty open air exercise is doubly imperative for the women about to become mothers, while under the ordinary states, only their own lives, happiness and health are at stake. Modern philanthropists have given much charitable attention to improvement in the ventilation and daylight illumination of the dwelling places of the poor or of those in very moderate circumstances in our large cities. While on the other hand, modern society, in obedience to fashionable whims, seems engrossed in the shutting out of these old time and hackneyed agencies, thus most seriously adding to inherited infirmities. With the rich this is accomplished by means of such devices as heavy, deeply set, narrow window sashes, with stained glass, often too costly to permit of their being opened for any purpose, lest a passing breeze might dash them to pieces. While, as if to further seal the doom of both mother and offspring, what little light or air may penetrate these barriers, is again obstructed by heavy hangings in the way of blinds, curtains, lambrequins, portierres and various forms of arras. This evil is so great, and upon the children of the thrifty its results are so monstrous, that it would seem to be an equally important benefit to mankind to rescue these worthies from the darkness of their abodes and the perils with which they are innocently, but no less direfully, threatened.

Already the sufferers from diseases engendered by these modes of living are increasing with alarming rapidity, and the premature impairment or loss of most important organs, as the eyes and teeth, are compelling the employment of specialists by thousands. Fortunes are daily expended by our fashionable and wealthy people in the vain endeavor to hold fast to the decrees of their fickle goddess and yet maintain a moderate degree of health for themselves and their still more unfortunate families. The injury to the respiratory apparatus by fine dust, the fuzz of the carpetings, and various forms of fashionable detritus, have been dwelt upon, but almost in vain, by the very practitioners whose honorable and praiseworthy motives are unapproachable, and in defiance of the fact that their entire livelihoods depend largely upon their reputation for skill in relieving the discomforts and diseases directly traceable to these pernicious furbelows. Yet, while this sacrifice of self-interest may not have been in vain, still the evil is growing and advancing in greater proportion than the numbers of our population.

The selection and use of the best materials for fertilizing and feeding is urged with

Digitized by Google

so much stress in the advertisements daily paraded before the public in agricultural and medical journals, that the more important matters of energy from the sunlight and the pure oxygen of the atmosphere seem to be overshadowed in the minds of the members of our clienteles. Children are begotten daily by parents who put at defiance the most imperative laws of life, and the innocent fœtuses are compelled to run the gauntlet of a process of temporary deprivation or *diminution* of the most essential matters and forces that sustain life. The newborn infants are coddled in darkness amid the bed hangings and coverings, which are costly both to the purses and the lives of the community, and the mothers silently and secretly grieve over the deficient offspring, whose troubles were coexistent with their first moments of earthly being. Birth-marks among these unfortunates, who should be the best reared, are as plentiful as their monstrous deprivations, and have been brought about by a prodigality of money expenditures that is productive of a like prodigality of diseased conditions and surroundings. Dental histology has opened the books of the lives of the patients written by nature until their pages are filled with the ineradicable marks upon the successive layers of the teeth structures; writings as momentous, as infallible and truthful; records fraught with instruction to intelligent observers. Upon the microscopical tablets of dentine may be seen the impressions that plainly show the defects and omissions in nutrition of the former life, beginning as early as the fortieth day of the existence of the new being, who, like Noah, has been tempest-tossed in the amnion ocean, secure in the ark of its embryonic membranes.

The deficiencies of the mothers resulting from false notions of the propriety of exercise, as above mentioned, and the evils of their depraved digestion, assimilation and circulation are oftentimes duplicated with increased injury to the fœtuses or children that depend upon these mothers for their nourishment. Thus results the stunting of the development and impairment of the structural formations of the different organs that so universally beget disease. If the darkness of the abysses of the Mammoth Cave is responsible for the lack of development of the eyes of the fish that abound in the rivers of this wonderful cavern, why should not the eyes of unborn babes be subjected to like deleterious influences during intra-uterine life? Certainly this is borne out by analogy with the teeth, which unerringly show the markings of the afflictions of their possessors, due to defective assimilation during general disease both of the children and the mothers. So plainly are these demonstrable as to put the causes in full view beyond the question of the necessity of any further search for them. Added to the inheritance, from the most trifling peculiarities to defects which may compel the unfortunates to submit to the premature extraction of the whole or a portion of a denture, there is further to be deplored the almost willful starvation and consequent defective nourishment of all the organs at the time when the most trivial causes and slightest events are proven to be prolific of the gravest after results. The direction of the developing bud is even more important to its after life than the proverbial bending of the twig is to the inclination of the tree.

The conditions of the teeth are closely allied to those of the bones, and as the former are much more easily subjected to examination as to density and histological structural changes, why should they be neglected in an examination for physiological, diagnostic, or prognostic purposes. The sympathetic changes in the mouth have long been known to be in close relation to the diseases of the alimentary canal, but they pass so swiftly as rarely to leave more than an evanescent history behind. Upon the contrary, we find that the teeth preserve these same records for their entire life, nay more, the markings are rarely obliterated during their very existence. By carefully conducted microscopical examination of the deciduous teeth as they are shed, we may learn much of the children's past, as to their nutrition and assimilation, that will be of untold value. By means of regular periodical examinations as to the density and

Digitized by Google

general condition of these organs we may gather practical information as to the effects of changes of diet, water, and mode of life. The variations in the teeth of the mothers in like manner are worthy of more earnest and constant attention by the general practitioner, since they quickly indicate the lack of lime for hardening them and a similar impoverishment of the fœtal bones, mostly due to absence of this material from the food and rarely attributable to defect of assimilation. Temperament and disposition are so well shown by the shapes, shades and arrangement of the teeth, that we may, without much if any risk, presume that they are the underlying causes, the expressions or the effects of these upon the general nutrition, and with which these organs thus harmonize. The transverse axes of the human teeth in their natural positions form the parabolic curves of the jaws, which arrangements are affected in like manner by certain diseases, as has been shown by Mr. John Tomes in his studies of the development of these organs. The lateral pressure exerted by the cheeks upon the teeth during the labored breathing of children suffering from chronic swellings of the tonsils, tends to make flattened sides, lateral compression, and gothic arching of the roof of the mouth or palatal plates of the superior maxillary bones and pushing forward of the vomer, causing aquiline or prominent noses. This may be a natural explanation of the peculiarities of visage presented in different races. Thus the Roman climate and pontine marshes may have, by natural causes, produced the Roman nose.

Facial expression enters largely into the elements of which a diagnosis is made up, and it depends much upon the formation of the maxillary bones. It requires but little clinical observation to see that the maxillæ vary according to the arrangement of the teeth, which corresponds in its perfection with the use of these organs in mastication, and the efforts made to comminute the food. Thus we may see how, without fully understanding the subject, a practical diagnostician is forming opinions of patients from the effects of their diet and past life.

Digitized by Google

FIFTH DAY.

Dr. O. EVERTS, of Cincinnati, Ohio, read a paper—

ON THE CEREBRAL FUNCTION—TRANSMUTATION OF ORGANIC OR LIFE FORCE INTO PSYCHIC FORCE—THE PHENOMENA OF CONSCIOUSNESS, SEQUENTIAL, ETC., ETC.

DE LA FONCTION CÉRÉBRALE—TRANSMUTATION DE FORCE ORGANIQUE OU VITALE EN FORCE PSYCHIQUE—LES PHÉNOMÉNES DE LA CONSCIENCE EN SUITE, ETC., ETC.

ÜBER CEREBRALE FUNKTION—UMWANDLUNG ORGANISCHER ODER LEBENSKRAFT IN PSYCHISCHE KRAFT—DIE ERSCHEINUNGEN DES BEWUSSTSEINS IN FOLGE, ETC., ETC.

The propositions of this paper, expressed or understood, include the following:—

Matter, restless and eternal, is everything. Force, undergoing transmutations corresponding to material changes, is a manifestation of an inherent quality of matter. Psychic force, although apparently peculiar, as a manifestation, to living mechanisms, is correlatable with all other forms of force. Brain mechanisms alone supply the necessary conditions for the manifestation of a transmutation of energy of other characteristics into mental energy. All phenomena, however familiar or strange, are manifestations of conditions of matter by which apparent transmutations of force are effected; hence natural, "inevitable sequences" of such "antecedent conditions."

The importance of the brain as an organ essential to life has always been recognized. Vague ideas of a mysterious relation between brain and mind, and of certain inexplicable powers exercised by the brain in the economy of animal existence, were entertained by the older physiologists. Its real capabilities and offices, however, were but dimly foreshadowed by instructors in physiology before the beginning of the present scientific era—or the present century. The gross anatomy of the brain, cord, and nerves had been accurately described; Charles Bell and Marshall Hall added greatly to our knowledge of the capabilities and functions of the brain and its appendages as related to respiration, circulation, nutrition and locomotion; and Dr. Gall had made memorable dissections, observations, and suggestions, leading in the direction of scientific truth; but it was reserved for the present generation of physiologists to demonstrate the more intricate mechanisms of brain structure, and ascribe to them supreme functions, of which they alone are capable.

It is by reason of the profound researches of modern physiologists, aided by the light of a general illumination of the universe effected by modern science, that we are now brought face to face with, and stand more or less hesitantly in the presence of, certain final questions in physiology.

Is consciousness an inherent quality of matter, when reduced to its lowest term, responsiveness; or a condition of matter incidental to changes essential to the apparent transmutation of vital into psychical energy?

Digitized by Google

Are the phenomena of mind, or manifestations of consciousness, however simple or complex, natural and inevitable sequences of material movements, effected and affected by material conditions and capabilities? In other words: Is matter, under any combination of circumstances, capable of feeling? and do brains think, unaided by supernatural entities, of which no scientific knowledge is obtainable?

Why should physiologists, or others, emancipated from the ancient thralldom of the "supernatural" in other respects, hesitate in the presence of these final questions in physiology? Is it because of the lack of testimony? Is it not rather because of a natural, perhaps commendable, reluctance to destroy the last ship that might serve in the days of our weakness, and weariness of adventure, to bear us back to the haven of our ancestors in the land of myths and the realms of the supernatural?

What is the testimony respecting these things?

(A) As all our knowledge is based upon information derived from material objects through the senses, and all imaginations are combinations, simple or complex, of memories of sensuous things, we have no knowledge, and cannot have any, of immaterial objects—any attempt to think of which necessarily resulting in confusion of ideas.

(B) Self-consciousness, the source from which supernaturalists presume to derive their information respecting mental phenomena, is, after all, consciousness of matter, as all efforts to construct mental concepts of anything without form or substance end in confusion or the substitution of images of material objects of which we have already become conscious through sensuous perceptions.

(C) Matter, under all known or presumable circumstances, manifests capabilities so nearly resembling consciousness as to suggest the inherency of such capableness, however latent under ordinary circumstances. Unorganized matter manifests elective affinities. The lowest recognizable living mechanisms exhibit responsiveness to excitations corresponding to their necessities as living beings. As more complex structures are evolved by natural processes of selection and adaptation, in accordance with a trustworthy uniformity of conduct characteristic of matter, equivalent to a reign of law, greater capabilities of responsiveness, corresponding to increased necessities of the being evolved, are invariably manifested, until we see in succession the selective responsiveness of unorganized matter—the irritability and contractility of vegetable structures, and the lower forms of animal mechanisms—instinct, perception, memory, imagination, reason, etc., manifested by the highest and most complex beings; the chain of beings, and the chain of manifested capabilities running in parallel lines, inseparably and harmoniously.

Is not the testimony conclusive? If valid and convincing as to the inherent capabilities of a plant, a worm, a fish, a snake, a bird, a beast, of any description, it is valid, and should be convincing, respecting the cerebral capabilities of a man. If the inherent capabilities of a molecule of living matter do not require, for the performance of its functions, and is not inhabited by, a dominating supernatural entity, having no other relation to matter than that of temporary association, it is irrational and unscientific to assume that a man requires the services of and is inhabited by such a hypothetical being, in contempt of logic and appearances.

The occult phenomena of modern spiritism, even—such of them as may be genuine—do not require the presence, or imply the existence, of such beings; inasmuch as such phenomena are always associated with material, living beings, the full psychical capabilities of whose brain mechanisms even science is not competent to limit, or explain.

The occultness of a phenomenon should not remand modern observers, necessarily, to the supernatural for an explanation that, inasmuch as being purely hypothetical and outside of human knowledge and capability of knowing, does not explain, but

Digitized by Google

still further mystifies. Do brains think? Is an aggregation of living mechanisms constituting a man capable of thinking? Why not?

Are our ideas of matter so gross and unsympathetic—so limited and dogmatic—that we cannot entertain such affirmations? What warrant have we for supposing that immaterial beings, if the existence of such beings were possibly supposable, are more capable than material beings whose performances are as palpable and unmistakable as our capabilities, whether material or spiritual, are qualified to appreciate. Is it not our "familiarity" with matter that "breeds contempt" of it? Is it our total ignorance of supposititious spiritual beings that justifies an exaltation of their imaginary capabilities to any degree necessary to meet the exigencies of our ignorance?

Some modern physiologists, having rejected the ancient theory of supernatural interference in the performance of the functions peculiar to cerebral mechanisms, substitute spiritual entities by the introduction of "force," which, although attributed to "nervous action" as the cause of its being, instead of a miraculous and supernatural parentage, is treated of as an entity in association with material mechanisms, with coöperative, if not independent, capabilities, under the general designation "mind." Having thus substituted a hypothetical entity, "spirit," with an entity equally hypothetical called "the mind," one modern author of distinction goes so far as to accommodate his new theory of cerebral physiology to ancient metaphysical notions respecting "the mind" by asserting: "The mind, like some other forces, is compound—that is, is made up of several sub-forces. These are: perception, intellect, emotions and will. Either one may be exercised independently of the other," etc. This theory is without scientific justification or support. It is not true physiologically nor psychologically, if the two branches of knowledge are really distinguishable. Force is not an entity capable of subdivision and the performance of function. It does not bear the same relation to structures manifesting it, or relations to the brain similar "to those existing between any other viscus and the product of its action." Force is phenomenal, not material. Bile and all other products of glandular action, for example, are substantial—as material as the viscera that organize or eliminate them.

Neither the brain nor any other organ or material mechanism "exercises" force or is exercised by force as an entity. As well might one say that a steam whistle exercises sound, or that a cannon ball exercises momentum, or that the clouds exercise lightning—the phenomena of sound, motion and lightning being manifestations of conditions of matter undergoing change or transmutations and interchanges of relation between particles effected by inherent qualities, responsive at all times and under all circumstances to necessities implied by such changes. It may be convenient to call these manifestations "forces," and speak of matter as "producing" them, as if they were material offspring. But when steam is generated by heat manifested during the combustion of coal, or a tree is shattered by lightning, or a cannon ball crashes through the timbers of a ship, what has really occurred? The material conditions of coal and water, of clouds and tree, of cannon ball and ship's timber, have all been changed in consequence of their relation to, and changes taking place in, other material mechanisms, each change, with its manifestations, being an "inevitable sequence of antecedent conditions" of matter implicated, as is indicated by the great similarity, if not absolute uniformity, of such occurrences under similar circumstances. Surely nothing has been added to or taken from matter as an aggregate.

Much of apparent error in the assertions of advanced thinkers may be attributable to the "despotism of words." The terms that they are compelled to use in their endeavors to communicate new ideas to the less apt or forward renders the process difficult and the result sometimes doubtful. If it were practicable to use the term "spirit" or "spiritual" in speaking of the inherent qualities and capabilities of matter, even to the extent of recognizing many of the so-called "spiritual phenomena" of

Digitized by Google

all ages as genuine, but natural—without conjuring in imagination the presence of a troop of ghostly spectres from an imaginary world, and ascribing such phenomena to supernatural, personal entities—scientific truths might be communicated thereby, and dissension of opinion and beliefs correspondingly avoided.

But fortunately for science, it is never impatient nor in haste. It believes in and waits for growth, and that a time will come when such terms can be used intelligently; when it will be seen by many, if not by all, that matter, uncreated and eternal, is everything; that "force," "intelligence," "spirituality," and all of the facts and phenomena that have so dazed mankind in his infancy, and still overawe with portentous shadows multitudes of truth-seeking beholders, will be recognized as manifestations of inherent capabilities and qualities of matter, and the supernatural will disappear forever from this world's horizon.

The following paper was read by title, the author being absent:—

DIE LEHRE VON DER REFLEKTORISCHEN SPEICHELSEKRETION.

THE DOCTRINE OF REFLEX SALIVARY SECRETION.

LA DOCTRINE DE LA SÉCRÉTION RÉFLEXE DE LA SALIVE.

VON DR. C. ECKHARD,

Giessen, Deutschland.

Die in der physiologischen Literatur über diesen Gegenstand vorhandenen Angaben stimmen theils mehrfach nicht mit einander überein, theils sind sie aus Versuchen abgeleitet, welche der Wiederholung bedürfen; theils sind die Folgerungen, welche man aus denselben gezogen hat, nicht sämmtlich der Art, dass man ihnen so ohne Weiteres beistimmen kann. Dies hat mich veranlasst, die fragliche Lehre in meinem Laboratorium einer Prüfung unterziehen zu lassen. Ich gebe hier eine kurze Darstellung der hauptsächlichsten Ergebnisse derselben.

Sämmtliche Untersuchungen beziehen sich ausschliesslich auf die reflektorische Sekretion in der Submaxillardrüse. Die in den anderen Speicheldrüsen sind vorerst bei Seite gestellt und einer weiteren Untersuchung vorbehalten worden. Als Versuchsthiere dienten Hunde. Zunächst wurden die Reflexe von den Schleimhäuten aus vorgenommen und vorzugsweise die von der Magenschleimhaut und der Conjunctiva aus in besondere Betrachtung gezogen. Beiläufig sind einige Erfahrungen über die Erfolge der Reizung der Nasen- und Trachealschleimhaut gewonnen, jedoch nicht weiter ins Einzelne verfolgt worden. Ueber letztere sei kurz Folgendes bemerkt. Jede Einleitung der Chloroformnarkose, gleichgiltig, ob man sie von der Nasenhöhle oder von einer Trachealfistel aus vornimmt, erzeugt im Anfang eine starke Sekretion in beiden Submaxillardrüsen, die eine gewisse Zeit anhält und dann bis zum Verschwinden nachlässt, wenn man die Einathmungen aussetzt. Erneute Einleitung des Gases hat denselben, wenn auch dem Grade nach oft schwächeren, Erfolg. Da

Digitized by Google

diese Sekretionen nicht weiter verfolgt worden sind, so kann über ihre Entstehung keine nähere Auskunft gegeben werden. Da sich die Versuchsthiere den Einathmungen widersetzen, dabei sehr unruhig werden und widerstrebende Bewegungen machen, ich aber bei diesen und allen folgenden Versuchen die Wahrnehmung gemacht habe, dass mit diesen Umständen meist sehr ausgiebige Speichelsekretionen combinirt sind, so kann möglicherweise dies die alleinige oder in den Vordergrund tretende Ursache der fraglichen Sekretionen sein. Möglich scheint allerdings auch, dass die Reizungen der Schleimhaut des Respirationstraktus an und für sich, sowie auch die in das Blut eingetretenen Gasantheile erregend auf die cerebralen Ursprünge der Speichelnerven wirken. Ueber diese Möglichkeiten ist aber erst noch durch besonders eingerichtete Versuche zu entscheiden, indem wir darüber keine Erfahrungen gesammelt haben.

Von besonderer Wichtigkeit war es, die Frage zu entscheiden, ob die Reizung der *Magenschleimhaut*, ähnlich der der Mundhöhlenschleimhaut fähig sei, reflektorisch die Speichelsekretion hervorzurufen, weil die bisher über diesen Punkt gemachten Angaben auseinandergehen. Die bezüglichen Versuche wurden folgendermaassen angestellt. Nachdem die Wharton'schen Gänge mit Kanülen versehen worden waren, wurde die Bauchhöhle eröffnet und durch die vordere Magenwand neben einander zwei stärkere Fäden gezogen, so dass zwischen beiden ein Raum blieb, gross genug, um daselbst eine Oeffnung anzubringen, welche zur Injection der reizenden Flüssigkeiten dienen sollte. Von den erwähnten Fäden wurde jeder an ein Stäbchen gebunden und dies über die Ränder der Bauchwunde gebrückt. Nunmehr wurde zugewartet, bis eine durch die erwähnten Manipulationen etwa angeregte Speichelsekretion vollständig zur Ruhe gekommen war. Ich muss hier einschalten, dass man dann und wann auf Thiere trifft, bei welchen, nachdem man Kanülen in die Wharton'schen Gänge gebunden, aus diesen ohne äusserlich wahrnehmbare Ursache fortwährend Speichel ausfliesst. Besänftigt sich die Sekretion nicht bald, so ist ein solches Thier zu keinem Versuch über reflektorische Speichelsekretion zu gebrauchen. Wurde nun an einem so vorgerichteten Thiere, wie es eben beschrieben worden ist, keine Speichelsekretion beobachtet, so machte man demselben zwischen den Fäden einen Schnitt in die Magenwand und, nachdem man sich von der auch jetzt noch bestehenden Ruhe der Drüsen überzeugt hatte, injicirte man verschiedene Mengen reizender Substanzen, wie Essigsäure, verdünnte Kochsalzlösungen etc., in den Magen. Unter diesen Umständen wurde aber *niemals* eine Sekretion in den Speicheldrüsen hervorgerufen. Es ist darauf zu achten, dass die Versuchsthiere auch während dieser Injectionen völlig ruhig bleiben, widrigenfalls eine mit der Unruhe des Thieres möglicherweise einhergehende Sekretion, als eine durch den Reiz der Magenschleimhaut entstandene, vorgetäuscht werden kann. Die Versuche verlaufen in völlig gleicher Weise an narkotisirten, wie nicht narkotisirten Thieren. Schon vor einer Reihe von Jahren liess ich bei einer anderen Gelegenheit Versuche über denselben Gegenstand ausführen. Damals bedienten wir uns mit Magenfisteln versehener Hunde, von denen bekannt ist, wie sie im Allgemeinen bei den verschiedenen Manipulationen, die man an ihren Fisteln vornimmt, sich sehr geduldig benehmen. Auch bei dieser Gelegenheit konnten wir durch keine Reizung der Magenschleimhaut irgend welcher Art eine reflektorische Speichelsekretion hervorrufen. Es wäre nicht darauf zurückzukommen gewesen, wenn nicht das Gegentheil von den hier mitgetheilten Ergebnissen in die neueren Hand- und Lehrbücher übergegangen wäre. Dies rührt offenbar daher, dass die Verfasser dieser sich auf einige Versuche, welche positive Ergebnisse von den Reizungen der Magenschleimhaut berichten, mehr zu stützen für berechtigt halten, als auf die mit negativen Ergebnissen. Wer aber solche Versuche mit der von uns angegebenen Vorsicht wiederholt, wird unsere Angabe für die richtige erkennen und sich dabei überzeugen, wie höchst wahrscheinlich die gegentheilige Versicherung dadurch zu Stande gekommen ist, dass die Thiere, an denen man die Versuche ausführte, während dieser sich nicht absolut ruhig

Digitized by Google

verhalten haben. Eine Speichelsekretion in Folge der gereizten Magenschleimhaut würde auch den Zwecken der Magenverdauung wenig dienen. Welchen Sinn sollte es haben, durch die fortwährende mechanische etc. Reizung der Magenschleimhaut mittels der aufgenommenen Nahrung fortwährend Speichel absondern zu lassen und dadurch den Magen mit einer Flüssigkeit zu belasten, die durch ihre alkalische Reaktion die Pepsinverdauung beschränken muss!

Zur Verständigung muss noch Folgendes hinzugefügt werden. Wir haben mehrmals beobachtet, wie das Hervorziehen der Magenwand oder die Quetschung derselben, wie sie namentlich bei der Einbindung einer Kanüle in eine kleine Oeffnung der Magenwand stattfindet, bei dem dadurch beunruhigten Thiere mehr oder minder deutlich Speichelsekretion hervorrief. Es ist deshalb in Zukunft sich dahin auszudrücken, dass solche Einwirkungen auf den Magen, welche über das eigentliche Bereich der Schleimhaut hinausgehen und die übrigen Häute des Magens anätzen oder sonstwie verletzen, Veranlassung zur Submaxillardrüsensekretion geben können. Doch wird sich zu hüten sein, solche Sekretionen als rein reflektorische aufzufassen. Da sie bisher nur bei gleichzeitig eintretender Unruhe des Thieres beobachtet wurden, so sieht man sie wohl besser vorerst als damit *combinirte* Erscheinungen an und stellt sie in eine Kategorie mit den hernach zu besprechenden, bei Reizung *sensibler* Nervenstämme auftretenden Erscheinungen. Nach diesen negativen Ergebnissen an der Magenschleimhaut lag keine Veranlassung vor, noch andere Abschnitte des Tractus intestinalis zu prüfen.

Die physiologische Literatur berichtet auch von reflektorischen Speichelsekretionen in Folge von Reizungen der Conjunctiva des Auges. Dieselben sollen schon auf verhältnissmässig geringfügige Reize, wie z. B. auf Einträufelungen sehr verdünnter Atropinlösungen, eintreten. Dabei soll letzteren durchaus keine specifische Wirkung zukommen, sondern dieselben sollen ähnlich anderen Reizen wirken. Die Versuche, welche in meinem Laboratorium angestellt worden sind, beziehen sich nur auf die Submaxillardrüse des Hundes und haben ergeben, dass durch Reizungen der Conjunctiva mittels Atropin- oder verdünnter Kochsalzlösungen etc. die Speichelsekretion nur *ausnahmsweise* einleitbar sein muss. Wir sahen sie bei behutsamen Manipulationen am Auge des Hundes *nie*. In allen Fällen blieben dabei unsere Thiere vollkommen ruhig. Es mag aber sein, dass gelegentlich, namentlich wenn man noch andere Thiere, etwa Katzen, zur Prüfung verwendet, auch einmal Sekretionen eintreten können; auf alle Fälle handelt es sich aber um keine sehr einfache Beziehung zwischen Conjunctival- und Speichelnerven, ähnlich der, wie sie zwischen den letzteren und den Zungennerven besteht.

Besonders zahlreich sind die Versuche, welche über die Wirkung gereizter sensibler Nerven*stämme*, ausser den sensiblen Zungennerven, auf die Speichelsekretion berichtet werden. Die Berichte unterscheiden zwischen die Speichelsekretion erregenden und hemmenden Einwirkungen. Die ersteren werden gemeldet als Folge der Erregung sehr verschiedener, gemischter oder sensibler Nervenstämme, so dass es scheint, als ob fast jeder sensible Nervenstamm unter gewissen Erregungsverhältnissen zur Speichelsekretion führen kann. Wirklich beobachtet ist diese zur Zeit bei Reizung des Ischiadicus, Ulnaris, Musculo-cutaneus, Medianus, Auricularis und Vagus. In Bezug auf die Submaxillardrüse des Hundes schliessen sich die hier angestellten Versuche diesen Berichten im Allgemeinen an. Es sind aber dabei die folgenden Bemerkungen zu machen. Das Thatsächliche anlangend, so trat bei den Versuchen die Eigenthümlichkeit hervor, dass bei Weitem in der Mehrzahl der Fälle die Speichelsekretion in Folge der Reizung gemischter Nervenstämme (N. isch. oder n. ulnaris) sich nur dann einstellte, wenn jene einen solchen Grad erreichte, dass dadurch die Thiere in merkliche Unruhe geriethen und diese durch mancherlei widerstrebende Bewegungen ausdrückten, wobei man noch den Eindruck empfing, als ob die letzteren ein besonders günstiges Moment bildeten.

Digitized by Google

Auf alle Fälle kommen die auf Reizungen gemischter Nervenstämme erfolgenden Sekretionen in der Submaxillardrüse ohne die erwähnten Begleitungserscheinungen verhältnissmässig selten zur Beobachtung. Man wird in diesem Glauben noch besonders durch die Beobachtung bestärkt, dass man nicht selten bei Untersuchungen in der Chloroform- oder Chloralnarkose bemerkt, wie Reize geringeren Grades, die in nicht narkotisirtem Zustande Sekretion hervorrufen, merklich verstärkt werden müssen, bevor sie in der Narkose denselben Erfolg geben, und bei welchen dann die Thiere gewöhnlich aus ihrer Narkose erwachen. Es soll damit nicht gesagt werden, dass die in der Narkose in der Regel erst bei stärkeren Reizgraden auftretende Sekretion allein in der geringeren Empfindlichkeit der Thiere ihren Grund habe. Ausser dieser können noch andere Momente mitwirken, die zur Zeit nicht mit Präcision anzugeben sind. Nur bei den höheren Graden der Curaravergiftung tritt es ein, dass die Reizungen der gemischten Nervenstämme Submaxillardrüsensekretion ohne Aufregung der Thiere und ohne besondere Bewegungen derselben geben, was bei der bekannten Wirkungsweise des Curara wohl selbstverständlich ist. Vergl. hierzu weiter unten.

Ueber die Art, wie man sich die auf Reizung sensibler oder gemischter Nervenstämme entstehende Speichelsekretion zu denken habe, gehen die Meinungen der Physiologen auseinander. Einige stellen sich vor, dass die fraglichen Reizungen eine allgemeine Erhöhung des Blutdruckes bewirken und davon die entstehende Speichelsekretion eine Folge sei. Dagegen ist mit Recht eingewendet worden, dass die Speichelsekretion in keinem besonderen causalen Zusammenhange mit dem Blutdrucke stehe, wie sich unter Anderem daraus deutlich ergiebt, dass bei atropinisirten Thieren die Reizung des Ischiadicus zwar den Blutdruck erhöht, aber keine Speichelsekretion bewirkt.

Weiter hat man die Hypothese aufgestellt, dass durch Reizung der sensiblen Hautnerven Gefässverengerung in den nervösen Centralorganen bewirkt werde und dadurch eine venöse Hyperämie entstehe, die eine Erregung der Speichelnervencentren hervorrufe. Folgende Combination von Erscheinungen hat zu dieser Meinung geführt. Man beobachtet bisweilen, dass die beiden grösseren Speicheldrüsen ohne besondere äussere Veranlassung secerniren. Es ist geglaubt worden, dies komme nur bei lebhaft in der Verdauung begriffenen Thieren vor und sei auf die grössere Kohlensäurebildung zu dieser Zeit zu beziehen, indem man daran erinnert, dass bei der Asphyxie sich zu einer gewissen Zeit Speichelsekretion einstellt. Man ging dann weiter und sagte, dass die Reizung der sensiblen Nervenstämme, indem sie eine Zusammenziehung der Hirngefässe und venöse Hyperämie erzeuge, einen ähnlichen Erfolg wie die Asphyxie bewirke. Diese Deutung der Entstehung der Speichelabsonderung in Folge der Reizung sensibler Nervenstämme ist aber gleichfalls unhaltbar. Die erwähnten spontanen Speichelungen stehen mit der Verdauung in keinem Zusammenhange und besteht keinerlei Recht, von einem Verdauungsspeichel im erwähnten Sinne zu sprechen. Sie kommen bei nüchternen Thieren ebensogut vor, als bei solchen, die eben Nahrung zu sich genommen haben, und können ebenso bei beiden fehlen. Ueber ihre wahren Ursachen will ich mich hier nicht verbreiten. Ferner stellt sich die in der Asphyxie vorkommende Speichelsekretion erfahrungsgemäss erst bei so hohen Ueberladungen des Blutes mit Kohlensäure ein, wie sie durch die reichlichere Kohlensäurebildung in der Verdauungsperiode nicht vorkommen können; es müssten sich ja sonst auch in dieser die Zeichen der Asphyxie einstellen. Endlich geben die Reizungen sensibler Nervenstämme, wenn sie ihrer Stärke nach überhaupt geeignet sind, Speichelsekretion hervorzurufen, diese in so kurzer Zeit, innerhalb derer eine *vollkommene Schliessung* der Trachea sie niemals hervorzurufen vermag.

Mir will es scheinen, als würde man zur Zeit am correctesten verfahren, wenn man die auf Reizung sensibler Nervenstämme erfolgende Speichelsekretion nur als eine

Digitized by Google

solche bezeichnet, die sich mit dem aufgeregten Zustande combinire, in welchen man die Thiere durch jene Reizungen versetzt, wobei man freilich darauf Verzicht leisten muss, anzugeben, wie sich diese Combination im Einzelnen vollziehe. Ich mache diesen Vorschlag aus folgenden Gründen. Erstens ist eine Anzahl physiologischer Erscheinungen bekannt, mit denen sich eine Absonderung des Speichels ohne weitere Zuthat von aussen sehr leicht vergesellschaftet, und an diese kann die auf Reizung sensibler Nerven erfolgende als ein weiteres Beispiel bequem angeschlossen werden. Wenn man ein beliebiges motorisches Rindenfeld, es muss dies nicht nothwendig das des Facialis sein, intensiv oder längere Zeit reizt, so entstehen bekanntlich leicht Krämpfe, die auch noch nach Entfernung der Reize eine gewisse Zeit fortbestehen. Mit diesen Krämpfen ist fast ausnahmslos Speichelsekretion verbunden, wie denn auch nicht selten eine solche bei durch anderweitige Ursachen hervorgerufenen Krämpfen eintritt. Weiter erzeugt der Anblick begehrlicher Speisen bei manchen Personen Speichelabsonderung, so dass auch bei gewissen Vorstellungen die Speicheldrüsen in Thätigkeit gerathen. Wir haben endlich, wie weiter unten nachzusehen ist, bei gewissen Graden der Curaravergiftung die Beobachtung gemacht, dass in Zeiten, wo noch keine vollkommene Lähmung aller motorischen Körpernerven eingetreten ist, die Thiere von mehr oder weniger ausgedehnten Muskelzuckungen ergriffen werden, und dass damit parallel eine vermehrte Speichelsekretion einhergeht, gleichfalls ohne dass ein äusserer Reiz einwirkt. Zweitens ist gegen den Ausdruck "reflektorische" Speichelsekretion, der für unsere Erscheinungen empfohlen worden ist, Mancherlei einzuwenden. Bei allen denjenigen reflektorischen Erscheinungen, die nur einmal, höchstens doppelt, am Körper vorkommen, wie Pupillarbewegung, Husten, Niesen, Augenblinzeln, sind die sensitiven Nervenbahnen, welche jene auslösen, ganz ausserordentlich beschränkt, während in unserem Falle, der gleichfalls zu dieser Gruppe sozusagen beschränkter oder localer Reflexe gehört, wohl von fast allen sensiblen Nervenstämmen aus unter gewissen Umständen Speichelsekretion hervorgerufen werden kann. Mit Rücksicht auf diese Bemerkung erscheint es angemessener, nur insofern von reflektorischer Speichelsekretion zu sprechen, als dabei die Geschmacksnerven, oder vielleicht Mundhöhlennerven überhaupt, als incitirende Nerven in Betracht kommen, und für diejenigen Speichelsekretionen, die auf die Reizung fast aller sensiblen Nervenstämme sich einstellen, den Ausdruck "combinirte" Speichelsekretion einzuführen. Ausserdem machen sich auch erfahrungsgemäss die Sekretionen auf Reizungen sensibler Nervenstämme der Art, dass nicht bloss jene, sondern mannigfache andere, in verschiedenartigen Sphären auftretende Thätigkeiten erscheinen, wie: Schmerzensäusserungen, veränderte Athembewegungen, sehr complicirte und nicht gesetzmässige Körperbewegungen etc. Es ist auch behauptet worden, dass man durch gewisse Grade der Reizung sensibler Nervenstämme eine aus irgend welchen Gründen bereits bestehende Speichelsekretion *hemmen* könne. Die Versuche, welche wir hier über diesen Punkt angestellt haben, ergaben keine *überzeugenden* Resultate. Sie scheiterten daran, dass die Sekretionen, die wir zu hemmen beabsichtigten, in ihrem Verlauf zu variabel waren, mochte man die durch Reizung der Mundhöhlenschleimhaut ausgelöste oder die spontan bei der Curaravergiftung auftretende zu dem beabsichtigten Zwecke auswählen. Bei dieser Gelegenheit machten wir die bereits oben angedeutete Wahrnehmung, dass in solchen Fällen der Curaravergiftung, wo noch nicht absolute Lähmung aller motorischen Körpernerven eingetreten ist, die Thiere in unregelmässigen Intervallen von mehr oder weniger ausgedehnten Zuckungen befallen wurden und jedesmal, wenn sich diese einstellten, der Speichelfluss, welcher vorher in Folge der Vergiftung bestand, auffallend beschleunigt erschien. In einem Beispiele war diese Coincidenz so scharf ausgesprochen, dass man mit der grössten Sicherheit aus dem Eintritt der einen Erscheinung den der anderen ankündigen konnte. Aber auch noch nach dem Verschwinden der

Digitized by Google

erwähnten Zuckungen floss der Speichel nie für längere Zeit in gleicher Stärke aus. Wir versuchten zwar, einmal mit dem erwähnten Umstande bekannt, verschiedengradige Reize auf den Ischiadicus während verschiedener Phasen der Curaravergiftung einwirken zu lassen, aber mit Bestimmtheit haben wir niemals etwas Anderes als Beschleunigung der Sekretion gesehen. Wir kennen zwar einige Erfahrungen, auf welche ein man immerhin geneigt sein kann, an eine reflektorische Hemmungswirkung sensibler Nervenstämme bezüglich der Speichelsekretion zu glauben, aber es ist uns bis jetzt nicht gelungen, ein nie versagendes Experiment in dieser Beziehung vorzulegen. So wird beispielsweise von anderen Forschern erwähnt, dass in Fällen spontaner Speichelung, die vorher nicht zutreffend Verdauungsspeichelung genannt wurde, bei dem Hervorziehen von Darmschlingen aus einer Bauchwunde die Sekretion gestockt habe, und wir selbst haben mehremal eine ohne Anregung durch äussere Ursachen entstandene Sekretion aufhören sehen, als wir gewisser Zwecke halber eine Trachealfistel anlegen wollten. In einem Falle war die spontane Speichelung sehr erheblich, aber als die Einbindung der Kanüle in die Trachealfistel vollendet war, existirte keine Spur mehr von jener. Wir glaubten, die Erscheinung auf eine Zerrung des Vagus bei der fraglichen Operation beziehen zu sollen, da uns die Angaben über eine Hemmungsfunktion dieses Nerven bezüglich der Speichelsekretion bekannt waren. Als wir aber in drei bis vier anderen Fällen, in denen gleichfalls spontane Sekretion in beiden Submaxillardrüsen bestand, bei der Herstellung der Trachealfistel eine Zerrung des Vagus absichtlich vermieden, bemerkten wir ein gleiches Stocken der Sekretion, ja in einem Falle sahen wir den Stillstand schon eintreten, als wir kaum die ersten Schnitte durch die die Trachea deckende Haut angelegt hatten. Derartige Erscheinungen entziehen sich zur Zeit einer näheren Zergliederung, fordern aber zu weiteren Untersuchungen auf.

Wir haben dann zum Schluss noch beim Hunde eine Anzahl von Reizungen des oberen Stumpfes des durchschnittenen Vago-Sympathicusstammes, mit und ohne vorangegangene Trennung des Sympathicus, bevor er sich mit dem Vagus mischt, in Fällen nicht bestehender Submaxillardrüsensekretion vorgenommen. Unsere Erfahrungen über diesen Punkt können wir dahin zusammenfassen, indem wir sagen, dass sich unter diesen Umständen Sekretion wie bei Reizung sensibler Nerven einstellen *kann*, wie dies auch schon vor uns von einem Beobachter gesehen worden ist. Manchmal haben wir kein gutes positives Resultat bekommen. Dieser Theil der Untersuchung befindet sich also noch in einem unfertigen Zustande und soll demnächst hier in ausführliche Arbeit genommen werden.

Zum Schluss noch ein paar Worte über die mehrfach erwähnten spontanen Sekretionen der Submaxillardrüse. Weshalb wir dieselben als nicht mit der vermehrten Kohlensäurebildung nach der Nahrungsaufnahme im Zusammenhang stehend ansehen können, ist oben bereits angegeben worden. Eine Zeit lang hegten wir den Verdacht, dass sie dadurch zu Stande kämen, dass man bei den Operationen, welche zum Einlegen der Kanülen in die Wharton'schen Gänge nothwendig sind, den Chordafasern zu nahe komme und diese gewisse Reizungen erfahren möchten. Aber als wir sahen, dass man auch dann noch auf Fälle spontaner Sekretion stösst, wenn man die Einbindung der Kanülen möglich weit vorn vornimmt und somit den erwähnten Nervenfasern fern bleibt, konnte an den erwähnten Umstand nicht mehr gedacht werden. In manchen Fällen mögen die Stricke, deren man sich zur Fesselung der Thiere bedient, reizend auf die Mundschleimhaut wirken und die fragliche Sekretion hervorrufen. Doch kann auch dies in vielen Fällen nicht die wahre Ursache sein; denn mehrmals haben wir bei spontaner Sekretion die durch das Maul ziehenden Fesseln entfernt und die Sekretion bestand noch so lange fort, dass man endlich des Zuwartens müde wurde. Es bleibt somit kaum etwas Anderes als die Annahme übrig, dass sich mit den Aufregungen, in

Digitized by Google

welche die Thiere beim Fesseln gerathen, die Thätigkeit der cerebralen Speichelcentren verknüpft. In der That macht man auch im Allgemeinen die Wahrnehmung, dass je unbändiger sich die Thiere beim Aufbinden benehmen und je langsamer sie sich nach der Fesselung besänftigen, desto wahrscheinlicher eine spontane Sekretion zu gewärtigen ist.*

* Eine vollständige Darstellung des besprochenen Gegenstandes, nebst den nothwendigen Literaturangaben, findet sich in der demnächst erscheinenden Inauguraldissertation meines Schülers, des Herrn R. Buff, "Revision der Lehre von der reflektorischen Speichelsekretion." Giessen, 1887. — Wird auch abgedruckt im 12. Bande meiner "Beiträge zur Anatomie und Physiologie."

Digitized by Google

Digitized by Google

In Memoriam.

Alonzo B. Palmer, M.D., LL.D.

Digitized by Google

SECTION IX—PATHOLOGY.

PRESIDENT: ALONZO B. PALMER, M. D., LL. D., Ann Arbor, Mich.

VICE-PRESIDENTS.

ANDREW FLEMING, M. D., Pittsburgh, Pa..
JOSEPH COATS, M. D., F.F.P.S. Glasgow, Scotland.
CHARLES CREIGHTON, M. D., London, England.
JULIUS DRESCHFIELD, M. D., F. R. C. P. Lond., Manchester, England.
JAMES FREDERICK GOODHART, M. D., F. R. C. P. Lond., and M. R. L. S. Eng., London, Eng.
DAVID JAMES HAMILTON, M. D., F. R. C. P. Edin., F. R. C. S. Edin., Aberdeen, Scotland.
JOHN B. JOHNSON, M. D., St. Louis, Mo.
HENRY F. LYSTER, M. D., Detroit, Mich.
JOHN NORTH, M. D., Keokuk, Iowa.
JOSEPH FRANK PAYNE, M. D. OXON., F. R. C. P. Lond., London, England.
EDWARD O. SHAKESPEARE, M. D., Philadelphia, Pa.

SECRETARIES.

HERMAN M. BRIGGS, M. D., New York, N. Y.
ISAAC N. HIMES, M. D., Cleveland, Ohio.

COUNCIL.

Ezra P. Allen, M. D., Athens, Pa.
Nathan Smith Davis, Jr., M. D., Chicago, Ill.
Llewellyn Eliot, M. D., Washington, D. C.
Thomas J. Gallaher, M. D., Pittsburgh, Pa.
Daniel R. Hagner, M. D., Washington, D. C.
Wm. J. Herdman, M. D., Ann Arbor, Mich.
Chas. H. Hunter, M. D., Minneapolis, Minn.
Sam'l Ruff Skillern, M. D., Philadelphia, Pa.
E. M. Schaeffer, M. D., Washington, D. C.
A. S. Von Mansfield, M. D., Ashland, Neb.
A. F. Wheelan, M. D., Hillsdale, Mich.

Digitized by Google

FIRST DAY.

The Section met on Monday, September 5th, at 3 P. M., and was called to order by its President, Dr. ALONZO B. PALMER, who delivered the following—

ADDRESS.

GENTLEMEN:—In commencing the duties of assisting in the deliberations of the Section of Pathology of this International Medical Congress, to which I have the honor to have been called, you will permit me to invite your attention to some thoughts which suggest themselves as appropriate to this occasion.

Those who are now present as members of this Section are supposed to be workers, or at least advanced students in Pathology, and I shall venture only upon suggestions, and not attempt instruction. This department of knowledge must be considered as embracing subjects among the most fundamental, the most intricate, and the most scientific which are included in the broad and daily widening field of medicine.

You well know its importance, and how far from scientific, how exclusively empirical, how really inefficient or injurious, was much of the practice of medicine, even in the most enlightened hands, before pathology was cultivated in the light of modern science. You also know what wild and irrational speculations have been indulged, and what absurd *systems* have been constructed whenever, even up to the present time, the study of pathology as now understood, based upon anatomy, physiology, and chemistry, has been repudiated or neglected.

You know that the author of the most absurd of exclusive modern systems of medicine not only neglected the proper study of pathology, but actually repudiated it, and ridiculed the "pottering with morbid specimens" as useless or leading astray.

Taking into view chiefly methods of treatment, though the most modern methods, many men of intelligence and writers in popular periodicals deny to medicine the character of a science, not even allowing it to be what we must admit it is, an imperfect science. In fact, all the natural sciences are more or less imperfect; pathology certainly not more so than some others; but the denial of all scientific qualities to medicine, from respectable sources, though so unjust, has had an effect, and has lowered the dignity and general character of the profession in public estimation; and worse, has encouraged every species of quackery and deception.

We must look to the advancement of pathology and to a dissemination of some knowledge of its principles and methods of investigation among the people, to correct popular errors and prevent the adoption of absurd beliefs and practices.

Though certain useful facts have been known, and empirical knowledge in Medicine has been accumulating, since the time of Hippocrates, yet if we go back a single century, or even less, we shall find many crude notions prevailing among the most learned and orthodox of the profession; and a few centuries further back will bring us in contact with a very irrational jumble of medical statements, and with a marvelous array of inert and disgusting, if not injurious, agents then in general use.

Digitized by Google

The study of the intimate nature of disease—of accurate pathology—something beyond the mere observing of its external phenomena and the outer circumstances of its prevalence, was the first important step in strict medical science.

In order to a proper view of the present state of pathology, or of general medicine, we must not overlook the steps which have led up to its successful study and the aids which have been afforded by all the modern arts and sciences.

Though there is much that is interesting in ancient modes of thought and expression respecting nature, and in the mediæval systems of speculative philosophy, they had little influence in establishing exact science, and it is not till since the time of Bacon, whom we should not forget (and whom we are not likely to forget while Englishmen have a voice), that scientific methods have been prevailing; and it is especially to the *present philosophical views of nature and her fixed laws*—the general trend of modern science—that we are indebted for the present state of pathology and what we hope for it in the future.

As to its special cultivation the Continent of Europe is most conspicuous for the earlier and more persistent labors in original experimental investigations; yet Great Britain has done much most excellent critical and cautious work, while America has been too much occupied in what are called "practical matters," and with applying the scientific discoveries of facts and principles to useful purposes, to appear otherwise than as following at a respectful distance in the more elaborate investigation of pathological subjects; but she is doing something at the present, as we may see before our meeting closes, and she has a hopeful future, if not so long and brilliant a past.

At this meeting of the Congress, in consequence of the broad intervening Ocean, a very large proportion of its membership will be Americans, and there will doubtless be greater activity and a larger number of papers in the Sections of Practice of Medicine, of Surgery, of Obstetrics, and the Specialties, than in those of the foundation sciences, upon which the more showy superstructures, if substantial and abiding, must be based.

But Pathology is steadily pushing its way to the front, even in America. Pathological laboratories are being established and furnished with apparatus and men, and experimental investigations are being pursued; and ere long, if not so much here and now, we hope to be heard from, joining (not over modestly, it may be presumed,) in the chorus, if not performing the solos, or wielding the baton. Some things American have already been done which are not mere imitations, and there will be more to come.

With the increase of the number of men of leisure and scientific tastes, and with the endowments of professorships for the *advancement*, as well as the teaching of science, it will, we hope, be demonstrated to the scientific world, as it has been to the business and practical, that the American mind, an offshoot from the European, is not destitute of originality, ingenuity, and the power of persistent application, which are the main elements of scientific success. In the meantime, for our consolation, we can repeat the aphorism, "All that is not behind us is before us," and the desirable will be reached if with sufficient energy we move forward.

But Pathology, wherever cultivated, is an offspring, or rather a part, of general science, is under the universal "reign of law," and has advanced and will continue to advance with the other sciences. A knowledge of every department of nature—of Matter and Force, of forms and properties, of the persistence and correlation of the forces, of sensible and molecular motion, of the particular chemical as well as physical properties of matter, of the great principles of heat, light and electricity,

Digitized by Google

and especially a knowledge of that crowning department of nature where *Life* comes into action, the science of Biology, as is too well known to require me to say, has wonderfully developed in recent times; and it would have been strange if pathology, which is but a part of biology, had not partaken in the common movement.

Since the conception has become common in the profession that the laws of nature are general and uniform—that pathological processes are but modifications of physiological—that disease is but a change of proper actions and conditions under unfavorable influences, that its processes are moved by the same natural forces, governed by physiological laws, simply modified by injurious agencies, and that the results of wrong actions in perverted functions and structure have their likeness in normal actions—in the natural processes of growth, renewal, decay and death—since this, pathology has to a large extent lost its obscurity and mystery, and the whole science and art of medicine has become at once more simple, more intelligible, more rational and more successful.

But I must not detain you longer by these general and trite remarks. I will, however, venture a few further suggestions upon some matters of detail which seem to me to justify a passing mention.

Speaking from the standpoint of the great body of the profession—those engaged in distinguishing and treating diseases—in administering to the sick and injured—it may not be presuming to refer to some things that are needed by them from the working, laboratory pathologists, which it is possible in their special absorbing pursuits, they may not always bear in mind.

The practitioner needs, of course, to learn of the pathologist the structural and chemical changes which occur in disease. This knowledge he needs, the better to understand the living processes he witnesses, and to make a proper diagnosis of his cases. He also needs to know from the pathologist the causes of particular diseases, in order, if possible, to avoid or counteract them.

And here we can but be reminded of the essential service which pathology has already rendered to preventive medicine, now, perhaps, the most practically useful department of the profession. During the long past, while diseases were completely enshrouded in mystery, or were regarded as produced by some supernatural cause—by some evil spirit, by the displeasure of the gods, or by a special dispensation of Providence—incantations, exorcisms, sacrifices and invocations were the preventive measures employed. The real causes being unknown, proper efforts were not made for their removal, and all forms of disease, especially devastating epidemics, prevailed.

Since pathology has revealed the nature of diseases and so many of their causes, true preventive measures have followed, and the value of human life in all enlightened countries has largely increased. Even within a very few years the bills of mortality have wonderfully changed, as public sanitation has received popular attention and legislative action. In England, for instance, within the last thirty-five years the annual death rate from continued fevers has been reduced from about 1000 in 1,000,000 of the living, to less than 300; and the proportion of deaths from all causes will be as wonderfully diminished when private and personal hygiene receives even as much attention as has been given to municipal health measures.

When pathology is still better understood by the profession, and its leading principles reach the masses of the people, and the lessons it teaches are fully carried out in practice, the continuance and the enjoyments of life will be enlarged beyond all former conceptions. Pathology, then, is a subject in which humanity is deeply concerned.

But to the physician it is quite as interesting, if not as necessary, to know how

Digitized by Google

special causes produce their effects, and the particular actions which occur in their production. At the bedside and in the consulting room it is with the living phenomena of disease he chiefly has to deal. It is these morbid living processes that he desires to modify or remove; and he can go about his work more rationally if he knows the particular character of the processes that are going on. For instance, it is interesting and important to know that a certain microbe or ptomaine is a factor in the production of a particular disease, but it is also interesting to know how the microbe produces such an effect, and the particular process that occurs in its production. I am aware of the obscurities which exist in these matters, and the great and often insurmountable difficulties of this part of the investigation; but our pathological knowledge is not complete and our therapeutical measures are not all entirely rational until this is understood. It is not reasonable to insist upon impossibilities, but the investigating pathologist should have this part of his function prominently in view.

"Pathology," as was recently said by one of our Vice-Presidents—Dr. Goodhart—"is something more than morbid anatomy; it is an attempt to interpret morbid changes; an attempt to unravel, not so much the conditions themselves, as how they came about—the processes which produce them, and the successive steps by which the result is reached."

Allow me to add, indeed it goes without saying, that pathology should deal with morbid functions as well as structural changes, and that it becomes the complement to physiology, as well as to normal chemistry and anatomy. Indeed, there are morbid functional processes which, with our present means of investigation, have no morbid anatomy or morbid chemistry to show. But the functional changes aside from structural, are too frequent and too important to be in any degree neglected.

The nervous activities, their centres and connections, their control over secretions, circulation and nutrition, over production and growth, over waste and decay, normal and abnormal, the sympathetic relations of different parts of the brain to each other, and of the different and distant parts of the body to other parts through the nervous system, and through blood and lymph supply (the latter modified by nervous action), all these, in their relation to the production of disease, constitute a subject at once vast in its extent, obscure in its nature, difficult to study; and at the same time most important to be understood.

A department of this subject which needs elucidation, and belongs equally to the physiologist and pathologist (and everywhere physiology and pathology are intimately interwoven), is that of the *excitor* and *inhibitory* functions of the nervous system, their actions upon each other and upon the various parts and functions of the body—and particularly the *pathological relations of inhibition.*

I am aware that no little attention has been given to the general subject of inhibition by very able observers, experimenters and writers, such as Lauder Brunton, Brown-Séquard, Rossback, Gaskell, Herrmann, Panum, and many others; but it has been treated chiefly as a physiological problem, though its great importance to pathology, etiology and therapeutics is acknowledged. Such questions as the following have been chiefly discussed. What is the essential nature of inhibition? What is the mode of its action? Are there special and exclusive inhibitory centres and nerves, or do the same centres and nerves produce either excitation or inhibition, according to the state, at the time, of the nervous matter, or the organs to be acted upon? Is the inhibitory function exercised in some cases by nerves that have also excitor function, and in other cases by exclusively inhibition centres and nerves? What is the influence of the excitation of one function upon the inhibition of other

Digitized by Google

functions? What is the physiology of reflex action, and what are the laws of sympathy as connected with inhibition? All these are important questions and have relations to pathology; many of them are still undecided and require further investigation. But, however they may be settled, or whether or not they must be left open questions, there are, somewhere or somehow, two forces existing in living parts, or two processes going on, perhaps in each cell or group of cells, one excitory and the other inhibitory, and by the proper balancing of these, normal functional activity throughout the system results. The unbalancing of them causes disease.

The particular effects of morbid agencies on these balancing forces, especially on the inhibitory functions, in the production of particular forms of disease, have not been so fully investigated. Among these agencies are the poisons taken in from without, and those developed within the system—both the common and specific poisons—and, in fact, all varieties of morbid impressions, of whatever character, which are made upon these centres and nerves; and these influences and the manner of their operation upon the excitor and inhibitory functions should particularly interest pathologists.

It is not only scientifically interesting but practically important to know, when an impression is made upon the nervous system which results in disease, whether the derangement is produced by impressions upon the excitor or inhibitory functions. When an increased action results, it is important to know whether it is produced by paralyzing inhibition or by stimulating excitation. If by paralyzing inhibition, is the agent a *stimulant* in any proper sense, and will its effects in increasing action be permanent, however constantly the agent is applied?

No organ has been so much studied in relation to excitation and inhibition as the heart. The vagus has generally been considered as exercising the inhibitory function, while the sympathetic acts as an excitor upon the organ. Compressing or otherwise arresting the action of this vagus nerve, it is well known, generally increases the frequency of the heart's action, though it may not its force, and thus its inhibitory function is declared. But exceptions have been observed to this effect, which have been explained by the alleged fact that two sets of fibres, inhibitory and excitor, are found in the vagus nerve. They, of course, come from different centres, and each centre and nerve has its own selective affinities for excitants and depressants.

These and other facts show the complexity and difficulty, as well as the importance, of this subject; but complexities and difficulties call for efforts at solutions, especially when important practical results depend upon such solutions. For illustration, it may be mentioned that at present we are not entirely in the light as to whether various poisons or therapeutical agents, such as the narcotics or the so-called narcotico-stimulants, produce those effects which appear to be stimulant by increasing excitor activities, or by paralyzing inhibitory functions. For example, the direct effects of the alcohols upon the tissues of the isolated heart were found, by Drs. Ringer and Sainsbury, to be clearly paralyzant and not excitant, in every stage and degree of their operation. (*Practitioner*, London, May, 1883.) The same was found by Dr. Martin, of Johns Hopkins University, in his experiments with common alcohol upon the heart of the dog. Yet, from certain sensations felt and other temporary effects observed, alcohol has been regarded as a cardiac stimulant; and the statement to this effect, handed down from the darkness of the past, still holds its place in the common language of the profession and the people. The question certainly fairly arises as to whether the apparent excitation, when it occurs, is not dependent upon the greater paralysis of the inhibitory, rather than the stimulation

Digitized by Google

of the excitor function; and whether alcohol should not be called a heart depressor rather than a heart stimulant.

Dr. Norman Kerr, of London, explains what is the matter with drunkards as follows: "There is an abnormal cerebral condition, a dynamical and psychical disturbance of the brain and nerve function, a real departure from sound health, which is itself a pathological state with, in all probability, its post-mortem equivalent in hyperplasma of the neuroglia."

It may well be questioned whether any of the substances with distinctly narcotic properties—substances whose ordinary full effects are depressant—can be regarded as stimulants in any stage or degree of their action. A more exact knowledge of the physiology and pathology of this subject might lead to important modifications in therapeutics and hygiene.

But this is only one of many instances which shows the necessity of greater knowledge of the influence of inhibition in deranging proper activities. These remarks are made as suggestive of the course of future investigations, rather than with the object of establishing any particular theories or practical views.

But the properties, actions and relations of the most ultimate known particles of the body—the protoplasm and the cells, fixed and wandering—constitute another subject requiring further elucidation. The grosser and more perceptible actions of organs in health and disease have been properly designated as the "outer court" of the science of life. This outer court, it is true, must be traversed, its passages understood, but the inner court, within the incasement of the more perceptible physical and chemical conditions, is the more fundamental part of physiology and pathology. When we fully penetrate to this central part and understand its nature and all the laws which govern it, our physiological and pathological knowledge may then, but certainly not till then, become complete. "Pathology, as distinguished from physiology, is some deeper strata of phenomena brought to the surface by the upheaval of some catastrophe"—phenomena produced by some injury or perverting influence acting upon the ultimate elements. A new phase is then presented, new combinations occur, but the same vital laws are present, requiring to be studied in the same general way.

It is at this central point that injurious influences produce their effects, and here the relations of poisons, organic or inorganic, living or chemical, bacteria, ptomaines, leucomaines, retained excretions, or what not, are to be studied. The contests between bacteria and the cells, alleged to have been seen by Mecknikoff, the destruction of some tissue cells by other tissue cells, and the attacks of the living cells, both the more fixed and mobile, upon various intruding irritants—these contests, followed often by the disturbing effects of a battle—are matters that require further investigation. Here, it seems to me, is the great field of future achievements in pathology. Upon this field some of the papers awaiting our consideration will enter.

Of the recent achievements in pathological science, those of Koch and Pasteur, and their colaborers, are most conspicuous. Whatever may be thought of some of their conclusions, it cannot be denied that they have rendered very important services to pathology. The discovery of the tubercular bacillus, which is now so generally regarded as having an intimate, if not a necessary, causative relation to tubercular phthisis, at present seems to be a permanent addition to our pathological knowledge; and the investigations respecting the comma microbe as the cause of cholera, though less conclusive, have instituted a method of inquiry which, it is hoped, will result in the establishment of positive truth as to the etiology of that disease.

The labors of Pasteur in regard to the different infectious diseases of the lower

Digitized by Google

animals, and the means of their prevention, have opened up new fields of inquiry, developed new facts, and established some new principles; and his more recent experiments with the poison of rabies have filled the whole world with some knowledge of this subject. Lister's name should be mentioned because of the practical results which have followed his investigations. But the success of these three distinguished men should not cause other laborers in similar fields to be overlooked. It might, however, seem invidious to name others without naming all who have contributed to the present stock of knowledge on the subject of infections.

The inoculation experiments for destroying susceptibility to certain infectious diseases, in which, in the matter of rabies, Pasteur has been the central figure, have broadened a field not new, which may be followed by consequences to humanity greater even than we have heretofore thought.

The inoculation with the virus of smallpox, for the purpose of modifying and rendering milder that disease while destroying susceptibility to future attacks, was introduced into Europe and this country, from the East, a century ago. The reason for the milder effects of the poison when introduced by local inoculation instead of the natural way by the lungs, may not be clear; but its more speedy operation, affording less time for its multiplication in the system, and possibly its introduction in smaller quantity, may account for it. Its practical benefits were decided, and it doubtless would have continued in use had not the discovery of Jenner been made. But this inoculation of smallpox was the first grand illustration of a principle and a practice which are widening before us.

Vaccination was the next example, the glorious results of which are the standing boast of our profession. This involves the same principle as the smallpox inoculation, as essentially the same virus is used, modified, however, and rendered milder, by passing through the system of the cow. It is introduced in a similar manner, and requires a shorter period for the development of its effects. A much milder form of disease is induced, but it has the power, as a rule, of preventing another attack of this non-recurrent affection.

Inoculation with the diluted virus of syphilis was at one time practiced, it was said successfully, for the purpose of destroying the susceptibility to that disease. Then followed the experiments upon the lower animals with which Pasteur's name is particularly connected, until, according to the recent report of the British commission sent out to investigate the subject, the *demonstration* comes that animals may be made insusceptible to the poison of rabies by a series of inoculations with the diluted and weakened virus.

Whether human beings bitten by rabid animals have been prevented from having the disease or not, the fact that, commencing with weak and modified preparations of the poison and going on to the introduction of stronger, destroys the susceptibility to the strongest, is of the greatest interest; and the additional fact, which is just coming to the light, and is to be reported here, that similar inoculations with the virus of the rattlesnake cause a similar insusceptibility to its effects, shows that the principle is a more general one than was before known.

The questions here suggest themselves: Can it be made applicable to other infectious diseases? to scarlatina, measles, diphtheria, cerebro-spinal fever? to typhoid and typhus? to cholera? and, above all in importance, to phthisis? Will the principle explain the facts of acclimation?

So far this principle has only been proved to apply where the poisons artificially introduced are identical with those naturally taken. Will it apply to poisons that are similar but not the same? and to what extent will it apply to poisons that do not

Digitized by Google

multiply themselves in the system? To show that the principle has some degree of application to non-multiplying poisons, I need but mention opium, tobacco, arsenic and alcohol, as illustrating the modifying effect which the repetition of doses and the establishment of habits have upon the action of these agents. The comparative insusceptibility produced by habitual use and the modification of actions, especially upon the sensations, should be taken into the account in judging of the effects and determining the avoidance or the use of these articles.

The widespread indulgence in narcotics, especially in tobacco, alcohol and opium, and the threatened introduction of cocaine, should lead the pathologist to inquire into the physical and mental conditions of those who become habitués in the use of these substances. Whether there is any substantial good in their moderate use (except as therapeutical agents), or in their habitual or even occasional use as matters of luxury, to compensate for their evils or not, it is certain that the amount of injury which the narcotic habits are inflicting throughout the world is enormous and incalculable. Can the habitual use of these articles, however moderate, be physiological and well, when their abuse is acknowledged to be so frequent, and their general and ordinary effects are so pathological and in every way so injurious? This constitutes one of the most urgent practical questions of our time, and the medical profession—physiological and pathological science—must be looked to for the proper answer.

In the general subject of the production of disease by poisons, there is a field for further investigation which, in its extent and importance, is certainly second to none other. The interests of science, no less than the calls of humanity, demand its thorough exploration, and he who penetrates it furthest will stand high among the distinguished pathologists of the future.

But all diseases are not specific and infectious, and all wrong actions of the system are not produced by material and recognized poisons acting upon the individual system. Various diseases and a great amount of suffering arise from constitutional peculiarities—from a want of proper balance in the different functions, leading to deficient, excessive and irregular actions and their results. Circulation, secretions, growth, wasting and renewal—explosions of nervous and muscular force—become deranged, producing various morbid consequences; and all these must depend upon wrong actions of the ultimate cells and tissues. These may be disturbed, temporarily, periodically, or more permanently, by a variety of causes, external and internal, even by immaterial influences—by mental impressions, by emotions and thoughts. Some of the wrong tendencies are congenital and hereditary, and some are acquired. The exhaustion of fatigue and the over-indulgence of passions; the changes of age, physiological or premature, and, indeed, every influence disturbing the most healthy actions, are matters demanding the attention of the pathologist. His work extends in every direction, to the very boundaries of human imperfection and suffering, and, with the rest of the profession, he should ever bear in mind that the great aim of his calling is not merely to learn the morbid conditions which affect humanity, but to find means for relieving them; that the chief object in studying diseases is to *prevent*, *alleviate* and *cure* them.

In pursuing investigations, the mind should not be too narrowly fixed on the special alterations that characterize distinctive nosological affections. The patient's general surroundings, his habits, ancestral and personal, and everything that can influence him, should be considered. These, at least, modify results.

A man is greater than his diseases, and, in administering to him, we should remember that we have a person to care for and not simply a disease to contend with and subdue. In subduing the disease the patient might not be spared.

Digitized by Google

A division of labor in our profession, as in every other complicated occupation, is necessary for the greatest efficiency: but that devotion to these pursuits which makes one a great pathologist need not, indeed cannot, make him a small man. The subject is great enough to elevate the man, and from an elevated position other things can be seen. The benevolent, the humanitarian, the Christ-like character of our profession should never be lost sight of, and it never is by those who labor with warm sympathetic hearts and with a consciousness of their great responsibility. If, as a poet* has recently said, the doctor is "the flower of our civilization; and when the present stage of man [or of that civilization] is done with, and only remembered to be marveled at in history, he will be thought to have shared as little as any in the defects of the period, and most notably exhibited the virtues of the race"—we must not only pursue our science and art for humanity's sake, but must cultivate and exhibit those personal qualities which all appreciate and approve.

* Robert Louis Stevenson.

PLATE I.

TAYLOR ON THE CRYSTALLOGRAPHY OF FATS.

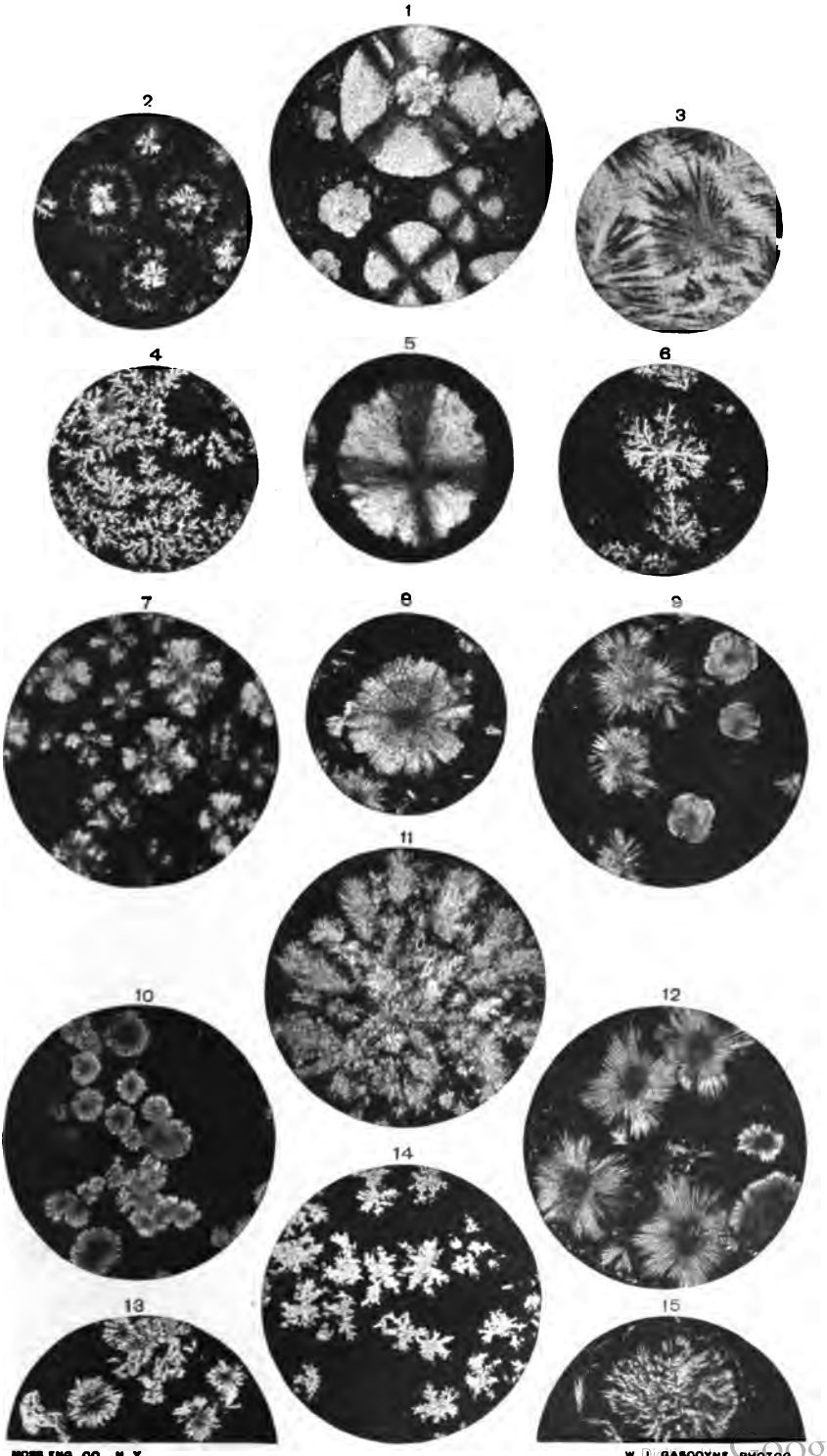
DESCRIPTIVE LIST.

Fig. 1.—Crystals of Boiled Butter. (Shorthorn Durham.)
Fig. 2.—Crystals of Boiled Dog Fat. Coach Dog.
Fig. 3.—Crystals of Boiled Lard.
Fig. 4.—Crystals of Boiled Monkey Fat.
Fig. 5.—Crystals of Boiled Butter. (Jersey.)
Fig. 6.—Crystals of Boiled Beef Fat.
Fig. 7.—Crystals of Boiled Human Fat. Man, Kidney. (Inebriate.)
Figs. 8, 9, 10, 12.—Crystals of Boiled Human Fat. Kidney. (Dementia.)
Fig. 11.—Crystals of Boiled Duck Fat. Erismatura Rubida.
Fig. 13.—Crystals of Boiled Seal Fat.
Fig. 14.—Crystals of Boiled Cat Fat. (Domestic.)
Fig. 15.—Crystals of Boiled Cotton-Seed Fat.

Digitized by Google

PLATE I.

CRYSTALLOGRAPHY OF FATS.



MOSS ENG. CO., N. Y.

W. H. GASCOYNE, PHOTOG.

CRYSTALS PREPARED BY

THOMAS TAYLOR, M. D.

Digitized by Google

Digitized by Google

[ABSTRACT.]

THE CRYSTALLOGRAPHY OF FATS AND THEIR COMPOSITION, CONSIDERED IN RELATION TO HEALTH AND DISEASE.

LA CRYSTALLOGRAPHIE DES GRAISSES ET LEUR COMPOSITION, CONSIDÉRÉES PAR RAPPORT À LA SANTÉ ET A LA MALADIE.

DIE CRYSTALLOGRAPHIE DER FETTE UND IHRE ZUSAMMENSETZUNG, MIT RÜCKSICHT AUF GESUNDHEIT UND KRANKHEIT BETRACHTET.

BY THOMAS TAYLOR, M.D.,
Washington, D. C.

MR. PRESIDENT, AND MEMBERS OF THE SECTION:—I have, for several years past, devoted much of my time to the study of the crystalline forms of animal fats in general, having in view their ultimate classification.

In crystallizing fresh butter fat, I subject it, as nearly as possible, to a uniform treatment, as follows: About two ounces of the butter is placed in a porcelain capsule, in a water bath at a temperature of 212° F. (100° C.) until its water is evaporated. It is then strained through a clean cloth, to separate the casein, brought to the boiling point (212°F., 100° C.) for a moment, over a spirit lamp, and then slowly cooled for a period of twelve hours, at a temperature of about 60° F. (15.56° C.), when it will appear granular to the naked eye, and if viewed under suitable powers of the microscope, with polarized light, the field in view will present numerous crystals, the forms in some cases varying within certain limits.

I have, in the course of these investigations, through the courteous coöperation of breeders and owners of valuable herds of cattle, obtained samples of the butter of registered milch cows of different breeds, in various parts of the United States, in order to ascertain whether butter crystals are modified in form, color, etc., by breed, or by marked changes of food, climate, or other conditions. Although these investigations, thus far, are comparatively limited, the practical results have a substantial value, as shown by my present exhibit of photographs of their crystallized fats. In two instances I have found marked modifications of the butter crystal, produced, it would seem, by giving the cows a peck of cotton-seed meal each day in addition to other food. The cows were of the Jersey and Ayrshire breeds, respectively. The butter was of a lardy consistency, its secondary crystals very large, resembling, in some respects, crystals of beef fat rather than those of butter. The butter crystals of the Shorthorn breed are generally very smooth as compared with other types, but seem to have a tendency, under certain conditions, to assume the broken outlines characteristic of butter crystals of other breeds.

Animal fats other than butter should be rendered at a temperature of about 300° F. (148.89 C.) in order to separate the fat from the tissues, and should be strained and cooled, as in the case of butter. If a fat is firm to the touch when congealed, it should be brought to the consistency of butter by remelting it with a small quantity of cotton-seed oil, at a temperature not exceeding 212° F. (100° C.) This treatment is necessary, as normal crystals do not form in hard fats.

Certain foods, it is well known, affect the quality and quantity of milk and butter, and necessarily of the fats deposited in the tissues. This results from the food containing, in varying proportions, glycerides of the fatty acids which enter into the composition of these substances, or yielding them in varying proportions under the action of animal ferments and of the organs of secretion. It is to the varied proportions of the fatty compounds that I attribute the differentiation of their crystals. I have observed that the fat of several aquatic animals boils like butter, owing to the large amount of water contained. Milch cows and swine on the same class of feed, secrete a fat which varies materially, owing to some functional differences in the organs of secretion; the one is a soft lard, the other a hard tallow.

Digitized by Google

I find that the fats of animals generally differ very much in health and disease, as to consistency, color, odor, drying qualities, etc. "Loon" fat has the odor of fish oil and when boiled becomes a drying oil. In some fishes an almost pure olein prevails in certain tissues, while in other tissues of the same fish solid fats abound.

In the prosecution of this inquiry, I have observed, incidentally, that the fat of several monkeys, victims of consumption, when rendered, strained and cooled at a temperature of about 70° F., according to my usual method of treating fats in order to crystallize them, exhibited but a trace of the solid fats; the mass consisted principally of olein. From this fact my attention was directed to the consideration of the fat of human beings, subjects of emaciating diseases from whatever cause. I have tested the fat of three consumptives, each of whom was greatly emaciated. The fats were crystallized in the usual manner; boiled, strained and cooled at 70° F., and when examined, were found to be nearly destitute of solid fats, olein largely predominating.

While the cases cited are insufficient data for determining what may be the invariable condition of the fats of emaciation, I deem the facts thus far observed worthy of attention, and I propose to continue these researches in order to ascertain whether the conditions I have stated are constant. If in the animal economy it can be shown that there is a greater tendency to oxidize the solid fats than the oil, in cases of high temperatures, valuable information will be gained.

With regard to the composition of fats in general, they consist of three distinct fats, viz: olein, palmitin and stearin. These are known as the glycerides of the fatty acids, oleic, palmitic and stearic. Early chemists applied the term margarine to a substance which they believed to be a simple fat, but modern chemistry has demonstrated that this substance is merely a combination of palmitin and stearin. It has been demonstrated that the fat of milk and butter contains but a trace of stearin, and that it is composed mostly of palmitin and olein, and it is stated on high authority, that the fat of man is of similar composition.

In my investigations relating to the forms of animal fats, I have observed that the crystals of human fat have a marked resemblance to the crystals of milk butter, suggesting the importance of its use as the proper form of fat for man, as superior to fats which consist largely of stearin, such as some oleomargarines I have tested. According to modern chemistry, stearin requires a temperature of 144° F., to melt it, and each succeeding melting requires a still higher temperature. For this reason, the fats in common use cannot be as well adapted for the sick as butter, which melts at blood heat; therefore, I think it might be well to use pure butter as a substitute for the various forms of olein now in use, in cases of emaciation. For this purpose, I would suggest that the butter used should be boiled and strained, to remove its casein, the casein of milk butter frequently proving very indigestible and unbearable to a weak stomach.

Milk, the natural food of man, contains all the essential proximate principles necessary to build up a perfectly healthy human being; but the butter fat of milk contains but a small proportion of nitrogenous matter as compared with the milk from which it is made; therefore, butter of itself would not sustain life, but for a brief period.

It being desirable to make sure that the diet contains a proper amount of nitrogenous matter in an acceptable form, the use of peptonized beef, in other words digested beef, in connection with butter so treated, would prove favorable to the sustenance of the vital tissues.

In the oxidation of fats in the tissues, water and carbonic acid are formed, factors favorable to recovery, as by an increased elimination of these conveyers of heat, products of fat oxidation, the abnormal temperature of the patient may be reduced to a condition approaching the normal, and this in a higher degree theoretically than would take place in the use of alcohol for the same purpose, since a pound of alcohol contains but 52.17 per cent. of carbon, while the solid fats contain about 57 per cent. of carbon.

Digitized by Google

PRESSURE PARALYSIS OF POTT'S DISEASE, TOGETHER WITH THE REPORT OF A CASE AND PRESENTATION OF SPECIMEN SHOWING PRESSURE OF THE CAUDA EQUINA, ACCOMPANIED BY SECONDARY ASCENDING DEGENERATION OF THE POSTERIOR COLUMNS OF THE SPINAL CORD.

PRESSION ET PARALYSIE DE LA MALADIE DE POTT, AVEC LE RAPPORT D'UN CAS ET LA PRÉSENTATION D'UN SPECIMEN DEMONTRANT LA PRESSION DE LA QUEUE DE LA MÖELLE ÉPINIERE, SUIVIE D'UNE DÉGÉNÉRATION ASCENDANTE SECONDAIRE DES COLONNES POSTÉRIEURES DE LA CORDE RACHIDIENNE.

DRUCKPARALYSE DES POTT'SCHEN ÜBELS, NEBST BERICHT EINES FALLES UND DEMONSTRATION DES OBJECTS, WELCHES DRUCK AUF DIE CAUDA EQUINA UND AUFSTEIGENDE DEGENERATION DER HINTERSTRÄNGE DES RÜCKENMARKS AUFWEIST.

BY GEO. R. ELLIOTT, M.D.,

Instructor of Orthopedic Surgery, New York Post-Graduate Medical School and Hospital.

The object of this paper is to determine the mechanism of the lesion or lesions which give rise to pressure paralysis, limited to that form of paralysis which complicates Pott's disease

ANATOMICAL CONSIDERATIONS.

It may not be out of place to call attention to the following anatomical points :—

1. The spinal cord reaches only to the lower border of the first lumbar vertebra.
2. The spinal cord only partially fills the spinal canal.
3. The cord swings in the spinal canal, insulated by means of the arachnoid fluid which surrounds it.
4. The cord is much nearer the anterior wall of the canal than the posterior, held in position by means of the anterior nerve roots.
5. The tracts conducting motor impulses are, generally speaking, more superficially placed than those transmitting sensory impulses.

It further may not be out of place to state that paralysis in Pott's disease bears no relation to deformity; a spinal column may be bent at nearly right angles without giving rise to any paralytic symptoms, while in another case where no deformity exists complete paraplegia may be present.

PATHOLOGY.

In order to arrive at an intelligent understanding of the pathology of the lesion, I cite briefly the history of the carious process which is the exciting etiological factor. It matters little whether we accept the tubercular theory or not. The process is one insidious in its onset and slow in its progress. Cornil and Ranvier, in comparatively recent investigations, show rather conclusively that the disease begins not as an inflammatory process, but by "a fatty degeneration of the bone cells; the result a destruction of same, and consequent inability on their part to perform their functions as agents of nutrition. The osseous trabeculæ, killed by the death of their cell elements, form so many small, foreign bodies, which determine suppurative inflammation around themselves. The medulla becomes vascular and adipose cells disappear, and are replaced by embryonic cells, and suppuration is established. The bone cells which have escaped fatty degeneration become active, the osseous substance around them is dissolved, the necrosed trabeculæ becomes free, and granulations or fungosities are formed from embryonic medulla. Inlets of osseous tissue become necrosed oftentimes. These are dislodged by granulation and carried away by suppuration. In their place are left irregular cavities."

Digitized by Google

Here, then, we have a suppurative process in progress which is usually confined to the anterior portion of the bodies of the vertebræ. These portions giving way permit the vertebræ to fall together. The disease creeps dorsal and destroys, to a greater or less extent, the posterior common ligament. The latter, dissociated, allows the pus to pass from the vertebræ. The dura mater and the peri-dural tissue are aroused to inflammatory activity. The external surface of the dura mater becomes covered with granulations of a fungoid character, and infiltrated with caseous pus. This condition passes under the name of *pacchymeningitis externa caseosa.* This inflammation shows no tendency to extend beyond the site of the vertebral disease. It is limited. In a cross section we can see how the diseased condition abruptly gives way to the healthy membrane. The membrane may be infiltrated throughout its entire thickness with embryonic cells, or simply on its surface. Cornil and Ranvier* have found tubercular granulations in different stages of evolution in the thickened dura mater, even on the internal surface.

Up to this point we can safely say the pathology stands undisputed. All pathological findings bear evidence of this. In the large majority of cases of spinal caries the pathological chapter closes here, the products of inflammation remaining imprisoned, ultimately to be absorbed and replaced by reparative material. Frequently paralytic symptoms manifest themselves, and here the pathology is by no means settled.

Ollivier† was among the first to describe at all carefully the changes which take place in the cord as a result of slow compression. He cites numerous cases where the membranes were found thickened and the cord softened without evidence of further disorganization. His findings were verified and described later by Louis.‡

These writers and many followers who have described Pott's disease, have dwelt upon the mechanical point as the cause of the paralysis. From time to time cases were reported where the cord seemed harder than natural, a condition not touched upon by these writers and, consequently, no explanation offered. That the cord should become injured, as a result of crumbling and broken vertebral bodies, which led to the development of marked angular curvature, was considered but a natural sequence. This idea became so thoroughly fixed that many books, even to this day, refer only to bone as the pressure cause of the paralysis.

It was noticed, however, that very commonly no paralysis followed in cases where the deformity was marked. The spinal cord was even seen to be bent to a right angle and still no paralytic symptoms manifest themselves. That marked deformity can occur without the cord becoming compressed, is also observed in lateral curvature, where sometimes the deformity assumes a frightful aspect.

In the words of Fagge,§ "every pathological museum contains specimens which show that the spinal canal generally retains its full width, however much its direction may be altered. Further, paralysis was seen to complicate caries where careful examination of the spinal column gave no evidence of deformity. These observations called for some explanation other than the current view.

The middle of this century, which inaugurated an active interest in the physiology and pathology of the great nerve centres, produced men who sought a solution of the problem. Among others, Charcot and his pupils made investigations and published to the medical world their conclusions. To Michaud‖ can be credited, I think, the pio-

---

* "Manual of Pathological Histology," Second Edition, 1882, Vol. I, p. 607.

† "Ueber das Rückenmark und seine Krankheiten." Uebersetzt von Dr. J. Radius, Leipzig, 1824, p. 202.

‡ "Mémoire sur l'état de la moelle dans la carie vertébrale." Paris, 1826.

§ Fagge, Vol. I, p. 410.

‖ "Sur la Méningite et la Myélite dans le mal vertébral." Paris, 1871.

Digitized by Google

neer article upon this form of paralysis. These authors and investigators contended, and apparently demonstrated, that the paralysis in these cases was not directly due to a mechanical lesion, but to a myelitis, the result of the mechanical lesion. Michaud held that myelitis is invariably the lesion, and even went so far as to claim its existence in cases giving no evidence of paralysis.

This explanation of the cause of the form of paralysis first described by Michaud and Charcot,* and subsequently supported by Bouchard† and Corjon,‡ was accepted and verified by Leyden § in his microscopical examinations of compressed cords.

NATURE OF THE PATHOLOGY OF MYELITIS.

Great controversy has now long been in vogue regarding the exact nature of inflammatory processes of the cord. So loosely has the term myelitis been used, that various affections of the cord have been called myelitis—polio-myelitis, ascending and descending degeneration, various kinds of softening—all these have been described by observers under the head of inflammatory lesions. Recent pathologists have come to look upon the disease myelitis as one giving rise to certain unmistakable and ever present findings. Stricker, Leyden, Joffroy, Schultze, Strümpell and others, have found the following pathological conditions, which they consider pathognomonic of the inflammatory lesion.

1. Increased size of nerve elements. Axis cylinders enlarged, or without the sheath of Schwann.

2. Swelling of the interstitial tissue and increase of same, according to age of the process.

3. Increased number of round cells in the connective tissue and around the blood vessels.

4. Enlargement of Deiter's cells.

The later stages characterized by hyperplasia of the connective tissue.

Erb practically coincides with the view of these authors, and their observations have been verified by numerous competent and careful observers, both here and abroad.

The progress of the lesion in cases of slow compression is, as a rule, insidious, and to reconcile the clinical symptoms with the lesion the interstial form of myelitis is believed to predominate. This is characterized by an increase of the neuroglia or connective tissue, similar to that occurring in other organs, the parenchyma becoming secondarily affected. The interstitial tissue becomes increased together with proliferation of the nuclei, great thickening of the walls of the blood vessels and distinct atrophy of the nerve fibres. A later stage is characterized almost exclusively by the presence of connective tissue, among the fibres of which run isolated nerve fibres.

In support of the inflammatory theory, are usually found post-mortem considerations, showing the cord at the site of compression converted largely into connective tissue with thickened blood vessels and sparsely distributed changed nerve fibres and granular bodies.

Further, in one case examined by Michaud,‖ where the compression lesion had not as yet produced paralysis, but where sensory disturbances in the form of fulminating pains were complained of by the patient, the microscope revealed increase of connective tissue in the posterior root zone.

The inflammatory theory of compression paralysis, which was so ably treated of by

---

* "De la Compress. lente de la moelle ép., Leçons sur la mal. du syst. nerv., II Sér., ii fascic. 1873.

† "Compress. lente de la moelle, Dictionn-ency., des sc. Midie." II Sér., vol. viii, p. 664, 1874.

‡ "Etude sur la Paraplégie dans le mal de Pott."

§ "Klinik der Rückenmarks Krankheiten," II B, i Abth., § 149. ‖ Michaud. *Loc. cit.*

Digitized by Google

Michaud and Charcot, supplemented by the studies of Corjon and Echéverria,* became as thoroughly implanted and accepted as had the explanation of Ollivier and Louis a quarter of a century before.

Let us now proceed to a closer study of the subject in hand, and to that end we ask: *Can a mechanical lesion satisfy the demands of pathology and symptomatology?* Let us see.

A mechanical cause is present. We have seen how as the bodies of the vertebræ become destroyed by the carious processes going on in the bone, leading to the formation of an abscess cavity, the walls of which are largely composed of the thickened surrounding soft tissues, the anterior ligament and dura mater undergo changes characterized by the name of *pacchymeningitis externa caseosa.* This is not a simple abscess cavity, but in it are developed fungoid granulations and the abscess products tend to accumulate. Nature tends to fortify the walls as far as possible. It is not common for the pus to perforate the walls and escape into the surrounding tissue. The site of least resistance is toward the spinal cord, which swings insulated in the spinal canal. The membrane bulges into the spinal canal and the cord recedes. The canal offers escape for the cord up to a certain limit, when, if the bulging still goes on, the cord substance is destined to suffer. Here, then, is practically a fluid sac, its pressure force commensurate with its fluid tension—mechanically an elastic ball.

Clinically, the appearance of a psoas or gluteal abscess in a case of paralysis is oftentimes followed by the disappearance of paralytic symptoms. No one who has seen many of these cases has failed to note this clinical symptom. It is unnecessary to dwell on the mechanism of this relief, further than the allusion to it.

What does the gross lesion tend to show? At the compressed site the spinal cord is often reduced in size to such an extent as to appear simply like a band of connective tissue. It is a striking fact, that the diseased dura mater is limited to the site of the diseased vertebræ—the change from "pacchymeningite externe" to normal dura mater is abrupt. This has been noted by numerous observers. Cornil and Ranvier† say, "inflammation of the dura mater is limited exactly to the parts of the vertebræ diseased." This change in the dura mater is invariably in the anterior part of the membrane, and only in grave cases of caries does it encircle more than one-half of the cord. Does not this limitation of the diseased dura to the exact site of vertebral disease argue the non-tendency of the inflammatory process to extend in the membrane beyond what is necessary to protect the cord from the abscess cavity itself, beyond nature's necessary protective area.

The *medullary surface of the dura mater* is usually intact and perfectly normal in appearance, the inflammatory process seeming to have spent its force in the external layers. Frequent microscopical examination of the diseased membrane rarely shows even infiltration of the internal layers. Can we not draw a further inference from this, of the non-tendency of the process to extend.

Here, then, is a slow form of inflammation, which does not seem active enough to extend beyond the limits of the diseased vertebræ, not active enough to involve, as a rule, the internal layers of the membrane in which it originates. Is it probable, then, *a priori*, that it will extend to the cord? The irritation force, then, is simply that of the pressure of a bland mechanical body.

Is there anything in the pathological findings to militate against the lesion being a mechanical one? Examination of compressed cords at the site of compression, in the recent state, show:—

1. Granule cells, many or few, according to the mass of the destroyed nerve fibres.
2. Débris of broken-down nerve tissue.

---

* Gonzalés Echéverria. Sur la nature des affections dites tuberculeuses des vertébres. Thèse Paris, 1860.

† Ed. 1882, Vol. I, p. 606.

Digitized by Google

3. Swollen axis cylinders, or absence of same.

If examined at a later stage—

1. Nothing indicative of changes in the blood vessels.
2. No increase of cells about the vessels.
3. Occasionally traumatic hemorrhage.
4. Nerve fibres in course of destruction and others which have been destroyed.
5. Swollen axis cylinders or empty spaces, the former site of nerve fibres.

Then follows, as in all other analogous conditions, a secondary increase of the neuroglia.

Experimental Physiology.—It is a settled fact that pressure upon nerve trunks interferes with nerve conductivity. It has been demonstrated over and over again that a very moderate constrictive force about a nerve separates the myeline within the sheath, and if severe enough leads to secondary degeneration accompanied by all its pathological results, rendering the nerve incapable of transmitting nerve impulses, *e.g.*, the injury to the musculo-spinal nerve from pressure. Has it been demonstrated that the pressure in these cases invariably leads to a neuritis in the sense of an inflammatory lesion?

Vulpian has shown experimentally that paralysis follows the introduction of a wooden match so as to press upon the spinal cord of a guinea pig; the pressure kept up for a certain length of time, and the paralysis disappearing shortly after the pressure was removed.

Attention is directed to what occurs after a nerve has been tied:—

1. Segmentation of the myeline and disintegration of same.
2. Disintegration of the axis cylinders.
3. Complete absorption of the mass, leaving the nerve sheath empty, or containing only débris and nuclei. The nuclei of the perineurium and endoneurium aid in the transformation of the nerve into a band of connective tissue.

Here, then, is a process the result of which is practically identical, up to a certain limit, with that following compression of the cord—changes degenerative in character, the result of the mechanical interference, followed by increase of the connective tissue elements. Pathological views characterizing degenerative changes as inflammatory are by no means established.

In the endeavor to settle this point, Dr. O. Kahler* made certain experiments upon dogs. He injected beeswax about the spinal cord. Motor and sensory paralytic symptoms followed in the lower extremities. At varying periods the cords were examined. After the compression had existed from six to thirteen hours, he found slight nodular foci located in the posterior and lateral columns. Microscopical examination of same revealed marked swelling of the axis cylinders, disappearance of the myeline sheath. *No interstitial changes were found.*

Two to ten days. Microscopical examination showed greater swelling of the axis cylinders and destruction of same; granular cells; slight increase of Deiter's cells.

Later, five weeks to six months. Examination showed nodules of sclerosis with marked thickening of the connective tissue. He claims to have found none of the characteristics considered by Leyden, Joffroy, Strickler, Schultze and Strümpell as pathognomonic of myelitis, and concluded that the results were purely those of a mechanical lesion.

Kahler states, as in peripheral nerves, so in the spinal cord, a moderate pressure is enough to excite a break in conduction; a few fibres are destroyed in such cords, and we find lacunar changes, distributed in the form of nodules.

Do the connective tissue growth changes which are usually found at the site of the compression lesion necessarily signify the inflammatory character of the lesion? It is not

* *Zeitschrift für Heilkunde*, 1882, vol. III, p. 187.

Digitized by Google

denied that we usually find a sclerotic condition of the cord at the compressed site when paralysis has existed for any length of time. A glance at the tabulated result of post-mortem findings will make this conclusive. That such is the outcome of the process is beyond dispute. We contend, however, that the presence of sclerotic tissue in the cord is no proof that the initial lesion was necessarily inflammatory.

We have seen, moreover, that this follows various processes.

Ziegler says the increase of connective tissue is a result of every process of softening; that connective tissue increase characterizes the later stage of every process of degeneration; that throughout the nerve centres destruction of nerve tissue by processes, degenerative or otherwise, is *invariably followed by increase of the neuroglia.*

The examination in the great majority of these cases is made long after the onset of the lesion. We then find only the result of the process which has produced destruction. Leyden,* who came to look upon the lesion as identical with transverse myelitis, drew his conclusions almost exclusively from old findings, and these as shown are identical with those following myelitis. Charcot's case, where the disease was cured after one year's duration, and where two years subsequently death occurred, the end at the compressed site being found smaller and presenting a sclerotic condition, does not seem to offer any great support to the theory that the original lesion was inflammatory. Michaud himself offers but one case as the strong pillar of his argument, where the examination was made at an early stage, and this case had not as yet given evidence of paralysis.

Erb † says the fact that in not a few cases of compression of the cord the microscope reveals in the softened mass no granule cells, no hyperplasia of the connective tissue, no proliferation of the nuclei, but only swollen and disintegrated nerve elements, speaks unmistakably in favor of the view that the process is sometimes one of simple softening.

Strümpell ‡ contends that in repeated examinations of cords compressed from spinal caries he has *never found signs of inflammatory change.* He refuses it upon pathological grounds, and believes that there is nothing found histologically that cannot be accounted for as a result of a mechanical lesion. He says: "We must maintain against the theory generally received at present, on the ground of many of our own investigations. We have not the slightest ground for referring the occurrence of paralysis in spondylitis to a secondary myelitis. Such a 'compression myelitis'—that is an inflammation of the spinal cord arising from the pressure as such—is to be rejected, from general pathological reasons, and the microscopical examination of the cord also shows nothing which points to an inflammation, or what may not be entirely the result of mechanical compression. If we make examinations of hardened sections we see under the microscope no signs of vascular changes, of hyperæmia, of accumulation of cells about the vessels, and only exceptionally a little traumatic hemorrhage; but we do find, in addition to many still preserved nerve fibres, other fibres, which are involved in the disintegration or are already destroyed. If the destruction of nerve tissue has advanced to a certain degree, then, as in the later stages of all similar processes, there is a secondary involvement of the connective tissue. Now follows an increase of this, and we find in place of nerve fibres, firm fibrous tissue."

What inference can we draw from reported autopsies? In examining the literature of the subject, I have found but twenty-five fairly well recorded cases of pressure paralysis due to spinal caries, among which are cases reported by Michaud, Mathieu, Kahler, Rollet, Ogle, Gibney and others. From a careful perusal of these cases, I find that in nearly all mechanical pressure upon the cord was present in the form of bone, abscess or exuda-

* "Klinik der Rückenmarks-Krankheiten," vol. II, part i, p. 149.

† Ziemssen, vol. XIII, p. 469.

‡ "Lehrbuch der Sp. Path. und Therapie der inneren Krank.," vol. II, part i, p. 167.

Digitized by Google

tion into the peri-dural space. In all but three cases the condition known as pacchymeningitis externa caseosa was marked.

In eleven cases the spinal cord was converted into connective tissue at the site of the lesion. In four cases there was well marked evidence of inflammation, shown in direct involvement of the pia mater. In three of these four cases perforation of the dura mater had occurred. Simple softening of the cord is recorded in four cases, one of which, reported by Kahler, showed marked anæmia, together with softening. Michaud, in his memoir, reports but five cases where autopsies were made, three of which showed the sclerotic condition. Of the two remaining cases, his report of the autopsy of one refers to the lesion in no more definite terms than beginning myelitis—no attempt being made to enter into detail. The one remaining case showed beginning interstitial changes. In this case, prior to death, no paralytic symptoms had manifested themselves.

By way of summary, then, we have shown—

1. We have present a simple mechanical pressure, in the form of an abscess cavity, thickened dura or bone.

2. The inflammatory process is invariably a limited one, the inflammation of the dura mater being limited exactly to the site of the diseased vertebræ, showing no tendency to extend in the membrane—nature's protection.

3. The ventral or medullary surface of the dura at the site of the lesion is almost invariably normal.

4. The pressure lesion is simply a mechanical one, manifesting no tendency to involve the spinal cord through any inherent specific characters of the carious process—a simple bland mechanical lesion, and the damage it does commensurate with the pressure exerted.

5. Pathological examinations have, in the vast majority of cases, necessarily been limited to old cases where the lesion had existed for a very considerable period, and such examinations usually reveal the cord largely converted into connective tissue at the site of the lesion.

6. The presence of sclerotic tissue at the site of compression is no evidence that the original lesion was of an inflammatory nature, since it is a well-established pathological law that throughout the nerve centres destruction of nerve tissue by processes, degenerative or otherwise, is *invariably followed by increase of connective tissue.*

7. Experimental physiology gives no evidence of an inflammatory lesion following experimental compression of the spinal cord—microscopical examination of same showing a few granular cells, swollen axis cylinders, and evidence of destruction of same, together with foci of traumatic hemorrhage—changes identical with those observed by Kahler, Strümpell and others, at the site of recent compression of the human spinal cord. We do not find signs of vascular changes; of accumulation of cells about the vessels and in the connective tissue; in other words, no evidence of those pathological changes which are considered pathognomonic of inflammatory change.

8. Finally, a careful revision of the pathological findings of carefully reported cases reveals to us, in the light of recent pathological knowledge, but a very few cases (4) where we have reason to believe the lesion to have been inflammatory. The suppurative carious process in these cases had been a very virulent one, and the pia mater had become involved. In three of the cases the dura mater had been perforated.

Mechanical interference with the circulation of the spinal cord as a direct cause of certain forms of paralysis complicating spinal caries. Whatever claim there is to consider myelitis as the active lesion in the cases alluded to, there certainly is a large class which it is folly to classify as due to myelitis or to the more profound mechanical lesion already described. Those accustomed to see many cases of paralysis of Pott's disease will readily recall patients who recover the use of their paralyzed members with a rapidity out of all keeping with any pathological lesion which has produced inflammatory changes,

Digitized by Google

even though the latter have been of short duration; out of all keeping, too, with a destructive mechanical lesion, be it bone or abscess, or thickened dura. How frequently do we see immediately upon the institution of treatment in the form of thorough and efficient support, a paralysis begin to disappear. Frequently within twenty-four hours there is an increased muscular control which rapidly marches on to complete restoration of normal power. Tuesday, August 30th, I applied a support to a little patient who had been paralyzed in the lower extremities for ten days. There was complete motor paralysis; sensation but slightly affected. August 31st, the day following the application of the support, he was able to stand. Inflammatory lesions, destructive mechanical lesions, stand unable to satisfy the mind with any rational explanation.

Nearly all writers upon the subject have recognized this large class, and also recognized the failure of the commonly accepted lesions to explain it. Some have asserted that they were due to circulatory disturbances, but dismissed the subject with but an allusion to it. That the paralytic symptoms in these cases are of the nature of reflex paralysis is by no means probable—this brought about by vasomotor ischæmia, Brown-Sequard's theory of reflex paralysis, is by no means probable. Many authorities are skeptical as to the existence even of reflex paralysis, and in the words of a well-known writer, here "a vigorous scientific imagination has contributed much more than observation has supplied." To offer the theory of "reflex paralysis" as an explanation of the class of cases alluded to, in a lesion so grave as spinal caries, is, I think, as useless as it is unscientific.

In direct interference with the circulation we believe, lies the solution of the problem.

Experimental physiology teaches us that interference with the blood supply of the cord causes paralysis. When the aorta is compressed motor and sensory paralysis immediately occur. Gull, Romberg, Leyden, Tutscheck and others have reported such cases. When the aorta is gradually compressed, the paralysis advances correspondingly slow. We anticipate the question, is not the paralysis in these cases due to shutting off the blood supply from the peripheral muscles and nerves? That the paralysis is largely due to the shutting off the blood supply to the spinal cord, has been shown by Kussmaul and Tenner, and later by Schiffer. Ehrlich and Brieger* found that after temporary ligation of the aorta, by which the cord was deprived of blood for half an hour, the gray matter and anterior roots were markedly affected and decided paralytic symptoms were present.

Erb† says it is plain that mechanical pressure upon the cord may produce ischæmia in a corresponding portion. How do disturbances of the circulation occur? Here it is important to direct attention to certain well-established anatomical points. The spinal cord is completely insulated by means of the surrounding spinal fluid. It hangs suspended in a canal of vastly greater diameter than the cord itself. It receives its blood supply from the vertebral arteries at the base of the brain, viz., the anterior and posterior spinal arteries, both of which run down the cord in rather a tortuous course. The anterior spinal artery is the larger. This artery is unusually long, and it is a well-established law that the blood pressure is diminished in proportion to the length of the vessel. I take the liberty of quoting here from Moxon,‡ whose observations have an important bearing on the subject. He says: "The supply of this important part (referring more especially to the lower part of the cord) becomes one of nature's difficulties. How does she meet it? The difficulty is overcome by little reinforcing arteries which are sent in

* *Zeitsch f. Klin. Med.*, 1884, vol. VII, supplement, p. 155.

† Ziemssen, Vol. XIII, p. 284.

‡ "The Croonian Lectures on the Influence of the Circulation on the Nervous System." *Lancet*, April 2d, 1881.

Digitized by Google

from the inferior thyroid, intercostals, lumbar, ilio-lumbar and lateral sacral arteries, through the intervertebral foramina. These little arteries course along the spinal nerve roots and reinforce the blood supply which comes down from the brain. The nerve roots of the upper part of the cord pass out at once through their respective foramina. In the middle and lower portions of the cord, the nerves run some distance along the surface before making their exit. The length of the little reinforcing vessels varies with the length of the corresponding nerves."

We know, also, that the central part of the cord receives its blood supply from arteries passing from the main trunk through the anterior median fissure. What is true, also, of incompleteness of anastomosis in the brain is, also, in great measure, true of that in the cord.

Further, it is a well-established fact that the lesion of the cord in pressure paralysis is usually superficial, and the arteries which supply the cord are necessarily placed at a great mechanical disadvantage by being placed on the surface of the cord and nerves.

With these anatomical points in view, it is not difficult to see how pressure would interfere with the circulation.

The intervertebral foramina are, as we know, usually at the site of the lesion, *filled* with *inflammatory* or *exudative material*. This is common even at an early stage of the carious disease. We have thus the nerves, and consequently the arteries, subjected to this pressure.

Again, as the arteries pass with the nerves through the dura mater, they are again subjected to the pressure of the inflammatory thickening of this membrane.

Further, microscopical examination of the minute arteries show their walls thickened by the inflammatory process. Add to this the course of the vessel running on the surface of the cord, and the slight pressure from a thickened dura, or from the pressure of an abscess in a canal already narrowed by deformity, and an imperfect circulation is but a natural conclusion.

Erb says if interference with the circulation of the cord reaches a certain degree of intensity and duration, ischæmic necrosis and softening are induced. On the other hand, if the outflow of blood is hindered by the compression, we have hemorrhage from venous engorgement. The white matter is the first to suffer—the gray matter persists, its blood supply being derived, not from the periphery, but from the vessels of the longitudinal fissures.

We have thus demonstrated, in support of a circulatory cause being a common one, producing pressure paralysis, the following :—

1. Loss of blood supply to the spinal cord produces motor and sensory paralysis. This is the result of experimental physiology to that end.

2. Increasing interference with blood supply to the cord produces gradually increasing paralysis.

3. The blood supply of the cord depends largely upon the reinforcing branches which pass through the intervertebral arteries.

4. These arteries course over the surface of the cord.

5. The symptoms of this form of paralysis arise from affection of the anterior rather than the posterior portion of the cord, of the superficial rather than the deep, and the pathological findings sustain the clinical picture.

6. We have further shown that the result of interference with the blood supply may be in all degrees from partial impairment of function to complete ischæmic softening.

7. This form of paralysis complicates a lesion when there are inflammatory products pressing upon the nerves and arteries coursing through the intervertebral canals.

8. The clinical picture of the vast majority of these cases is satisfactorily explained by the variance of the vascular mechanism.

Is it, I ask, too much to suppose that the lack of nutrition consequent upon the interference with the blood supply may be sufficient to interfere with nerve tissue per-

Digitized by Google

forming its function in originating and transmitting impulses without necessarily being sufficient to lead to its complete disorganization?

Finally, our researches have led us to the following conclusions :—

1st. The lesion producing pressure paralysis is, in certain cases, of an inflammatory character, a true myelitis, manifest in either the parenchymatous or interstitial form. Clinically, there are cases which progress rather rapidly and manifest the active symptoms of myelitis. The dura mater is frequently perforated, and in the intensity of the process the pia mater is apt to become involved.

2d. The vast majority of cases, however, we believe are due to a mechanical lesion, of which the circulatory cause is but a variety, and that this in the future is to be made the basis of pathology, and will merit the place held by the inflammatory theory of to-day. This, I think, we have demonstrated from a pathological standpoint, supported it by experimental physiology and corroborated it by clinical manifestations.

The following case, which came under our observation, is interesting, as showing well one of the varieties of this lesion: Male, æt. nine years; family history good; father and mother still living and well. No brothers or sisters; no hereditary phthisical or syphilitic evidence attainable. In November, 1881, at the age of three years, he fell, striking upon his back. Soon afterward he began to complain of pain in the back, localized over the lower lumbar region, worse after walking and exercise. The disability increased rapidly and he was removed to the Hospital for Ruptured and Cripples, December 10th, 1881.

Upon admission examination showed slight fullness of lumbar region, rather large and somewhat tympanitic abdomen, inability to bend the spinal column, "stooping stiffly" to pick up objects from the floor. No paralysis of motion or sensation; great pain upon motion, localized in the lumbar region. The diagnosis of lumbar caries was made, and a spinal support applied, which gave the patient great relief. Patient remained in the hospital one year, during which time a psoas abscess (left) formed, which was aspirated. At the end of one year patient was discharged improved. He was readmitted June 17th, 1884, still showing no paralytic symptoms. A slight angular deformity was now distinctly noticeable in the lumbar region of the spinal column. Early in January, 1885, patient complained of pain about the region of the left knee joint, which continued while he remained in the hospital. About this time paresis of the lower extremities began to develop, and increased rather rapidly, and soon it became impossible for him to stand. At first constant trembling of the legs was present, which after a while subsided. He was discharged May 20th, 1886, with almost complete paraplegia.

Soon after this he was admitted into Randall's Island Hospital. It is impossible to obtain here a history more complete than that the lower extremities were completely paralyzed, with marked atrophy of same until death, which occurred November 25th, 1886; death apparently due to exhaustion (sepsis).

Autopsy made twelve hours after death. Body markedly emaciated, most marked in the lower extremities; œdema of feet and legs. A sinus was found over the left gluteal region, 1½ inches from the anus, also another over the left sacro-iliac synchondrosis. Both communicated with a common abscess cavity about the size of a hen's egg, which was found in the left sacro-iliac synchondrosis. Heart and lungs normal. Liver, spleen and kidneys showed slight amyloid degeneration, most marked in the liver. Stomach and intestines normal. Brain not examined.

SPECIMEN.

Examination of the spinal column showed caries of the last dorsal and first lumbar vertebræ, with fungoid implication of the intervening intervertebral substance. The bodies of the first and second lumbar vertebræ are partially destroyed and broken down into several pieces. The bodies of the twelfth dorsal and third lumbar vertebræ are in a condition of beginning caries. The dura mater, at a point opposite the diseased ver-

Digitized by Google

tebræ, is thickened and covered externally with a layer of carious pus. The thickening is limited to the site of the carious disease of the vertebræ, and appears to encircle only the central half of the spinal cord and cauda equina. The internal or medullary surface of the dura is smooth and apparently normal. The anterior vertebral ligament is destroyed at a point opposite the second lumbar vertebra. The opening formed by the destruction of this anterior ligament leads to a pus cavity, the walls of which are partially formed by the diseased bones. In the recent state this cavity contained about three drachms of carious pus.

Microscopical Examination.—Examination of the vertebræ gives well-marked evidence of caries. The dura mater at the site of the lesion is markedly thickened and in the condition of pacchymeningitis externa caseosa. The external layers of the diseased portion are infiltrated with numerous granulation cells, and there are also nodules, apparently of a tubercular character, but careful examination fails to detect either bacilli or giant cells.

Spinal Cord.—The cauda equina is partially surrounded by the thickened and diseased membrane. Cross section of same shows destruction of some of the nerve fibres. Many of the nerve fibres of the cauda equina, upon piercing the diseased dura mater, pass directly into the caseous thickening on its external surface and are lost in same. Examination of sections of nerves just at point of passage through the thickened dura shows the thickening of the endo- and perineurium and destruction of nerve fibres, many of the latter being totally destroyed and replaced by connective tissue. Sections of the spinal cord give evidence of ascending degeneration of the posterior columns. In the lumbar region the degeneration involves the postero-median and postero-lateral columns. In the upper dorsal region the columns of Goll only are degenerated. Changes in the first cerebellar column are but slightly marked.

Unfortunately the upper part of the cord was not obtained, and thus we were deprived of tracing the changes above the upper dorsal region.

This case is instructive in showing a compression lesion situated so low. I have failed, in my researches, to find a similar case reported where the lesion was due to carious involvement. A number of cases where the compression of the cauda equina occurred from tumors. Among those who have reported such cases may be mentioned Simon,* Fisher,† and Graves.‡

A PRELIMINARY REPORT OF EXPERIMENTAL RESEARCHES CONCERNING THE INFECTIOUS NATURE OF TRAUMATIC TETANUS.

RAPPORT PRÉLIMINAIRE DES RECHERCHES EXPÉRIMENTALES SUR LA NATURE INFECTIEUSE DU TÉTANOS TRAUMATIQUE.

EIN VORLÄUFIGER BERICHT ÜBER EXPERIMENTALE UNTERSUCHUNGEN DER INFECTIÖSEN NATUR DES TRAUMATISCHEN TETANUS.

BY EDWARD ORAM SHAKESPEARE, A.M., M.D.,

Of Philadelphia, U. S. A.; Pathologist to the Philadelphia Hospital, etc.

On the five pages immediately following the details of experiments are tabulated, for more convenient reference.

---

* *Archiv f. Psych. u. Nervenkr.*, v, p. 114, 1874.

† *Transactions* Prov. Med. and Surg. Assn., 1882, x, p. 203.

‡ *Transactions* Path. Soc., Lond., 1875, '76, xxvii, p. 19.

Digitized by Google

AUGUST 1, 1887, A HORSE DIED DURING THE EARLY MORNING OR NIGHT, AT THE VETERINARY DEPARTMENT OF THE UNIVERSITY OF PENNSYLVANIA, OF WELL MARKED TRAUMATIC TETANUS. ITS BRAIN, MEDULLA AND SPINAL CORD WERE REMOVED BETWEEN THREE AND SIX HOURS POST-MORTEM, AND IMMEDIATELY PLACED UPON ICE.

1887. Aug. 1.	A white rabbit was trephined and the brain accidentally wounded to depth of a ½ inch; this followed by transient paralysis. Not inoculated.	A culture started, in neutral glycerine agar, as prepared by Roux for tubercle cultures, from the medulla.	At 5 P. M. a mouse-colored rabbit inoculated, sub dura, with emulsion of a portion of medulla.	At 5.30 P. M. a white rabbit inoculated, sub-dura, with a similar emulsion.		At 5.30 P. M. medulla put back on ice.		At 6 P. M. inoculated, under skin above the tail, a black and white rabbit. (See previous column, 9th:)	At 6 P. M. inoculated, under skin above the tail, a black rabbit.
" 2.			In early morning had trismus and tetanus, which became pronounced in a few hours; in afternoon extremely marked intermittent opisthotonos.	In afternoon had trismus and convulsive movements of legs when disturbed.		At 4 P. M. inoculated, sub-dura, a white rabbit.	At 4.15 P. M. a spotted white and gray rabbit inoculated, sub-dura.		
" 3.			In afternoon still living in tetanic convulsions and trismus, but former less intense.	In afternoon dead in opisthotonos; jaws set; portion of cord put to dry in jar.* ‖ At 4 P. M. inoculated, sub-dura, a white rabbit	‖ At 4.15 P. M. inoculated, sub-dura, half white and gray rabbit.	In afternoon sick; slight trismus; great difficulty in eating; slight tetanic convulsions when moved.	In afternoon down on side; head back; trismus; died next day.		
" 4.			Still living in tetanic spasms and trismus.	At 4 P. M. slight trismus and convulsions when moved. Died in the night in opisthotonos.	At 2 P. M. trismus and marked tetanic spasms; at 5.30 P. M. found dead in marked opisthotonos and stiff. At 6 P. M. inoculated, sub-dura, a mouse-colored rabbit.	In morning worse; at 3 P. M. found dead in extreme opisthotonos; portion of cord put to dry in jar.* At 6 P. M. inoculated yellow and white rabbit.			
" 5.			Died in morning, in marked tetanic spasms; put on ice.		Dead in opisthotonos.	Died in tetanic spasms during morning; kept on ice.			
" 7.			At 12 M. inoculated a mouse-colored rabbit; at 6 P. M. trismus and tetanic spasms.			At 12.15 P. M. inoculated a black and white rabbit; at 6 P. M. trismus and tetanic spasms.			
" 8.			Found dead in early morning in marked opisthotonos; a portion of cord put to dry in jar*; animal placed on ice.			Found dead in early morning; portion of cord removed and put in drying jar;* animal placed on ice.			
" 9.	a		In afternoon inoculated a mouse-colored rabbit.	b	c	In afternoon inoculated a black and white rabbit (he had been inoculated under skin on the 1st, from horse medulla, and had remained perfectly well).		d	

Digitized by Google

	a				b	c		d	
1887. Aug. 10.				Died in morning, of trismus and opisthotonos; put on ice.			Died in morning, of trismus and tetanic spasms; portion of cord removed and put to dry in jar;* animal put on ice.		
" 11.				In afternoon inoculated a mouse-colored and white rabbit.			In afternoon inoculated a black and white rabbit.		Died greatly emaciated, without having shown signs of tetanus; put on ice.
" 12.				Trismus and tetanic spasms.			Marked trismus and opisthotonos.		
" 13.				Found dead in opisthotonos; put on ice.			Marked trismus and opisthotonos.		
" 14.				In afternoon inoculated a gray rabbit.			Marked trismus and opisthotonos		
" 15.				Early morning, squealing; spasms; at 2 P.M. dead, and portion of cord put to dry.* At 3 P.M. inoculated a gray rabbit with right white shoulder.			Marked trismus and opisthotonos.		Inoculated, subdura, a white rabbit. This rabbit still living, Sept. 4th, and always well.
" 16.				Died without fits; he ate all the time and died suddenly. Inoculated a gray rabbit with white shoulders.			Dead; cord put to dry.* Inoculated a brown and white rabbit.		
" 17.				Early morning found dead in extreme opisthotonos; animal put on ice.			Died at 2 P.M. with marked trismus and opisthotonos; at noon had pleurosthotonos.		
" 18.		Inoculated in afternoon a yellow rabbit with white collar.	In afternoon inoculated a black rabbit with white collar.	Cord put in drying jar;* animal kept on ice.	Inoculated a mouse-colored rabbit with a white collar.		Inoculated a gray rabbit.	Inoculated gray rabbit, white shoulder	In afternoon inoculated a black and white rabbit.
" 19.		Early morning not eating; jaws set; slight spasms when disturbed; between 7 and 12 A.M. died in opisthotonos.	Early morning eating; seems well; at noon a little tendency to spasms; at 2 P.M. moribund; head back; no trismus; breathing heavily; soon died; jaws not set at any time.	At 3.30 P.M. inoculated a mouse-colored rabbit.	Early morning not eating; jaws set; head back; spasms when moved; at 2 P.M. better; at 6 P.M. worse.		Early morning would not eat; jaws set; slight spasms when moved; between 7 and 12 A.M. died in opisthotonos.	Not eating; no tendency to spasms, but jaws slightly set at 3 P.M.; at 5 P.M. died in marked opisthotonos.	Early morning not eating; jaws set; spasms when moved; at 2 P.M. better.

Digitized by Google

1887. Aug. 20.		Culture renewed.	At 5 A.M. found dead and stiff in marked opisthotonos. At 3 P.M. inoculated into muscles of the back a large yellowish-gray rabbit. 4th Sept. This rabbit still perfectly well.	At 2 P.M. better; jaws less set, but spasmodic movements of them; will not eat; dead between 3 and 6 P.M., in marked opisthotonos and stiff.						Jaws not set at noon; at 2 P.M. nearly moribund and jaws set; at 6 P.M. died, limp.
" 21.				At 4 P.M. inoculated, subdura, spotted yellow and white rabbit.	At 6 P.M. inoculated, under skin of back, a white rabbit.	At 6 P.M. inoculated, under skin of back, a white rabbit.	At 6.30 P.M. inoculated, under skin of back, a white rabbit (box 8).	At 6.30 P.M. inoculated, under skin of back, a yellow rabbit (box 2).		At 4 P.M. inoculated a yellow rabbit; at 10 P.M. it seemed well.
" 22.				At 5 P.M. no trismus or spasms.		Still living and well, Sept. 4th.				At 7 A.M. very ill; jaws set and intense spasms; died between 7 and 12 A.M. At 5 P.M. inoculated, with great difficulty, an Alderney Cow, which had tuberculosis pulmonum; brain wounded to depth of one inch.
" 23.				Head back; unsteady on legs, but no trismus.					Kept emulsion of medulla over night and inoculated, subdura, at 4 P.M., a young white rabbit.	In morning no symptoms; at 3 P.M. difficulty of breathing, which was not noticed at noon; at 3.30 P.M. distinct trembling of muscles of hind legs; at 5 P.M. down; could not cause movement of jaws or make her open the mouth; at 6 P.M. no trembling of legs; jaws movable; breathing easier.
			h		e		f	g		

Digitized by Google

			h		e		f	g		
1887. Aug. 24.	Died greatly emaciated, having shown no signs of tetanus.								Died with trismus and tetanic spasms.	Found dead in morning; legs drawn up; head straight out; autopsy showed wound in dura and corresponding cerebral hemisphere; intense engorgement of that side of brain; no marked congestion of lungs or trachea
" 25.	At 5 P.M. inoculated, sub-dura, a small white rabbit. This rabbit quite well up to Sept. 4th.	A MULE died of Traumatic Tetanus, and the brain, medulla and cord were placed on ice. At 5 P.M. inoculated, sub-dura, a white rabbit			Head back; tendency to spasms.					At 5 P.M. a young white rabbit inoculated, sub-dura.
" 26.		At 1 P.M. sick; tendency to spasms; head back; jaws set; at 5 P.M. and later in evening severe tetanic convulsions; head back; jaws firmly set; fore legs stretched out.			Above condition more marked.					Found dead in morning, but no signs of tetanus.
" 27.		Early morning found dead; put on ice. At 5 P.M. inoculated a gray rabbit with white collar.			Marked tetanic convulsions, but jaws seldom set.					Inoculated a yellowish brown rabbit with white collar.
" 28.		Early morning found dead, in marked opisthotonos; put on ice.			Early morning found dead in marked opisthotonos.					This rabbit still living and well Sept. 4th.
" 29.		At 3 P.M. inoculated a yellow rabbit with white shoulders; at 10 P.M. apparently well.								
" 30.		Early morning found dead in marked opisthotonos.	h		e		f	g		

Digitized by Google

			h	e		f	g		
1887. Aug. 31.	At 6.30 P.M. inoculated, sub dura, a brown and white rabbit.	At 6 P.M. inoculated, sub-dura, a white rabbit; at 10 P.M. not eating.	At 6.15 P.M. inoculated, sub-dura, a yellow rabbit; at 10 P.M. not eating	Virus from cord of 8th, in column 3, kept in drying jar.		Virus from cord of 3d, column 4, kept in drying jar.		Virus from cord of 4th, column 5, kept in drying jar.	
Sept. 1.	Quite well.	In morning and at 3 P.M. same condition, but no trismus or spasms, at 5.30 and at 7.30 P.M. trismus.	In morning, at noon and at 3 P.M. same condition, but no trismus or spasms; at 5.30 P.M. trismus.	Inoculated, sub-dura, a mouse-colored rabbit.	Inoculated, sub-dura, the white rabbit in box 6, which had been inoculated under skin on Aug. 21st; at 10 P.M. seemed well.	Inoculated, sub-dura, a gray rabbit.	Inoculated, sub-dura, the white rabbit in box 8, which had been inoculated under skin Aug. 21st.	Inoculated, sub-dura, the yellow rabbit in box 2, which had been inoculated under skin Aug. 21st.	Inoculated, sub-dura, a gray rabbit.
" 2.	Quite well.	Not eating; jaws set; at 9.30 P.M. eating a little; jaws only stiff.	Eating in morning; at 2 P.M. jaws not set; at 4 P.M. eating.	At 4.30 moping a little, otherwise well and eats.	At 7 A.M. found dead, but no signs of tetanus.	Well and eating.	Well and eating.	Well and eating.	Well and eating.
" 3.	Quite well.	In morning not eating; jaws stiff; at noon jaws set; at 3 P.M. same; at 5 P.M. jaws movable, but not eating.	In morning not eating; noon same; jaws set; at 3 P.M. same; at 5 P.M. jaws slightly movable.	Quite well.		Well and eating.	Well and eating.	Well and eating.	Well and eating.
" 4.	Quite well.	Same condition in morning and at 2 P.M.	Eating a little, but jaws stiff.	Quite well.		Eats but little; jaws a little stiff.	Eating but little; jaws a little stiff.	Well and eating.	Well and eating.

Explanation.—The lines = indicate end of the series; the lines ‖ indicate an inoculation; the line | indicates continuation of remarks. All inoculations, unless otherwise stated, were made between the dural and arachnoid membranes of the brain.

Digitized by Google

RESUMÉ OF RESULTS.

First Series.—Eight rabbits were inoculated sub dura from the medulla of a horse dead from traumatic tetanus.

The first rabbit showed the first symptoms of tetanus within 15 hours, and died in tetanic convulsions within 48 hours after inoculation. Both the period of incubation and that of death became markedly shortened in continuing the inoculations from rabbit to rabbit.

Second Series.—Four rabbits were inoculated sub dura from the same medulla of tetanic horse. The first rabbit showed the first symptoms of tetanus within 20 hours and died within 48 hours after inoculation. Continuing the inoculations from rabbit to rabbit, the period of incubation and of death shortened.

Third Series.—Four rabbits were inoculated sub dura from the medulla of the same horse after it had been kept on ice 24 hours. The first rabbit showed the first symptoms in 24 hours, and died in 48 hours after inoculation. Continuing the inoculations from rabbit to rabbit, the period of incubation and of death became shortened.

Fourth Series.—Three rabbits, in series, were inoculated sub dura from the medulla of a mule which had died of traumatic tetanus, with the same results as in the previous series.

Fifth Series.—Seven rabbits were inoculated under the skin from the medulla of the horse above mentioned (either into subcutaneous or intermuscular tissue of the back). One died within 18 hours and one within 10 days, but neither of them showed any signs of tetanus, either externally or internally. A rabbit inoculated sub dura from the medulla of the latter on August 15th is still (September 4th) living and quite well. The rest remained well.

Sixth Series.—A rabbit which had been inoculated under the skin eight days prior was inoculated sub dura from the last rabbit of the third series. It became ill and died promptly of tetanus in the shortened period.

A rabbit inoculated sub dura from its medulla showed signs of tetanus within 20 hours, but did not die until five days after inoculation.

Seventh Series.—Six rabbits were inoculated sub dura with emulsions of the spinal cords of tetanic rabbits which had been treated in a manner similar to that which Pasteur employs for the attenuation of the virus of hydrophobia, during periods varying between three and fifteen days. Five of these died of marked tetanus, the symptoms appearing and death occurring within periods longer than those of the corresponding rabbits from which the cords had been obtained, and usually proportional to the length of time in which the cord had been drying. One of the six showed doubtful signs of tetanus, but nevertheless very promptly died.

Eighth Series.—A rabbit was inoculated sub dura with the medulla of the rabbit which had died after inoculation with the cord which had been 15 days drying, and it showed the first signs of tetanus in 40 hours, and died in tetanic convulsions seven days after inoculation.

A rabbit was inoculated sub dura with cord which had been drying 14 days, and it died of tetanus in 20 hours. A rabbit and a cow were inoculated sub dura from its medulla, and the former died of marked tetanus, while the latter died without marked symptoms. (From the autopsy, it is probable that death was caused by injury to the brain.)

A young rabbit was inoculated sub dura, August 27th, from the cow's medulla, and died within 16 hours, but showed no signs of tetanus.

A rabbit was inoculated sub dura from its medulla, and is still living (September 4th), with no signs of illness.

Ninth Series.—Three rabbits were inoculated sub dura, September 1st, from cords which had been drying, respectively, 23, 27 and 28 days (the three oldest cords used in

Digitized by Google

the seventh series). Those inoculated with the 23- and 28-day cords showed no signs of illness up to the afternoon of September 4th. The one inoculated with the 27-day cord for the first time showed stiff jaws and ate but little on afternoon of September 4th.

Tenth Series.—Three well rabbits which had been inoculated under the skin, August 18th, were inoculated sub dura, September 1st, with the same cords as above, viz., 23-, 27- and 28-day cords. That of 23-day cord found dead next day, but had no signs of tetanus, either externally or internally. That of 28 days had stiff jaws and would not eat for the first time on afternoon of September 4th. That of 27 days showed no signs of illness up to September 4th inclusive.

Eleventh Series.—Three rabbits were inoculated sub dura, August 31st, from cultures from the horse's medulla which had been started August 1st. One rabbit remained quite well up to the afternoon of September 4th. One showed slight signs of tetanus September 2d, which continued through September 3d and 4th. One showed slight signs of tetanus September 3d, which continued through September 4th.

Twelfth Series.—A trial attempt was kindly made for me by Dr. L. Wolff, of Philadelphia, to isolate a ptomaine from the brain, medulla and cord of the above-mentioned mule and cow.

The method of Stass-Otto was more or less closely followed. The product obtained from the mule was injected into two rabbits under the skin of the back. Within twenty minutes they became very ill, slightly paralyzed, and very restless, with much difficulty of breathing, but had no trismus or decided convulsions. They entirely recovered in six hours.

The product obtained from the cow produced but little and only very transient effect.

NOTE.—The autopsies of the tetanic animals showed intense congestion of the lungs and trachea and of the kidneys. Sometimes congestion, but oftentimes none at all, of the cerebro-spinal central nervous system. Only in one (the cow) was there any indication of injury or local inflammation at the seat of inoculation.

Some of the series of experiments are still in progress, and only the results already obtained are announced. Upward of fifty inoculations have already been made, including control experiments. Two methods of inoculation have been employed. Intracranial inoculations, after the method of Pasteur in the case of rabies, and subcutaneous or intermuscular injections by means of hypodermic syringes. The inoculations were always made under thorough antiseptic precautions and with sterilized instruments. In none of the experiments was there any sign of accidental infection, such as suppuration, etc. The material used for inoculation was, in general, obtained from the medulla or the spinal cord, and cultures in neutral or slightly alkaline flesh, glycerine-agar, as recommended by Roux for the culture of tubercle bacilli. The tetanus material was taken, under aseptic precautions, from a horse and a mule, dead of traumatic tetanus, in the Veterinary Department of the University of Pennsylvania, the brain medulla and cord being removed one and three hours respectively, post-mortem, and immediately kept on ice until used. The inoculation material was, in general, prepared in the following manner: a small piece of the medulla or cord was thoroughly rubbed in sterilized distilled water; after the solid particles were allowed for a few minutes to subside to the bottom of the vessel, the opalescent emulsion thus obtained was drawn off by means of sterilized pipettes and placed in small sterilized vials until used, never having been thus kept longer than three hours before inoculation. Eight control experiments were made.

Conclusions drawn from the author's personal researches:—

1. Traumatic tetanus of the horse and mule is, at least sometimes, if not always, an infectious disease transmittible to other animals, and, therefore, possibly also to man; and during the progress of this disease a virus is elaborated and multiplied, which is

Digitized by Google

capable of producing the same infectious disease in some other animals, when placed beneath the dura mater of the cerebrum.

2. This virus is contained in the medulla and spinal marrow of the animal suffering with the disease. It is, like the virus of hydrophobia, capable of being strengthened in virulency by inoculation *sub dura cerebri* from rabbit to rabbit, and, like the virus of hydrophobia, is capable of attenuation by exposure for a sufficient time to the action of the dry air at a temperate or summer heat; and, still again, like the rabic virus, its effects are far more intense when the virus is inserted beneath the *dura mater cerebri* than when injected beneath the skin or between the muscles of the back.

3. The author reserves his conclusions concerning a prophylactic effect of inoculations of the attenuated virus until the completion of experiments which are at present in progress.

Conclusions drawn from the author's experiments when correlated with those of Nicolayer, Carle and Ratone, Rosenbach, Ferrari, Flügge, *et al*:—

Traumatic tetanus of the lower animals and of man, at least sometimes, possibly always, is a specific infectious disease, due to the action of a specific infectious virus which exists in the tissues at the seat of infection, in the blood and in the cerebro-spinal nervous system.

In view of the experimental evidence which we possess at present, and of many unassailable observations of many surgeons and veterinarians, there seems to be ample warrant for the admission that not infrequently tetanus in man is acquired directly and indirectly from some of the domestic animals which surround him, notably the horse.

DISCUSSION.

Dr. Lester Curtis said that in a recent number of the *Berliner Klinische Wochenschrift* he had noticed an article describing two fatal cases of tetanus in the human subject. The first of these was that of a man who contracted tetanus from a splinter under the nail while rolling tenpins. Portions of this splinter were thrust into the skin of guinea pigs and resulted always in tetanus.

The other was that of a boy who thrust a small stone under the skin of the foot while barefooted. Portions of the earth from the playground inserted under the skin of rabbits resulted in tetanus.

The observer found two bacterial forms in the blood of the patient and the infected animals; one of these was an ordinary micrococcus, and seemed to be that of putrefaction; the other was a small rod bacterium, not very minutely described. The latter form he thinks to be the agent in producing tetanus. But the active agent he thinks was a ptomaine which has been before described and named tetanin.

Cultures were made of this rod form, and tetanus resulted from inoculation of this culture, though the methods are not given in the paper.

Dr. Lewis P. Bush, of Wilmington, Delaware, asked the question whether the experiments of Dr. Shakespeare did not prove that tetanus was not communicated by mere contact of animal to animal—inasmuch as hypodermic injection failed to produce tetanic symptoms—but only when the substance taken from the spinal cord is njected beneath the dura mater of the brain.

To which Dr. Shakespeare replied that although his experiments seemed to admit of that conclusion, in Europe the hypodermic injection did produce tetanus, as reported.

Digitized by Google

Dr. VICTOR C. VAUGHAN, Ann Arbor, Mich., read a paper on—

TYROTOXICON: ITS NATURE, ITS CHEMISTRY, AND ITS ACTION UPON ANIMALS.

TYROTOXICON: SA NATURE, SA CHIMIE, ET SON ACTION SUR LES ANIMAUX.

TYROTOXICON: SEIN WESEN, SEINE CHEMIE UND WIRKUNG AUF THIERE.

In 1885 the writer announced the discovery in poisonous cheese of a ptomain, which produced upon man symptoms identical with those observed in persons who ate of the cheese.* Since that time, aided by my laboratory assistants, Messrs. F. G. Novy and E. V. Riker, I have continued the study of this poison, with the results to be given in this paper.

In November, 1885, a student brought me a bottle of milk which had stood tightly closed, with a glass stopper, for about six months. From this I isolated tyrotoxicon by the method which had been employed with the cheese. It was presumed that this milk was normal in composition when first obtained; but of this we could not be certain.†

I then put several gallons of normal milk in perfectly clean bottles with glass stoppers, and allowed these to stand in my work room. From time to time a bottle was opened and a test for tyrotoxicon was made. These tests were followed by negative results, until about three months after the experiment was begun. I then succeeded in getting the poison from one of the bottles. The method of testing for it was as follows: The coagulated milk was filtered through heavy Swedish filter paper. The filtrate, which was colorless and strongly acid, was rendered feebly alkaline by the addition of potassium hydrate, then agitated with ether. After separation the ethereal layer was removed with a pipette, allowed to run through a dry filter paper to remove a flocculent, white substance which floated in it, and then allowed to evaporate spontaneously. If necessary, this residue was dissolved in water and again extracted with ether. On the evaporation of the ether, the tyrotoxicon was recognized by its crystalline appearance, by placing a small bit upon the end of the tongue, and by its effects upon animals, which will be discussed later.

In June, 1886, I obtained tyrotoxicon from some ice cream which had seriously affected all who had eaten of it.‡ This cream had been flavored with vanilla, and the ill effects were at first supposed to be due to the flavoring, some of which, fortunately, remained in the bottle, and was sent to me along with the cream. As the surest means of deciding whether or not the vanilla extract was poisonous, Mr. Novy and I each took thirty drops of it. No ill results following this, Mr. Novy took two teaspoonfuls more, with no results. This settled the question of the poisonous nature of the vanilla more satisfactorily than could have been done by a chemical analysis. Moreover, it confirmed the views of Preusse, who, some years ago, from experiments with vanillin and vanilla extracts, reached the conclusion that the popular belief that poisoning from vanilla cream is due to the flavoring is erroneous.§ Tyrotoxicon has been found not only in vanilla cream, but in that flavored with chocolate and lemon, also in custard which contained no flavoring whatever, and in milk, cheese, cream puffs and oysters.

---

* *Zeitschrift für physiologische Chemie*, B. x, Heft ii.

† "Report of Michigan State Board of Health," July, 1886.

‡ "Report of Michigan State Board of Health," July, 1886.

§ *Zeitschrift für physiologische Chemie*, B. iv, § 209.

Digitized by Google

The tyrotoxicon obtained from this cream was administered to a cat, with results to be given further on.

We also ascertained that when normal milk was inoculated with some of the solid portion of the poisonous cream, the whole became poisonous after being kept in a warm room for a few hours This indicated that the poison was due to the growth of a germ.

In August, 1886, the poison was found in milk by Drs. Newton and Wallace,* chemists to the New Jersey State Board of Health, and later by Prof. Scherer,† chemist to the Iowa State Board of Health. An English Army medical officer seems to have been equally successful.‡ Dr. Stanton,§ Health Officer of Cincinnati, has obtained tyrotoxicon from some poisonous cream-puffs, while still more recently Dr. Wallace‖ has detected it in some cheese which seriously affected those who ate of it, and Prof. Ladd, of Geneva, N. Y.,¶ has obtained it from some poisonous chocolate cream.

In January, 1887, we ascertained that if some butyric acid ferment be prepared as is ordinarily done in the preparation of butyric acid, and some of this be added to normal milk, and the whole be kept in closely stoppered bottles for eight or ten days, the poison will be developed in the milk in considerable quantity. The milk should be filtered, the filtrate neutralized with sodium carbonate, and then extracted with ether.

Having a strong solution of the poison in absolute alcohol, which had been obtained from milk inoculated as stated above, I added it to some platinum chloride and began to evaporate on the water-bath. As soon as the alcohol evaporated the residue exploded with great violence. The vessel, a glass evaporating dish, was broken into fine fragments, and these were scattered over the room, while the gaslight under the water bath was extinguished. The experiment was repeated a number of times with like results. From some of this alcoholic solution the platinum was removed with hydrogen sulphide gas; but the filtrate was then found to have lost its explosive property. This reminded us that diazobenzol compounds form with platinum chloride a highly explosive compound, and that diazobenzol is also decomposed by hydrogen sulphide. Some diazobenzol nitrate was prepared, according to the method of Griess,** and comparisons made between this and tyrotoxicon.

With equal parts of sulphuric acid and carbolic acid the prepared diazobenzol nitrate gave a green coloration, while with the same reagents tyrotoxicon gave a color which varied from a yellow to an orange-red. But the diazobenzol nitrate dissolved in the whey of normal milk, and extracted with ether or in the presence of other proteids, gave the same shades of color as the tyrotoxicon did, and the potassium compound of tyrotoxicon prepared by the method to be given later produced the same shade of green as did the artificial diazobenzol. This color test may be used as a preliminary test in examining milk, or other suspected material, for tyrotoxicon. It is best carried out as follows: place on a clean porcelain surface two or three drops each of pure sulphuric acid and pure carbolic acid. This mixture should remain colorless or nearly so. Then add a few drops of the aqueous solution of the residue left after the spontaneous evaporation of the ether. If tyrotoxicon be present, a yellow to an orange-red color will be produced. This test is to be regarded as only a preliminary one; for it may be due to the pressure of a nitrate or nitrite. The tyrotoxicon must be purified according to a method to be given further on before the absence of nitrate or nitrite can be positively demonstrated.

In the filtrate from milk which is rich in tyrotoxicon, after neutralization with sodium carbonate, filtration and acidifying with hydrochloric acid, gold chloride pro-

---

* *Medical News*, Sept. 25th, 1886.

† *Iowa State Register*, Oct. 12th, 1886.

‡ *The Lancet*, June 29th, 1887.

§ *Cincinnati Lancet and Clinic*, April 23d, 1887.

‖ *Medical News*, July 16th, 1887.

¶ Unpublished.

***Annalen der Chemie und Pharmacie*, B. 137, § 39.

Digitized by Google

duces a precipitate, which is insoluble in water, but soluble in hot alcohol, from which it separates on cooling, in golden plates, which are more or less imperfect. Diazobenzol compounds give with gold chloride a precipitate having all these properties. In both cases the gold compound is decomposed by frequent treatment with hot alcohol, and this fact prevented any satisfactory ultimate analysis of this salt. It should be remarked here that from some samples of milk this gold salt is obtained much more easily than from others, and the difference is dependent not so much upon the amount of tyrotoxicon present, as upon the condition of the other organic matter present. It is best obtained from samples which have stood in well stoppered bottles for a month or longer.

Tyrotoxicon obtained from milk was treated according to the method recommended by Griess* for the preparation of diazobenzol potassium hydrate, and the per cent. of potassium in the compound obtained was determined. The filtrate from the milk which had been inoculated with the ferment and kept in a stoppered bottle in a warm room for ten days, was neutralized with sodium carbonate, agitated with an equal volume of absolute ether, allowed to stand in a stoppered flask for 24 hours, the ether removed and allowed to evaporate from an open dish. The aqueous residue was acidified with nitric acid, then treated with an equal volume of a saturated solution of potassium hydrate and the whole concentrated on the water-bath. On being heated the mixture became yellowish-brown and emitted a peculiar aromatic odor. Both the color and the odor corresponded exactly with the color and odor produced by carrying some of the artificial diazobenzol through a comparative test. On cooling, the mass crystallized, the resulting compound appearing in the test with the tyrotoxicon, and in the comparative test also, in beautiful, six-sided plates, along with the prisms of potassium nitrate. The crystalline mass obtained from the tyrotoxicon was treated with absolute alcohol, filtered, the filtrate evaporated on the water-bath, the residue dissolved in absolute alcohol, from which it was precipitated in a white crystalline form with ether. The precipitate was collected, washed with ether, dried, and the per cent. of potassium estimated as potassium sulphate.

.2045 gram of the substance yielded .109 gram of potassium sulphate.

Per cent. of potassium calculated in $C_6H_5N_2$ OK 24.42; found 23.92. This analysis establishes the identity of tyrotoxicon and diazobenzol.

Chemists will now appreciate the great difficulty that has been experienced in isolating the active agent of poisonous cheese. The readiness with which diazobenzol decomposes is well known. When warmed with water it breaks up into carbolic acid and nitrogen.

Recently, we received from Amboy, Ohio, two four-ounce bottles filled with melted cream. One kind was flavored with lemon and the other with vanilla. Both had proved poisonous, over 150 persons having been affected from eating the cream. The bottles were tightly corked, and a few hours after coming into our possession the bottle containing the vanilla cream burst, from the accumulated gas, and the contents were lost. The other bottle was then opened under water and its contained gas collected and analyzed. It yielded 88.8 per cent. of nitrogen. Some undecomposed tyrotoxicon was separated from the cream and its toxic effect was demonstrated on a kitten.

Hydrogen sulphide decomposes tyrotoxicon; therefore, all attempts to obtain the poison by precipitating it with some base, such as mercury or lead, and then removing the base with hydrogen sulphide, have failed. Moreover, diazobenzol is only a transition product of putrefaction.

I have frequently found that leaving some milk rich in the poison in an open beaker for twenty-four hours would be sufficient to destroy the whole of the poison.

---

* *Annalen der Chemie und Pharmacie*, B. 137, § 54.

Digitized by Google

We know nothing positively concerning the acid with which diazobenzol is combined in the milk or cheese. We prepared some diazobenzol butyrate, $C_6H_5N_2C_4H_7O_2$, and ascertained that the crystals of this compound correspond with those of tyrotoxicon, and that they decompose in moist air with the same rapidity. This is the first time that diazobenzol has been found as a product of putrefaction, and it is possible that many of its allied compounds may be formed in the same way.

The following experiments will show that the effects of tyrotoxicon and diazobenzol upon the lower animals are identical:—

Experiment 1.—From one-half gallon of some milk which had stood in a tightly stoppered bottle for three months, there was obtained quite a concentrated aqueous solution of the poison after the spontaneous evaporation of the ether. Ten drops of this placed in the mouth of a small dog three weeks old caused, within a few minutes, frothing at the mouth, retching, the vomiting of frothy fluid, rapid breathing, muscular spasm over the abdomen, and after some time, watery stools. The next day the dog seemed to have recovered partially, but was unable to retain any food. This condition continuing for two days, the animal was killed with chloroform. No examination of the stomach was made.

Experiment 2.—To a small kitten, nine days old we gave some tyrotoxicon obtained from lemon cream received from Amboy, Ohio. Within a few minutes there was violent retching, but no vomiting or purging. Death resulted within fifty minutes. The stomach contained some food. The mucous membrane of the stomach and intestines were not reddened, but the lungs were congested.

Experiment 3.—Tyrotoxicon obtained from poisonous ice cream was given to a cat. Within ten minutes the cat began to retch, and soon it vomited. The retching and vomiting continued for two hours, during which time the animal was under observation, and the next morning it was observed that the cat had passed several watery stools. After this, although the cat could walk about the room, it was unable to retain any food. Several times it was seen to lap a little milk, but on doing so it would immediately begin to retch and vomit. This condition continuing, after three days the animal was placed under ether, and its abdominal organs examined. We certainly expected to find marked inflammation of the stomach; but, we really did find the stomach and small intestines filled with a frothy, serous fluid, such as had formed the vomited matter, and the mucous membrane very white and soft. There was not the slightest redness anywhere along the alimentary canal.

Experiment 4.—Some tyrotoxicon obtained from milk which had been inoculated with poisonous cream and allowed to stand for 48 hours was administered to a large, old cat. It soon produced retching, but no vomiting or diarrhœa. The amount of the poison administered in this case was small.

Experiment 5.—Some tyrotoxicon from milk was given to a young but full grown cat. Within 15 minutes there was marked and evidently painful retching, and within half an hour vomiting accompanied by rapid breathing. Later there were several stools, the first two of which contained fecal matter; but the subsequent ones were rice-water-like and wholly free from fecal odor. After two days some more of the poison was given, and the vomiting and diarrhœa again induced. The animal was then anæsthetized, and examination of the stomach and intestine showed the mucous membrane blanched, as was found in Experiment 3.

We have records of a number of other experiments with tyrotoxicon on cats and dogs, but as the symptoms induced in all were substantially the same, it is unnecessary to note them here.

We will now give the effects observed in these animals after the use of the prepared diazobenzol :—

Experiment 6.—Gave to a large, old cat 100 milligrams of diazobenzol butyrate.

Digitized by Google

Immediately the animal began to purge. Then she lay upon the floor breathing rapidly and retching severely for two hours, when she died. The retching was most violent, but vomiting seemed impossible. Post-mortem examination showed the lungs greatly congested, but the mucous membrane of the stomach and intestines was not reddened. The stomach contained some food. We suppose that the congestion of the lungs was due to the violent retching and to the feeble action of the heart.

Experiment 7.—To a young but full-grown Maltese cat we gave 100 milligrams of diazobenzol butyrate. With most violent retching, but without vomiting or stool, the animal died within 30 minutes after the administration of the poison. The lungs were found acutely congested and the stomach free from any redness. The circular fibres of the small intestine were tightly contracted.

Experiment 8.—Gave to a full-grown cat 25 milligrams of diazobenzol butyrate. Within ten minutes vomiting and purging were induced. The first stools contained fecal matter, but the subsequent ones were like rice-water and wholly free from fecal odor. After two days the cat was able to take food, then ten milligrams more of the poison was given, with the reproduction of the vomiting and purging. The animal then rapidly emaciated, and after a few days it was anæsthetized and the mucous membrane of the stomach and intestine found blanched. The lungs were not congested.

Experiment 9.—Ten milligrams of the poison produced profuse diarrhœa and continued vomiting in a cat.

Experiment 10.—Seventy-five milligrams produced vomiting and diarrhœa with congestion of the lungs in a dog.

It is worthy of notice that guinea-pigs and mice are not affected by tyrotoxicon in the same manner that cats and dogs are. We administered five milligrams of the artificially prepared diazobenzol to each of four guinea-pigs. No effects were observed until the next day, when there seemed to be paralysis, first of the posterior, and then of all the extremities. This paralysis gradually extended, until within two or three days death resulted. There was neither vomiting nor purging.

Prof. Ladd, of Geneva, N. Y., writes me that he administered the poison obtained from ice cream and mixed with meal, to two mice. This produced, after about twenty-four hours, paralysis of the legs. The legs were spread out, he writes, and the animals moved as though they were swimming. There was no vomiting or purging.

It seems unnecessary to detail any more of these experiments, as the identity of tyrotoxicon with diazobenzol is now established, not only by chemical analysis, but this proof is strengthened, if chemical analysis can be strengthened, by the action of the poison on the lower animals and by post-mortem appearance.

We think it highly probable that diazobenzol, or some closely allied substance, will be found in all those foods which, from putrefactive changes, produce nausea, vomiting and diarrhœa. In some oysters which produced these symptoms I have recently found tyrotoxicon.

Milk or other fluid to be tested for this poison should be kept in well stoppered bottles, for if the fluid be exposed to the air, the tyrotoxicon may decompose in a few hours. The filtrate from the milk or the filtered aqueous extract of cheese should be neutralized with sodium carbonate, then shaken with half its volume of pure ether. Time should be given for the complete separation of the ether. Purified tyrotoxicon is insoluble in ether, and it probably owes its solubility in ether, at this stage, to the presence of impurities. After complete separation, the ether should be removed with a pipette and allowed to evaporate spontaneously from an open dish. The residue from the ether may be dissolved in distilled water and again extracted with ether; but repeated extractions with ether are to be avoided, for as the tyrotoxicon becomes purified it becomes less soluble in ether. To a drop of an aqueous solution of the ether residue apply the preliminary test with sulphuric and carbolic acids. To the remainder of the

Digitized by Google

aqueous solution of the ether residue add an equal volume of a saturated solution of caustic potash, and evaporate the mixture on the water bath. The double hydrate of potassium and diazobenzol will be formed if tyrotoxicon be present, and this may be recognized by its properties and reactions, which have already been described.

The physiological test should also be made, and for this purpose a young animal should be selected as the most susceptible.

Chemically, tyrotoxicon is diazobenzol; pathologically, it is a ptomain, *i. e.*, a basic chemical body produced by the putrefaction of organic matter. It is now a demonstrated fact that putrefaction is always due to some microörganism or germ. Just what germ tyrotoxicon owes its existence to, we are not positive; but it is either the butyric acid or some closely allied ferment. We have seen that if normal milk be inoculated with the butyric acid ferment prepared in the ordinary way, and then the air be excluded, tyrotoxicon will be developed in the milk. That the germ which produces this poison belongs to the anærobic class we have frequently demonstrated by the following simple experiment:—

Inoculate two portions of the same milk with some poisonous milk or cream. Place one portion in a bottle, filling the bottle completely and stopping closely in order to exclude the air. Place the second portion in an open dish and stir occasionally, in order to bring the milk freely in contact with the air. After a few days, it will be found that although both portions of milk have decomposed, the nature of the decomposition has not been the same in them. Tyrotoxicon will be found only in that portion from which the air has been excluded.

A nice practical application of this was furnished by a case occurring at Long Branch, N. J., and reported by Drs. Newton and Wallace. A number of persons at the hotels were poisoned by milk. Investigations showed that the cows were healthy, their food good, and their pasture and stables all in good sanitary condition. The milking was done at the usual hours of midnight and noon. The milk drawn at midnight was cooled by being left in cans surrounded by water until morning, when it was sent to the hotel. This milk never produced any unpleasant effect. The noon milk was placed in closed cans as soon as drawn, with all the animal heat still in it, and carted a distance of eight miles during the very hot days of August. This milk was poisonous, and from it Drs. Newton and Wallace separated tyrotoxicon.*

The relation between this poison and cholera infantum I have discussed elsewhere, and it will be referred to further, in a paper to be read before another Section of this Congress.

---

* *Medical News*, September 25th, 1886.

Digitized by Google

Dr. HENRY SEWALL, of Ann Arbor, Michigan, read an abstract of a paper entitled—

EXPERIMENTS ON THE PREVENTIVE INOCULATION OF RATTLESNAKE VENOM.

EXPÉRIENCES SUR L'INOCULATION PROPHYLACTIQUE DU POISON DU CROTALE.

EXPERIMENTE ÜBER PRÄVENTIVIMPFUNG DES GIFTES DER KLAPPERSCHLANGE.

This work was undertaken with the hope that it might form a worthy contribution to the theory of Prophylaxis.

I have assumed an analogy between the venom of the poisonous serpent and the ptomain produced under the influence of bacterial organisms. Both are the outcome of the activity of living protoplasm, although chemically widely distinct, the ptomains belonging to the group of alkaloids, while the active principles of the venom seem, according to Mitchell and Reichert and Wolfenden, to be of proteid nature.

I have assumed, further, that it is through the ptomains produced by their vital metabolism that bacteria cause disease. If immunity from the evil effects of snakebite can be produced in an animal by means of repeated inoculation with small doses of the fatal poison, we may suspect that the same sort of resistance toward germ disease might follow the inoculation of the appropriate ptomain, provided that it is through the products of their activity that bacteria produce their fatal effects.

It is a matter of common experience that the repeated exhibition of poisons of various kinds gradually endows the organism with a power of resistance against the drugs employed. And yet there is reason to believe that this resistance may result from either of two opposite conditions impressed upon the living parts of the body—a pathological or a physiological. In the first case the sum total of energy of the protoplasm is diminished; its irritability is lowered as well as its efficiency as a machine. In the second case, the total energy of the protoplasm is not diminished, but, perhaps, is even increased as the effect of the inexplicable tendency of living matter to adjust itself to its environment. Such a physiological resistance is shown by the secretory cell, which does not digest itself, and by the unicellular animals, which dissolve ingested matter but spare their own substance.

The venom used in these experiments was obtained from the Massasanga, *Crotalophorous tergeminus*, one of the smallest of the rattlesnakes.

A few drops of the poison were diluted with glycerine, and known quantities of this mixture, added to water, were injected hypodermically into pigeons. The minimal fatal dose caused death in from fifteen to twenty hours; large doses in from two to three hours. Pigeons were inoculated with amounts of the poison less than the fatal quantity, and after recovery from the more or less severe illness which usually succeeded the inoculation, the operation was repeated again and again, at intervals of one to three days, with gradual increase in the amount of the poison used.

The pigeons thus treated appeared to retain perfect general health, and could bear, without injury, constantly increasing doses of the venom; in one case seven times the fatal amount of poison was injected without effect. The prophylaxis gradually fails after the inoculations are discontinued; for an amount of poison which may be injected with impunity at the end of a series of inoculations, may be fatal if given a few months after the prophylactic treatment has ceased. It has been found, however, that immunity from the effects of ordinarily fatal doses of the venom is retained over a period of at least five months after cessation of the course of preventive inoculation.

Digitized by Google

In answer, then, to the main question proposed at the beginning of this research, it can be said that repeated inoculation with sub-fatal doses of rattlesnake poison produces in pigeons an immunity from its ill effects when much more than the usually fatal amount is administered; this condition of prophylaxis persists with gradually failing perfection over a period of many months.

Dr. N. S. Davis, of Chicago, Ill., presented a paper entitled:—

CELLULAR DIGESTION; ITS UTILITY IN PATHOLOGICAL PROCESSES.

DIGESTION CELLULAIRE; SON UTILITÉ DANS LES PROCÉDÉS PATHOLOGIQUES.

ZELLENVERDAUUNG; IHR NUTZEN IN PATHOLOGISCHEN PROZESSEN.

I offer the present paper, not because the views expressed therein are definitely proven, but hoping that they may be suggestive, and by discussion lead to an elucidation of some points now obscure.

In pathological processes cellular digestion is useful in the removal of foreign bodies, if we understand by foreign bodies such as are unnatural or no longer useful in the tissues. It is not every foreign body that is digestible; it is only those of organic origin. They consist chiefly of dead tissue, fibrin, ligatures, silk, cotton, etc. These substances, as it were, melt away under the influence of the living tissue about them. They are not soluble in the lymph or blood, as is readily shown by noticing that no change occurs on placing them in these fluids which have been drawn from the body, and are maintained at the bodily temperature.

Other insoluble substances exist that, when *imbedded* in living tissues, are slowly transported piecemeal and deposited elsewhere in the body, and often scattered widely in it. Such are pigments, charcoal, carmine, and silicious and calcareous substances. That this transportation is accomplished by the white blood cells, is a fact well established. The process is too well known to require illustration. This group of substances differs completely from the first one mentioned, in not undergoing true absorption, since the particles composing them remain in the tissues, though not perhaps originally deposited. Substances of the first group disappear entirely.

The query naturally arises how are these insoluble substances so completely removed. That the vitality of the surrounding tissue is an all important element is evident, since, when the same substances are placed in dead tissue, no disappearance takes place. The authors of most text-books content themselves by simply saying that these bodies are capable of solution in the living body, without telling us how.

Within the last three or four years, almost simultaneously, several have tried to explain this disappearance of non-soluble bodies, upon the ground that they were digested by the living tissues, and chiefly, if not wholly, by the leucocytes, which mass themselves about foreign bodies and sources of irritation.

In 1884 I began to advocate this view to my students, for it seemed the most probable explanation of the phenomena. It was self evident that the presence of living tissue was essential, and the uniform presence of leucocytes covering and permeating the disappearing body suggested that their vital activity was the essential factor, and

Digitized by Google

that probably they acted as an organized ferment or digester. The existence of a non-organized ferment has been suggested but never proven.

A few months later the articles by Metchnikoff, which are now well known and which first appeared in "Rouskaia Medicina," * on intra-cellular digestion, attracted my attention. To him belongs the credit of placing in form the theory of intra-cellular digestion as applied to insoluble but removable bodies. He did more than affirm this theory, for he cited proofs of such action by leucocytes and their analogues in invertebrates. All cells possessing this power he termed phagocytes. He found this function most fully developed in mesodermic cells, many of which are amœboid and devour foreign substances, for instance, elements already used and dead in the organism. The ability of leucocytes to take up and transport particles of foreign bodies has long been known, but Metchnikoff was the first to demonstrate their power of digesting some particles within themselves. This he showed in the atrophying tails of tadpoles,† which are filled with amœboid cells containing distinguishable particles of nerve and muscle fibres of varying size, according to the degree of intra-cellular digestion that has taken place. A similar removal of useless parts was observed in invertebrates.‡ In the lowest forms, the sponges and cœlenterates, the property of intra-cellular digestion is possessed by both ecto- and endo-dermic elements, and in those a little higher by mesodermic elements also. When the body to be removed was too large for a single cell to manage, several united, and thus produced what is analogous to the giant cells of vertebrates. Invertebrate cells were found to exercise a power of choice in regard to what they would thus appropriate.

These views have been placed before English readers most clearly and attractively by John Bland Sutton, in lectures§ delivered at the Royal College of Surgeons, London, in 1886, and since elaborated in the volume known as "An Introductory to General Pathology." ‖ Sutton cites additional illustrations of the power of intra-cellular digestion by the cells of invertebrates, and confirms the observations of Metchnikoff, on the atrophying tails of tadpoles.

These observations enlarge the function of the leucocyte and make its analogy to the amœba perfect. For within the living body they lead an independent existence, taking up nourishment, doubtless, usually, in liquid form, but at times in the form of solids, which must be digested by them. They take within themselves these solids by movements of their protoplasm precisely similar to those of the amœba.

To illustrate intra-cellular digestion in pathological processes, mention may first be made of its utility in the removal of blood that has extravasated into the tissues. In such cases the liquid portion of the blood is rapidly removed through the lymph channels. A part of the solid cellular constituents is also thus removed. Another portion breaks down into a granular mass which causes the pigment of the red cells either to go into solution in the plasma, thereby producing characteristic staining of the surrounding tissues, or to crystallize and be deposited in them. The largest part, however, of the solid and cellular elements, as well as of the granular matter thus formed, is taken up by the leucocytes which rapidly fill the extravasated tissues. Not unfrequently, a perfect red corpuscle can be found within a leucocyte, and others in all stages of disintegration and dissolution can be seen there. The red corpuscles when thus swallowed diminish in size and gradually disappear, numerous minute granules of brown pigment being all that is left of them. Thus the leucocyte is transformed into

---

* Nos. 1, 3, 4, 5, 6, 1884.

† "Rouskaia Medicin a" No. 1, 1884, and *Revue des Sciences Médicales*, Tome xxv, p. 69.

‡ "Rouskaia Medicina," Nos. 3, 4, 5 and 6, 1884, and *Revue des Sciences Médicales*, Tome xxv, p. 70.

§ See *British Medical Journal*, 1886.

‖ Published in America by P. Blakiston, Son & Co., Philadelphia, 1886.

Digitized by Google

a pigment carrier which may transport the pigment granules to a considerable distance and cause finally their elimination from the system, or their deposition elsewhere. In the course of these changes, undoubtedly the dissolution of the stroma of the red cell is brought about by the power of the leucocyte of intra-cellular digestion.

We, without doubt, see this same power exerted in the removal of fibrinous clots and exudates. For example, the fibrin which forms a thrombus may be thus removed, although it is liable to other methods of disposal. It may shrink into a dense compact mass, and thus remain almost indefinitely or undergo calcification. More frequently, organization or simple softening of the clot occurs. When organization takes place the mass of fibrine is first permeated from without inward by leucocytes. As these increase and fill the clot the fibrine disappears, and finally is completely replaced by embryonic cells which subsequently are transformed into fibrous tissue. The apparent melting away of the fibrine when invaded by leucocytes, is probably due to their digestion of it and appropriation of it as nourishment.

When such clots undergo simple softening and not organization, we cannot name so confidently the agent by which the softening is accomplished. Living cells do not play an important part in the process. For, according to the usual description, the clot is not filled with such cells, nor does the liquid composing the softened centre contain them. It may be said that, if in this case living cells do not cause liquefaction, there is no reason to suppose that they do in the former. The only answer that can yet be given is, that, from the fact that the fibrin, when organization occurs, disappears just in proportion as it is invaded by leucocytes, and begins to disappear about the edges and along lines where the leucocytes are most numerous, it would seem probable that their presence influences its disappearance.

The removal of fibrinous masses in this way is also seen in the changes that occur in the absorption of the exudate which fills the air cells of the lungs in croupous pneumonia. In the first stage of croupous pneumonia the alveoli contains serum, red and white blood cells, and desquamated pulmonary epithelium. After a time coagulation takes place, fibrin filaments appearing and binding together the cells. This constitutes the stage of red hepatization. As resolution begins the red corpuscles disappear, and hence the lung becomes decolorized and gray. The other cells become fatty. Many of the white corpuscles present the appearances of pus cells. Simultaneously with these latter changes leucocytes migrate freely from the vessels and mingle with the liquefying exudate. The lymph channels are crowded often with granular white corpuscles that undoubtedly have found their way back from the alveoli loaded with its contents. It is thus that resolution takes place under the influence of actively vital leucocytes in patients who scarce expectorate any of the exudate that has filled considerable areas of their lungs.

Cellular digestion in pathological processes is probably illustrated again in the separation of dead from living tissue when sloughs and sequestra are formed. The line of demarcating inflammation is filled with leucocytes, which apparently attack the dead tissue, and undoubtedly the dead cells in the inflamed zone; thus leading to the production of a line of separation or a solution of continuity in the solid structures. Indeed, the most important phenomenon of inflammation is the migration of leucocytes from the vessels and the filling of the tissues by them. In the lowest animals, which do not possess a blood vascular system, the process that is analogous to inflammation in vertebrates consists in the unusual activity and massing of amœboid cells about the source of irritation. The activity of these cells is for the purpose, if possible, of removing or destroying the irritant. The purpose is furthered in higher animals by the first outpouring of serum, since often the irritant is thus dissolved or diluted so as to destroy or neutralize its toxic power. When the serous exudate is inefficacious it becomes the function of the white corpuscle to attempt to remove the irritating body, if solid, either by transporting it granule by

Digitized by Google

granule to a distance, and thus lessening its amount, or by digesting it and producing chemical changes that will alter its character. Inflammation from this standpoint is a conservative process. While the process is conservative, it is provoked by those things that cause always some destruction of tissue, or of the cells composing it. Even the simplest trauma that may provoke inflammation, and whose action may only be momentary, causes destruction of tissue elements. The removal of these is probably the duty and reason for the migration of the leucocytes. They accomplish the object by transforming the albuminous substance of the dead cells into soluble substances, or by appropriating them to their own nutrition.

The behavior of the leucocytes in the walls of abscesses suggests that in the forming of such cavities they take an important part. The tissue cells are probably destroyed by the cause of the purulent inflammation, but their removal cannot be thus accounted for, and so they disappear only in the presence of leucocytes, and in proportion to the abundance of the latter they would seem dependent on them for solution. Lining the abscess cavity are found a mass of round cells which, by becoming detached and mingled with fluid contents, are transformed into pus cells. As we pass outward from the cavity these round cells are at first so abundant as to be the only visible elements; further out they are infiltrating tissue that is in an atrophying or disintegrating condition, and in proportion as we pass from the centre the infiltrating cells are less numerous and the normal cells more perfect and natural, until the healthy tissue is reached.

Metchnikoff has urged that the leucocytes are the natural enemy of pathogenic bacteria. He points to the numerous examples that have come under the notice of pathologists, of the occurrence of bacteria within the white blood cells. He thinks that this is brought about by an attempt on the part of the leucocyte to destroy and remove the offending body. This endeavor is not always successful, for the protoplasm of the white cells seems to be a favorite place for the growth of some bacteria, but when it is successful recovery takes place. Metchnikoff first described the absorption of bacteria by leucocytes in a fungoid disease which attacked Daphniæ, or water-fleas.* The colorless corpuscles of the Daphnia could be watched as they attacked, devoured and digested the foreign substances. When anthrax bacilli were introduced into the tissues of frogs similar changes were produced, the leucocytes devouring and causing the disappearance of the bacilli.† The examination of two fatal cases of erysipelas‡ showed the cocci free in the tissues, never enclosed in cells. However, in numerous convalescent cases they were found in great numbers enclosed in leucocytes. Thus imbedded, all stages of transition, from fresh, apparently normal cocci to those consisting of mere grain-like detritus, could be observed. That leucocytes remove bacilli that are dead or inactive is quite evident, but that they are generally inimical to bacteria, or that they generally attempt to destroy them, is not proven. For instance, the experiments of Dirckwick-Holmfeld with anthrax bacilli, whose virulence was made of different degrees by Pasteur's method, show that when introduced subcutaneously into rats, cats, etc., it produces local suppuration, and more perfectly in proportion as it has been weakened, or as the animal is resistant to it. He, therefore, in agreement with Metchnikoff, regards the purulent inflammation as a conservative process. But he did not agree with him as regards the mode of destruction of the bacilli. He found only a few of the microörganisms in the pus cells and many in the fluid of the pus, where they died and disintegrated. The bacilli in pus in the animals most resistant to them underwent changes according to the age of the inflammation. In pus twenty-four hours old they were shorter and thicker than natural and surrounded by a clear, capsule-like zone. These rods grew well on culture media and were fatal to mice. In pus forty-eight

* Sutton, J. B., "Introductory to General Pathology," p. 123.

† *Virchow's Arch.*, Bd. 97, Seit 502.

‡ *Virchow's Archiv*, CVII, 2, p. 209, 1887.

Digitized by Google

hours old the bacilli had become finely granular, many even mere granules or short, glistening, often irregularly formed rings. They were no longer capable of cultivation nor toxic to mice. Living bacilli were found very exceptionally in older pus. In reply to the query whether the bacilli had not been first swallowed and destroyed by the pus cells, and subsequently disgorged, the author urged the lesson of the following experiment. If pus containing living bacilli is sealed in a tube that is kept at 36° C. from forty-eight to seventy-two hours the changes above described will take place, but if bacilli growing in bouillon is similarly treated, they lose neither life nor toxicity. He supposed that the pus cells cease to be active, living factors when thus treated. It is evident, however, that the richly cellular pus is inimical to the bacilli, and probably if the cells are not their destroyers, something resulting from the presence of the cells is.

While calling attention to the utility of intra-cellular digestion in pathological processes, I have mentioned only the white blood cells as possessing the power. Undoubtedly connective tissue cells and granulation tissue cells, or embryonic cells, also possess it, and probably some forms of tumor cells.

It is a well-known fact that inflamed or dead bone will undergo absorption unless suppuration intervenes. Numerous theories have been propounded to explain the phenomena. Rindfleisch has suggested that the blood in the congested vessels contains an excess of carbonic acid, which may dissolve the lime salts, forming an acid carbonate.* Others have supposed that lactic acid is formed, which produces a soluble calcic sarcolactate. These hypotheses are improbable, for bone exposed indefinitely to the influences of purulent inflammation loses little of its substance and its surface may remain smooth. Virchow believes that cells derived from proliferation of bone corpuscles are the active agents. There is little doubt that the living cells which surround or permeate bone when it is undergoing absorption are the active factors in its removal, although most pathologists do not wholly agree with Virchow as to their origin. Usually the absorption power is ascribed to granulation cells or to osteoclasts, both of which originate from leucocytes. To illustrate, recall the appearance of the changes that occur in rarefying osteitis. The bone is indented by deep or shallow, simple or complex depressions that are filled with cells of varying size. These cells originate as do granulation cells, but among them giant cells are oftener found than in ordinary granulations. Frequently they are continuous with or outgrowths of neighboring granulation tissue. These so-called osteoclasts fill the pockets in the bone, which so evidently melts before their encroachments that no one can doubt their activity in its removal. It is literally eaten away by them.

Tumor cells are so various in character that we cannot affirm of all kinds digestive power, but some undoubtedly possess this function. The cells of infiltrating tumors have absorptive powers. This capability is most noticeable when bone is involved, for then the hard, compact bone is readily seen to have disappeared and to have been replaced by the new tumor tissue. When tumor cells infiltrate other tissues, the normal cells are seen to undergo atrophy and to finally disappear and be replaced by the tumor structures. This process is ordinarily explained by assigning to the growing tumor a mechanical power of interfering with the blood supply of the neighboring tissue, and therefore inducing atrophy. However, the disappearance of inert lime deposits in bone can scarcely be explained in this way. The influence of the contact of active, growing cells seems more important.

Can living cells render solid materials soluble by simple contact with them? There is little doubt that intra-cellular digestion takes place. But in the absorption of bone there is no evidence that particles of it are first detached, swallowed and then digested by the osteoclasts. On the contrary, the latter apparently erode it by their contact.

* Pepper, A. J., "Surgical Pathology," p. 234.

Digitized by Google

In this respect they behave as do many bacteria, which liquefy solid culture media by the changes incident to their own multiplication and growth in contact with them. The living cell thus behaves like an organized ferment. When, however, the substance to be removed is granular, the granules are generally taken within the cell before solution or digestion occurs. It would, therefore, be better to describe these processes by the term *cellular digestion* rather than by the now limited appellation used by Metchnikoff, of intra-cellular digestion.

In the course of such digestion, it is probable that various chemical decompositions are produced and new bodies formed, according to the nature of the substances attacked and the needs of the digesting cell. What these chemical changes are we do not know. There are, however, a few facts that may throw light on the subject or form a starting-point for an investigation to elucidate it. It seems probable that leucocytes have a predilection for peptones, and if so, the thought suggests itself whether this is not the form in which they require their nourishment, and whether its formation is not the first step in the process of cellular digestion by them. Hofmeister believes that in the subepithelial tissue of the digestive tract leucocytes take up peptone that has been formed in the alimentary canal.* That they play an important part in the absorption of nutriment is evident, since they are much more numerous during digestion than at other times, and it is known that they take up the emulsified fat and aid in its transportation. The absence from the blood, during digestion, of dissolved peptone is most easily accounted for, upon the supposition that it has been appropriated by the cellular elements. Hofmeister has also shown that the peptone which is found in pus is contained in the corpuscles.† There is no evidence as yet, however, that peptone can be produced by the corpuscles, since in the wall of the intestines it is furnished to them already formed, and also probably in pus. For the microörganisms peculiar to the latter have the power of forming peptone from albumins on which they are grown. The streptococcus pyogenes, when grown in a vacuum on white of egg or on beef albumin, causes this disintegration and energetic peptonization.‡ The micrococcus pyogenes aureus also converts albumins into peptones.§

The peptone in pus may, therefore, be formed by its bacteria and absorbed by its corpuscles.

If these views in regard to cellular digestion are true, we must recognize among bodies foreign to the living tissues a group of digestible substances. We can then classify foreign bodies into: 1, soluble bodies; 2, insoluble but digestible bodies; 3, insoluble, granular and transportable bodies; 4, insoluble bodies, completely impregnable to leucocytes and the surrounding tissues.

DISCUSSION.

Dr. DANFORTH thanked Dr. Davis for his very able and scholarly paper, but thought its scope was too narrow—that the digestive power alluded to was not limited to the leucocytes alone, but that the nuclei of all the tissues took part in the process. In traumatic states all the tissues become embryonic—the nuclei are liberated by the softening of their surrounding primal material, and become "amœboid" cells, or practically leucocytes. These amœboid cells acquire the digestive power which Dr. Davis claims for leucocytes alone.

---

* "Zeitschrift für Phys. Chemie," 4, pp. 253–281; 5, pp. 127–151; 6, pp. 51–73.

† "Studies from the Biological Laboratory, Johns Hopkins University," Vol. IV, No. 1, pp. 8–10.

‡ "Crookshank's Practical Bacteriology."

§ *Ibid.*

Digitized by Google

Dr. SHAKESPEARE regretted that he had not heard the paper just communicated, but he inferred from the discussion that the cellular organisms active in the process of destruction and repair of the tissues were under consideration. He wished to call attention to some observations he had the opportunity to announce in a series of lectures before the College of Physicians of Philadelphia, an abstract of which was published some years ago in the *Medical News*, of Philadelphia. The subject of those lectures was the intimate nature of inflammation in the non-vascular tissues. The minute normal structure of the cornea and hyaline cartilages was revised as a foundation for the study of inflammation of them. He had found, with Thin, of London, that the ultimate bundles of fibrils in the cornea, as recognized by most histologists, have not the simple constitution commonly described. There is a further subdivision of the fibrils into much smaller bundles. But the point he wished to call especial attention to is the fact that each of these minute smaller bundles is enveloped more or less completely by a single layer of exceeding small, thin, flat cells, resembling, in shape and relations to each other and to the bundles which they cover, the flat, fixed cells which we are very well acquainted with in silver and gold-treated pieces of tendon. The apposition of three or more of these minute bundles causes the formation of linear lymph spaces, parallel with the direction of the bundles.

I found, with Thin, and Spina, of Austria, that hyaline cartilage is not the simple structure so commonly described, but that it has a framework of fibrous connective tissue which is normally masked by the hyaline substance which envelops, and by similarity of index of refraction renders the real complex structure ordinarily invisible. I have found the same minute bundles of fibrils and their enveloping, minute, thin, flat, fixed cells, as in the cornea. I may say that they are to be found also in bone and in the central nervous system.

Now, what I wish to say is, that in the non-vascular tissues I have found these minute, flat, fixed cells in the immediate vicinity of the point of irritation, showing all the evidences of reawakened activity, viz., cloudy swelling and division, accompanied by the signs of softening, to a slight extent, it is true, of the fibrils with which they are in contact, in advance of the invasion of the emigrant white blood cells. In advanced inflammation the progeny of these cells, with that of the recognized common fixed cells and the emigrant cells of the blood, constitute the mass of cells in the so-called embryonal tissue which replaces the normal structure, and it becomes impossible to definitely settle the origin of any single one of them. Dr. Shakespeare holds that these minute, thin, flat cells, which may be regarded as fixed, play an exceedingly important rôle, either in destruction or repair, possibly in both.

Dr. N. S. DAVIS, Jr., closing the discussion, said:—Dr. Danforth has remarked that I limited the power of cellular digestion too closely. I wish to state that my feeling is that other tissue elements contribute to the so-called absorption process, but on thinking over the various pathological lesions, I could find no illustration of digestion by the higher forms of cells, but only by those of embryonic form. The suggestion of Dr. Danforth and of Dr. Shakespeare, that the higher forms of tissue elements, under conditions of inflammation, return to an embryonic state not distinguishable from wandering blood cells, and then exert the same function, may be true.

To the question of Dr. Vaughn, whether any chemical ferment had been isolated in the cells, I can only say that in my search through the literature of the subject I have found no evidence of the existence of such substances.

Digitized by Google

Dr. S. K. JACKSON, Norfolk, Va., read a paper on—

NATURAL AGENCIES INHIBITING THE LIFE-PROCESSES OF PATHOGENIC ORGANISMS CONSIDERED WITH A VIEW TO THEIR THERAPEUTIC EMPLOYMENT.

AGENCES NATURELLES, ARRÊTANT LES PROCÉDÉS VITAUX DES ORGANISMES PATHOGÉNIQUES, CONSIDÉRÉS AU POINT DE VUE DE LEUR EMPLOI THÉRAPEUTIQUE.

NATÜRLICHE AGENTIEN, WELCHE DIE LEBENSERSCHEINUNGEN PATHOGENER ORGANISMEN VERHINDERN, MIT RÜCKSICHT AUF IHRE THERAPEUTISCHE ANWENDUNG BETRACHTET.

In considering the statements and estimates of bacteriologists as to the vast numbers, the rapid and indefinite multiplication, and general distribution of parasitic organisms (which we have no inclination nor reason to question), we are struck by the great disproportion between these numbers and the disturbances they occasion. We might suppose, from such an ubiquitous distribution of such vast multitudes, that zymotic diseases were everywhere and at all times prevalent, and that no one would be able to escape them.

It becomes an interesting subject of inquiry as to what prevents the ravages of these organisms, which are so freely taken into our systems with the food we eat, the water we drink, and the air we breathe, and why and how they are restrained in their work of devastation. Why is it that in an atmosphere surcharged with these pathogenic organisms many persons are unaffected by them, and only a limited number succumb to their attacks? It has been a generally satisfactory explanation that the vital force, the vital energy, the "principle of life," has enabled some to resist, and that those only yield to an attack whose vitality is diminished, whose vital power is crippled. This explanation might be in some degree satisfactory were it not for the fact that very frequently the most robust, those of greatest constitutional vigor, become victims of these attacks, and, *vice versa*, those of feeble constitutions escape; and thus the exactions of science require a more searching investigation into the causes of immunity on the one hand and of liability on the other.

These investigations have a high practical value; for while we are ascertaining the modes or agencies employed by nature for preventing, arresting, or crippling the predatory attacks of these microörganisms, we are at the same time learning the most efficient means that can be employed for these purposes, and may be able to assist her in this beneficial work.

Let us then study, by the light of science thus far vouchsafed to us, the conservative processes of nature whereby we are protected from "the pestilence that walketh in darkness, or from the sickness that destroyeth in the noonday."

As heat is the physical agent most generally recognized as affecting the life of microörganisms, its effects upon them have been so carefully studied as to render unnecessary anything more than a mere allusion to it here. The degree most favorable to their propagation and development—that above which they cannot live, as well as that below which they are either destroyed or rendered dormant until revivified by higher temperature—have been so accurately determined as to leave nothing to be added. But there is one point, in this connection, to which I wish to call attention, on account of its bearing on the recent popular practice of reducing the temperature of the body, in fevers of zymotic origin, by the external application of cold.

It has been found that the temperature most conducive to the vigorous activity of

Digitized by Google

pyrogenetic organisms is between 68° and 75° F. A departure from this either way cripples or retards their growth more or less in proportion to its distance above or below this temperature. Now, this being the case, the departure above, as in typhoid fever, for instance, is at least 30°, and 30° still higher is the temperature which Sternberg has declared to be the highest that can be borne by them; that is, that they are destroyed at 132.8° F. While it is not claimed that this destructive temperature could ever be reached or could be endured by the human system, still it is a fact, that as the temperature of typhoid fever reaches at least half-way between these points, the parasitic microbes, by the heat themselves have generated, are in a medium less favorable to their life and growth and more nearly approaching the point of destruction; and when the system of their host, the medium in which they are living, becomes cooled down, say, to its normal degree (98.6°), a point more favorable to them is reached; that is, one further removed from that which is calculated to destroy them. Thus, by this reduction of temperature, by means other than that of destroying the pyrogens themselves, the system is brought into the same condition in which the inception occurred, and the parasites, instead of becoming enfeebled, actually become reinvigorated, and the consequent disturbances are repeated.

Is it not reasonable to suppose that the reaction, the increase of heat, which I believe invariably follows the use of cold affusions, may be owing, to some extent at least, to the impulse imparted to the fever-producing organisms by the restoration of this favorable condition?

Fortunately we have more efficient, and in my opinion, more philosophical and less deleterious means of reducing the temperature of fever, which it is not our purpose now to discuss. But in view of the proposition herein advanced, it becomes a question of much interest and importance, whether it is not an advisable practice not to reduce the temperature of the body below a point which may safely be endured, until the destruction of the pathogenic parasites, when most probably of its own accord or without assistance the normal temperature will be attained. In my treatment of enteric fever I am perfectly satisfied with a temperature of 102°, which is not a dangerous degree, for it is borne for weeks without disaster.

Next to the destructive effects of extremes of temperature, the most clearly recognized natural agency which affects the vitality of pyrogenetic organisms is the production, in the medium in which they live, by means of their vital processes, of substances which are poisonous to them and cause them to die; that is, they are killed by the products of their own life processes.

No living being can long be kept alive when so confined in the medium in which it exists as to absorb the products of its own life processes, its own excreta. It might be supposed to die from exhaustion of food, but that this is not the case can easily be proved by replenishing the supply. A fish in a limited supply of water cannot long be kept alive, even though abundant and generous food be supplied. The experienced agriculturist knows that successive crops of rank-growing plants cannot be cultivated for a length of time in the same soil, even though that soil be liberally supplied with plant food.

The excretions, then, of all living organisms are inhibitory to their life processes, and to avail ourselves of this law with a view to its therapeutic employment, requires a minute study of diseases of zymotic origin, and may necessitate a new treatment, directed to the destruction of the particular organism which may be ascertained to be the pathogenetic factor in each of them. I do not suppose that this new treatment will require an entire abandonment of many of the reliable remedies which have been so long and so successfully employed, for it so happens, and it was the merest happening, that these remedial agents are, in many instances, the very best germicide that probably ever will be discovered for the very diseases for which they have been used.

Digitized by Google

We need not expect, for instance, to relinquish mercury in syphilitic disease, nor iodine in scrofula, nor arsenic and sulphur in herpetic affections, nor quinine in malarial fevers. We have been using these for years; we know not for what reason; but we, fortunately, in these conditions; have stumbled on the right remedies, and it may be we will never find any more efficacious.

But what is especially necessary to ascertain is: 1st. The particular organism producing each disease; and, 2d. The most efficacious means of crippling or destroying it, and, if possible, nature's mode of effecting its destruction.

Each of the agents claimed as "germicides" are not equally efficacious for the destruction of all organisms. Quinine, which we recognize as, par excellence, the antidote for the malarial poison, has no power to destroy the organisms which, if they do not cause, exist in dyspeptic conditions. And on the other hand the salts of sulphurous acid, which are so destructive of the latter, have been proved to have no effect on the malarial organisms. (Sternberg.) Thus we see the absolute necessity of ascertaining the particular organism we wish to destroy. We must know to what class it belongs—whether animal, vegetable or fungus; we must know its life history, its life processes; the metamorphoses through which it passes—the length of time in passing through each; the stage of its life in which it is most virulent—that in which it is most vulnerable; the pabulum necessary to its growth and development; the toxic agents it is least able to endure—Nature's mode of destroying it, whether by limiting the duration of its life, or by poisoning it by the resultants of its own life processes; its natural enemies—in a word, what it has to contend with in its struggle for existence.

When we have acquired definite knowledge on these several points, the various germicides will be more intelligently employed than they are at present. Mercury and nux vomica, so destructive to animal life, will not be used for the destruction of a vegetable organism; chlorine, which hastens germination, will not be used to destroy *spores;* the manganates will not be relied on to cripple aerobiotic forms, whose very existence depends on the oxygen which these salts supply. We will soon find that there is no one germicide, but they may be as numerous as the germs to be destroyed. But the object of this paper is not to study the numerous germicides that have been proposed or that may be employed; nor the strength needed for accomplishing their purpose; nor how, in destroying the parasites, the system of the host can be protected from their poisonous influence. These important considerations must be left for others to discuss. Our object is to consider only those agencies which nature is known to employ, and our limited knowledge up to this time has enabled us to discover but a few.

If any one is disposed to doubt the general proposition upon which this study is founded, let him examine closely any one of the various fermentations; for, as the organisms producing them are so similar in many respects, to each other, and also to those which we know to be pyrogenetic, the consideration of any one will unravel the mystery of all, to some extent at least, and will inform us as to the behavior of all those organisms which, like the Torula cerevisiæ, are fungoid in their character, and have the power of converting starch into dextrine and sugar, and sugar into alcohol with the evolution of carbonic acid gas. Suppose now a vat is supplied with the materials necessary for this process, what occurs? So soon as the yeast is added and the proper temperature secured, a violent action is set up, and the various stages in the process just alluded to occur. But the particular point to which I wish to call attention is the fact that after a certain time a sudden stop occurs and the yeast, which was in lively motion all through the mass, suddenly falls to the bottom. What has arrested this activity? Perhaps it may be said that the sugar is all used up, and that the organism which has caused the fermentation has exhausted all of its pabulum and has died from starvation. This can easily be tested by adding more sugar, which would furnish

Digitized by Google

the necessary nourishment, and ought to start the process afresh, if it was food that was needed. The addition of sugar at this stage will have no such effect—it will only destroy the "dryness" of the fluid and communicate to it a dead sweetness. What then has been the cause of the stoppage of the fermentative process? Clearly it is to be found in the fact that there has been produced in the liquor some agent that has destroyed the vitality of the fungus, which, by its life processes, had been causing the chemical changes that had occurred. If the process had been taking place in a closed vessel, it may have been a saturation by carbonic acid that inhibited the life process; if in an open vessel, from which this gas could escape, it was evidently due to a saturation by the alcohol formed. Whatever it was, it is evident that the organism has been crippled or destroyed by something which has been produced by itself, a product of its own life processes.

It matters not what may be the nature of the fermentation, whether vinous or acetous, or whether it be a putrefactive process—a decomposition of animal or vegetable matters, it may be stated as a fact, that whenever decomposition occurs from the presence of a vitalized agent, this agent is eventually either arrested in its processes or destroyed by the resultants of that decomposition; in other words, by the products of its own life processes, or really by its own excreta. If this be true, we can enunciate the general law, that *no organism can live in its own excreta.*

This law furnishes us with a valuable hint in selecting the agents with which to lessen the vital activity of pathogenic parasites. If we surcharge them with these resultant products we can arrest their vital processes, before they would be arrested by the slow procedure of fermentation, and thus we may be able to save the system of their host from the protracted disturbances which would otherwise continue throughout their whole life cycle. I shall suggest but a few, indeed, I am as yet prepared to suggest only a few, instances where this plan of treatment might be adopted to advantage.

In the case proposed, where the organism is a true fungus, that is, without chlorophyll, and therefore unable to assimilate or appropriate carbonic acid, but must exhale it, as do animals, the indication would be to saturate the system with alcohol, or with carbonic acid, by internal administration. Brunton asserts that the "yeast plant is killed by the alcohol which it produces, so soon as this amounts to 20 per cent., and other organized ferments have their lives limited in a similar way." According to the observations of the late Dr. W. Farr (quoted by Brunton), "Alcohol appears to arrest the action of zymotic diseases, as it prevents weak wines from fermenting; like camphor, alcohol preserves animal matter—this is not now disputed. But may it not do more? May it not prevent the infection of some kinds of zymotic disease?" It is constantly contended by persons in malarial districts that whisky increases the effects of quinine.

With regard to the anti-fermentative power of carbonic acid, I have myself tested it by sealing apricots, of all fruits the readiest to sour, in a jar filled with this gas. The fruit remained firm for months without the slightest appearance of fermentation. That they lost their flavor is no argument against my position, for they did not ferment or decompose, and probably never would so long as they were tightly sealed.

But the most valuable anti-ferment we possess, which we all recognize as the remedy par excellence in malarial fever, is quinine, and it is more than probable that its power may be accounted for on the very principle for which we are contending, for it is one of the most highly carbonaceous articles of the Materia medica. Its formula, according to Miller, is $C_{20}H_{24}N_2O_2,3H_2O = 324 + 54$, and it is only exceeded in the amount of carbon by gallo-tannic acid, which, but for its unstable composition, might be supposed to be a more powerful anti-ferment, but by its absorption of oxygen gallo-tannic becomes converted into gallic acid, which contains much less carbon and could, therefore, never be suspected of possessing this power of checking fermentation. The results

Digitized by Google

of recent researches into the effects of quinine upon the malarial microbe are confirmatory of this position.

Again, in those dyspeptic conditions in which sulphureted hydrogen is freely generated, how easily are they prevented by the use of condiments or salads which contain sulphur, or allyl sulphide, *e. g.*, garlic, onions, leeks, cress, etc., and how decidedly is this condition corrected by asafœtida, "which contains a larger proportion of sulphur than garlic" (Miller), and still more quickly and radically by the salts of sulphurous acid, viz., the sulphites of soda or potash.

Another remarkable instance of the effects of saturation by the products of the life processes of pyrogenetic organisms, I have elsewhere stated and explained, in a paper on "The Ammonia Treatment of Typhoid Fever" (*Journal Am. Med. Ass.*, Vol. III, p. 183), in which disease a process occurs analogous to, if not identical with, putrefaction, in which ammoniacal gas is freely evolved. If I may judge from the reports of the various modes of treatment adopted in this country and in Europe, there has been nothing proposed or tried which has so controlling an influence on this fever as the saturation by the salts of ammonia, many times cutting it short at the end of the first or second septenary, and always keeping it under control and conducting it to a successful termination.

Another striking illustration of the principle for which we are contending, is the new treatment proposed by Bergeon for phthisis pulmonalis. Every clinician must have observed the odor of sulphureted hydrogen arising from the sputum of a phthisical patient. Enclosing it for a few days in a closely stopped vial, its existence will be revealed by the smell and without a chemical examination. The bacillus tuberculosis is abundant in the *fresh* matter expectorated, but is soon destroyed by being saturated with this gas generated in the decomposition, and according to the principle herein contended for, we ought to be able to destroy the parasitic bacillus by infusing into the system of the host this very gas, as proposed by Bergeon. I am inclined to regard favorably this plan of treatment, not only because of its being consistent with this law as we have enunciated it, but also because of its similarity to a line of treatment which I have been employing for some years with marked success. I allude to the free administration of the salts of sulphurous acid and the sulphides, *e. g.*, the sulphide of calcium. These means have a most controlling influence on the hectic paroxysms, and have effected the cure of five cases in my hands in the past ten years. Bergeon's method seems to do in a few weeks what it took some months to effect by my plan, and I believe is well worthy of trial.

Other instances will occur to the ingenious physician, and I must pass from the consideration of this mode of checking the life processes of microbian parasites to still another which has lately loomed up into great prominence by a claimed successful application recently made of it in Italy.

It is reasonable to suppose that these several parasitic organisms, in common with all organized beings, have enemies with which they have to contend in their struggle for existence.

In the present state of our knowledge it is impossible to point out, in every instance, the modes of destruction, whether by directly devouring their prey—by robbing them of needed food—or by inhibiting the life processes, either by so altering the chemical constitution of the medium in which they live as to render it incapable of supporting life—or by the direct generation of poisons.

Our study of this subject, so far as it has been pursued, seems to justify the conclusion that one or other of these modes of destruction occur in every instance in which different microbes are brought into contact.

A number of these antagonisms have been known for a long time, at least by their effects, but these effects have only lately been suspected as being due to destructive

Digitized by Google

warfare between pathogenic organisms. The antagonism between the malarial poison and the cause of typhoid fever was long ago noticed. In 1851, Valleix remarks that "Boudin has collected a great number of facts to prove it, but that the fact was not generally admitted at that day." Flint, in 1867, says: "It is certain that in some situations in this country well-marked cases of typhoid fever were hardly known so long as malarial fevers were rife, but that the former became the common form of fever after intermitting and remitting fevers *ceased to prevail.*" Of this fact he "has been personally cognizant in two situations, viz., Buffalo and Louisville." Wood, in 1858, speaks of typhoid fever as "the ordinary endemic fever of those portions of the United States in which miasmatic fevers *do not prevail.*"

We are aware that some authorities contend that the form of fever, first noticed during our war between the States, to which Woodward, of the Army, gave the name typho-malarial, is an admixture of the two diseases, or "a mild form of enteric fever complicated with symptoms of malarial poisoning" (Sternberg); but it is more than probable that further investigation will demonstrate this to be a distinct form of fever, having no relation to either of these forms, but caused by a different pyrogenic organism. During a residence of twenty years in a locality most favorably situated to have witnessed a commingling of these two poisons, if they ever were commingled, I never met a case of the kind. The location referred to was about midway between the banks of the Potomac river, upon which malarial fever was endemic, and the Catoctin range of mountains, at the foot and sides of which typhoid fever, if it was not always prevalent, was only for want of subjects to attack. There was a distinct line between the two, which could be traced for miles, beyond which neither fever was ever known to cross. If there had been a possibility of their mingling it must have occurred on this middle ground, but as far as my knowledge extended (and my practice included almost every estate on this line, for many miles), not a case of admixture occurred. It was frequently the case that I was obliged to remove negroes from a "quarter" in the low grounds across the line to escape malarial influences, but when they were on the typhoid fever side I never knew them to have the two diseases at once. Indeed, I never met with a case of "typho-malarial" fever until my removal into the tidewater region.

But my purpose is not to discuss this, but only to express my belief, and give my reasons therefor, that it does not furnish an argument against the antagonism of typhoid and malarial fevers. In my experience they have been so distinct that I should expect an impregnation of the one to prevent the pyrogenetic influence of the other.

But there is another instance of antagonism which was not only known, but of which practical application was made, many years ago. I allude to the old-fashioned yeast poultice as an application to ill-conditioned ulcers, just as nowadays we use carbolic acid and the various anti-suppurants. This yeast poultice was the most valuable means at our command thirty or forty years ago, and it was but little, if at all, inferior to the agents at present used for this purpose. How can its effect be accounted for? Clearly by the antagonism between the Torula cerevisiæ and the pyogenic organism of the ulcer. What this antagonism is, and how the one organism cripples or destroys the other, is easily ascertained, and the study of it becomes interesting and valuable as furnishing a type of the antagonisms between animalcular microbes and those of a vegetable or fungoid character.

If the bacillus subtilis be introduced into a fluid containing Torula cerevisiæ, the cells of which are in active multiplication, there will be observed the greatest activity on the part of the former microbe in feeding on the Torula cells, until he either becomes satiated or sickened and probably poisoned. The pyogenic microbe soon becomes sluggish and remains so still and quiet as to enable us to examine him, as we had not been able to do before his fatal meal. It requires, however, further investigation to

Digitized by Google

determine positively that death has occurred; it is more than probable that it has, for long continued watching has not discovered a restoration of motion.

The good effects, then, of the yeast poultice are to be explained by its furnishing the pyogenic microbe with a food which it devours voraciously, and which is evidently poisonous to it.

Brunton alludes to yeast as being employed in typhoid fever and dysentery. If its administration in these diseases is beneficial, it is probably, as they are of microbian origin, due to this antagonism.

Another remarkable instance of antagonism, and of which also practical application has been made, is that of the organism developed in the infusion of Jequirity (Abrus precatorius) to the parasite which is the acknowledged cause of trachoma. I do not contend for it as the best remedy for this disease, for the antagonizing organism may produce as much disturbance as the parasite. But this antagonism is none the less certain and has been demonstrated. Lupus, which has been proved to be occasioned by a parasitic organism, is said to be cured by the infusion of the same substance (Jequirity) in which its peculiar organism has been developed. (*Journal Am. Med. Ass.*, Vol. v, 324.) Chronic ulcers are also said to be improved by it (*id.*).

It is probable that we are furnished with some instances of antagonism by the well known drug ergot. There has been much uncertainty and difference of opinion with regard to its therapeutical effects. This uncertainty is probably owing to the great changes which the fungus undergoes by age, and also by the processes required for the manufacture of its various preparations. It seems to me to be necessary to distinguish between the effects of the introduction of the fungus itself into the system, and those of its alkaloids. To the first, in my opinion, is due the stasis which is the cause of gangrene, and to the latter the disturbances of the nervous system. If its claim to be prophylactic and curative of pertussis is confirmed, it is a good illustration of the antagonisms for which we are contending. So, also, if we can establish its claim to be prophylactic of puerperal fever, we shall find it has been of more service than we had suspected when using it merely for its ecbolic effects.

In conclusion we will hastily enumerate a few more instances of these antagonisms which have been proven or suspected.

Koch has shown that the comma bacillus is destroyed by the bacterium termo, and that, therefore, "in arresting putrefaction of the menstruum, you really preserve the comma bacillus." (*N. Y. Med. Abstract*, IV, p. 32.)

The virus of swine plague has been proved to be destroyed by the bacterium of putrefaction. ("Special Report Dept. Agriculture," No. 34, pp. 15, 16.)

Cantarin, of Naples, has claimed that the bacillus tuberculosis is also destroyed by the bacterium termo, and has reported a phthisical case in which it was being employed with much benefit.

Erysipelas is said to be destructive of cancer. Profs. Janick and Weisser (*Centralb. f. Chir.*, *Edinburgh Med. Jour.*, Nov.) declare that "the cancer cells and nests can be seen to perish before the advance of the erysipelas cocci."

Finally, one of the most remarkable evidences on this point is furnished by the reports of yellow fever, in New Orleans in 1832. It is recorded that so soon as the cholera made its appearance the epidemic of yellow fever declined. (Prof. James Jones, quoted by Dr. A. G. Tebault, *Virginia Med. Monthly*, Vol. I, No. 7, p. 397.) Tebault also asserts (*ib.*) that Asiatic cholera has been known "to displace remittent fever, and again give way to it."

These facts are adduced to substantiate the position herein assumed, and to prove that the antagonisms claimed really do exist; but as to their employment as therapeutic means, we need more accurate knowledge of them, and must learn to modify them by cultivation or in some way to keep them under control, for fear of inducing by

Digitized by Google

the antagonizing organisms a disease more to be dreaded than the one we wish to combat.

Enough is known of them, however, to establish their importance as subjects of study, from which, it is more than probable, important results will be reached.

[AN ABSTRACT.]

D. E. SALMON, D. V. M., H. A. R. C. V. S., and THEOBALD SMITH, M. D., Washington, D. C., presented an abstract entitled—

EXPERIMENTS ON THE PRODUCTION OF IMMUNITY BY THE HYPODERMIC INJECTION OF STERILIZED CULTURES.

EXPÉRIENCES SUR LA PRODUCTION DE L'IMMUNITÉ PAR L'INJECTION SOUS-CUTANÉE DES CULTURES STÉRILISÉES.

EXPERIMENTE ÜBER HERVORBRINGUNG DER IMMUNITÄT DURCH SUBCUTANE INJECTION STERILISIRTER CULTUREN.

Bacteriological investigations of the past few years have shown pretty conclusively that during the multiplication of pathogenic bacteria there are formed chemical substances, or ptomains, which are poisonous to the animal economy. The researches of Brieger have done more than any others to strengthen this conclusion. It was suggested by some that it is these poisons (rather than any other vital manifestation of bacterial growth) which produce such grave symptoms in infectious diseases. In the spring and summer of 1886, Sirotinin made some experiments (*Zeitschrift für Hygiene* I, 463) with the bacillus of typhoid fever, in which he endeavors to show that the smaller experimental animals cannot be infected by this bacillus, but that death caused by the injection of cultures subcutaneously or otherwise, must be attributed to an intoxication caused by the presence of a ptomain in the cultures. The results actually proved that the injection of sterilized cultures may produce death accompanied by lesions resembling those produced by living bacilli. In a later publication (*Zeitschrift für Hygiene* I, p. 110) Beumer and Peiper, in a long series of experiments, are led to the same conclusion—that the typhoid fever organism does not multiply in the body of smaller experimental animals; that there is no true infection, and that the severity of the symptoms depends entirely upon the quantity of culture material injected—in other words, upon the quantity of the poison or ptomain therein contained. They also point out that death does not follow the injection of large doses, if small, non-lethal doses have been given previously, and from this fact they argue that perhaps immunity may finally be brought about by the injection of sterile cultures in successively large doses. That the chemical products of bacterial growth may produce immunity is no new theory, but it seems to have gained ground but recently among investigators.

Before the publication of the researches mentioned, our own preliminary experiments had been published. ("Proceedings of the Biological Society of Washington," Feb. 22d, 1886.) They showed very decisively that immunity might be produced by the chemical products or ptomains present in the culture liquid. They received but very little attention, however, and the above writers were very likely ignorant of their existence. It is for this reason that it seemed justifiable to bring them once more

Digitized by Google

before the medical world, represented by this international gathering, in as brief a manner as possible.

The experiments were made upon pigeons with sterilized cultures of the hog-cholera bacterium. Inasmuch as our investigations of this organism are but little known, a few facts concerning its nature should anticipate an account of the experiments themselves.

The bacterium was first described in the "Report of the Department of Agriculture," for the year 1885, as the cause of the disease among swine known in this country as hog cholera, and we refer those interested to that publication and the recently published report for 1886, for a more detailed account of the biological and pathogenic character of this interesting organism. Suffice it to say in this connection, that it is a *motile* bacterium about 1.2 to 1.5 μ long and .6 to .7 μ broad, staining readily in aqueous solutions of aniline colors, decolorized by iodine according to Gram's method. It does not produce spores. It grows readily in the ordinary culture media, such as beef infusion, blood serum, beef-infusion peptone, agar-agar and gelatine, which it does not liquefy. It is destroyed by an exposure, for fifteen minutes, to a temperature of 58° C. It may resist drying for more than a month.

In swine it produces a disease characterized by more or less extensive ulceration of the large intestine. It now and then assumes the character of a virulent and rapidly fatal septicæmia, the lesion being then ecchymoses in the subcutis and serous membranes, hemorrhages in all the internal organs, the lymphatic glands and in the stomach and large intestine. In all cases the bacterium is present in the spleen; in the hemorrhagic type it is very abundant in the spleen, and more or less so in the other organs.

In rabbits, mice and guinea pigs it produces a fatal disease characterized by an enlargement of the spleen and liver, by centres of coagulation necrosis in the latter organ. It is an organism easily recognized; for none has been described, thus far, identical with it. Klein described an organism resembling this in 1884 (*Virchow's Archiv*, XCV, (1884) 468), as the cause of swine fever in Great Britain, but it needs more careful study before it can be identified with the one we have described.

Rabbits, mice and guinea pigs are very susceptible to this organism. Cultures diluted to such an extent that but 15 to 20 bacteria are injected beneath the skin prove invariably fatal to rabbits in from six to ten days.

Pigeons, on the other hand, succumb to this disease only when large doses are given. A culture in beef infusion, containing one per cent. of peptone in which active multiplication of bacteria ceases in a few days, is fatal to adult pigeons, as a rule, when three-fourths cc. is injected subcutaneously over the pectoral muscle or superficially into the muscle itself. The intra-muscular injection is more rapidly fatal.

The pigeon, after such an injection, may be dead within twenty-four hours. The inoculated pectoral muscle is more or less discolored throughout its depth. There may be a regurgitation of food from the crop, as grains are found mixed with mucus in the mouth and œsophagus. The injected bacteria are present in small number in heart's blood, liver and spleen.

About one-half of the pigeons do not die so soon. The bird stands quietly in a corner of the coop, with feathers ruffled, wings slightly separated and tail feathers drooping. The discharges are usually abnormal liquid, at times mixed with considerable mucus. The bird usually dies within a week. The pectoral will then be found extensively necrosed, the surrounding still living tissue very hyperæmic. The injected bacteria are found, sometimes in considerable number, in the liver and heart's blood.

The rapidly fatal cases might be regarded as cases of simple intoxication or ptomain poisoning. There is, however, some bacterial multiplication. In the more chronic cases there is an undoubted infection, characterized by multiplication of bacteria in the internal organs. Pigeons are far more susceptible in winter than in summer. Conse-

Digitized by Google

quently in the heat of midsummer the control animals occasionally resist, and thus impair the value of the experiments.

The experiments were carried out as follows :—

Culture tubes, containing ten cc. of beef infusion with one per cent. peptone, were inoculated with hog-cholera bacteria and placed in the incubator at 34° to 36° C. After a certain quantity of days, varying from three to ten, the tubes were exposed to a temperature of 58° to 60° C. for about one hour. Inoculation of fresh tubes showed that the bacteria had been destroyed. This test was always resorted to to make sure that no living bacteria were injected.

From 1 to 1.5 cc. of this culture liquid was injected, with a hypodermic syringe, beneath the skin of one pectoral muscle. This injection was repeated once or twice. Some days after the last injection the bird was inoculated with living bacteria. About three-fourths cc. of a beef infusion-peptone culture was injected beneath the skin of the other pectoral or into the superficial layer of muscular fibres. The vaccinated pigeons remained alive and well, the control pigeons nearly all died.

These statements are best illustrated by the tabulated results of a few experiments.

The first one, made in January, 1886, is given below. The control bird and the one which had received a very small quantity of sterile culture liquid died within two days after the test inoculation, the rest were well more than a week later.

TABLE I.

1885-86.	Subcutaneous Injection of Sterilized Culture Liquid.					Culture Liquid containing Bacteria.	Remarks.
Pigeon.	Dec. 24th.	Jan. 21st.	Jan. 29th.	Feb. 6th.	Total.	Feb. 13th.	
10	.4 cc.	1.5 cc.	1.5 cc.	1.5 cc.	4.9 cc.	¾ cc.	Well Feb. 20.
11	...	1.5 cc.	1.5 cc.	1.5 cc.	4.5 cc.	"	" "
12	...	1.5 cc.	1.5 cc.	1.5 cc.	4.5 cc.	"	" "
13	...	1.5 cc.	...	1.5 cc.	3. cc.	"	" "
8	.8 cc.	...	...	...	.8 cc.	"	Died in 48 hours.
14	...	...	...	...	...	"	Died in 24 hours.

A second series of injections, made to confirm these rather remarkable results, was equally unequivocal in its answer.

TABLE II.

1886.	Subcutaneous Injection of Sterilized Culture Liquid.				Fresh Culture Liquid.	Remarks.
Pigeon.	Feb. 19th.	Feb. 24th.	March 2d.	Total.	March 8th.	
16	1 cc.	1 cc.	¾ cc.	2¾ cc.	¾ cc.	Well after several weeks.
17	1 cc.	1 cc.	1 cc.	3 cc.	"	" "
18	1 cc.	1 cc.	1 cc.	3 cc.	"	" "
19	1 cc.	...	1 cc.	2 cc.	"	" "
20	1 cc.	...	1 cc.	2 cc.	"	" "
21	1 cc.	...	1 cc.	2 cc.	"	" "
22	...	...	...	...	"	Died March 9th.
23	...	...	...	...	"	" "
24	...	...	...	...	"	Remained well.

The third control bird (No. 24) was of a different race of pigeons. A good authority

Digitized by Google

at the time regarded it as having some of the characters of the carrier pigeon. Leaving this aside, the result is sufficiently convincing.

Since that time numerous other experiments have been made in practically the same way, to ascertain the effects of heat and extractives upon the ptomain. These experiments are not completed, nor have they any direct bearing upon the demonstration of the subject of this paper. They have, however, confirmed it, for experiments like the above have been repeated in this work as control experiments with practically uniform results, if we keep in mind the fact that pigeons are less susceptible to this virus in summer than in winter.

In a series of experiments made last spring, cultures were used which had been concentrated by evaporation in vacuo. About 100 cc. of beef infusion, containing 2% peptone and ½% sodium chloride, was concentrated to 20 cc. by evaporation at 40° C., and sterilized at 60° C. for three-quarters of an hour. Subsequent inoculation of fresh tubes showed that the liquid was free from living bacteria. The injections were made as usual, the needle entering the pectoral muscle very superficially.

TABLE III.

1887.	Sterilized Concentrated Culture Liquid.			Fresh Culture Liquid.	Remarks.
Pigeon.	April 19th.	April 22d.	Equivalent of ordinary culture fluid.	April 25th.	
1	½ cc.	½ cc.	2½ cc.	¾ cc.	Well May 31st.
2	¼ cc.	¼ cc.	2½ cc.	"	Well May 31st.
3	¼ cc.	¼ cc.	2½ cc.	"	Slightly ill April 26th; well May 31st.
4	1 cc.	...	5 cc.	"	Well May 31st.
5	1 cc.	...	5 cc.	"	Well May 31st.
6	1 cc.	...	5 cc.	"	Well May 31st.
7	...	...	...	"	Died April 26th, 9 A.M.
8	...	...	...	"	Died April 26th, 1 P.M.
9	...	...	...	"	Died April 30th.

The protected pigeons very rarely show any signs of illness. They are as active and as eager for food as before the final inoculation. In all, a small sequestrum is formed in the pectoral muscle, which at the end of a few weeks is surrounded by a dense membrane which seems to act as an absorbing surface for the sequestrum.

The three tables may be summarized as follows: Of 24 pigeons, 16 received sterilized cultures, eight being reserved as checks. Of the former none succumbed to the final inoculation; of the latter seven, or 87.5%. The conclusion to be drawn from these experiments is obvious. The birds are protected by the injection of sterilized cultures so as to resist a fatal dose of living bacteria. The sterilized cultures contain only the products of the bacterial growth. Among these the ptomain-like bodies—some of which we now know, owing to the researches of Brieger—are very likely the agents that produce immunity.

In the pigeon the mode of infection before and after vaccination is probably as follows: The injected bacteria multiply very actively in the muscular tissue; the ptomains there produced may enter the circulation in quantities large enough to produce speedy death. If the animal resist for a time, the absorbed ptomains reduce the vitality of the tissues to such a degree that bacteria entering the circulation begin to multiply in the internal organs. The additional quantity of ptomains thus produced finally kills the bird.

Digitized by Google

When ptomains in culture liquid have been previously introduced, the first shock caused by the local production of ptomains in the muscular tissue is overcome. The bird resists successfully general infection until the bacteria have been destroyed locally, the process is then checked, and the sequestrum in the muscular tissue becomes encysted.

Experiments made upon rabbits to produce immunity, uniformly failed to attain the desired object. The amount of culture liquid introduced under the skin was, relative to the body weight, somewhat less than that given to pigeons. Experiments upon swine have also failed. In both the failure was no doubt due to an insufficient quantity of the ptomain. The striking success with pigeons must be attributed to the fact that they are very near the border line of susceptibility, so that very little assistance from without decides the issue.

Dr. I. W. Blackburn, Washington, D. C., read a paper on—

A METHOD OF PREPARING BRAINS AND OTHER ORGANS FOR ANATOMICAL AND PATHOLOGICAL DEMONSTRATION, AND TISSUES FOR MICROSCOPICAL STUDY.

MÉTHODE DE PRÉPARATION DU CERVEAU ET DES AUTRES ORGANES POUR LA DÉMONSTRATION ANATOMIQUE ET PATHOLOGIQUE, ET DES TISSUS POUR L'ÉTUDE MICROSCOPIQUE.

METHODE ZUR PRÄPARATION DES HIRNS UND SONSTIGER ORGANE FÜR ANATOMISCHE UND PATHOLOGISCHE DEMONSTRATION, UND VON GEWEBEN FÜR MIKROSCOPISCHES STUDIUM.

The principle of the method is, that by prolonged immersion of the object to be prepared in a material which is liquid when warm, and solid at ordinary temperatures, the tissue may be so permeated by the material used that no change in size or shape can occur when exposed to the air. The material used for this purpose is the so-called "Japan wax," which is the product of Rhus Succedanea, Ln., a tree of Japan. It comes in large rectangular blocks about one and a half inch thick; has a yellowish-white color, and a somewhat rancid smell and taste. It is saponifiable with the alkalies and is properly a concrete oil. Its melting point varies from 42° C. (107° F.) to 50° C. (131° F.), and at ordinary temperatures it is firm and solid. It is insoluble in water, scarcely so in alcohol, slightly so in boiling absolute alcohol, which deposits the greater portion of it on cooling, and to a limited degree in ether and in turpentine. It is very soluble in *chloroform*, *benzole*, and *xylol*.

The method of using this wax is as follows: The specimen or organ, in this case the brain, is stripped of its membranes and carefully hardened so as to preserve its shape and to avoid shrinkage. The best hardening fluids for this purpose are Müller's fluid (bichromate of potash, two parts; sulphate of soda, one part; water, 100 parts) or Erlicki's fluid (bichromate of potash, two parts; sulphate of copper, one-half part; water, 100 parts). Other hardening agents—such as alcohol, chloride of zinc, and nitric acid—shrink the tissues too much, though the color is, perhaps, more pleasing. The hardening may be hastened by injecting the fluid into the vessels before removing the membranes, and these may be removed just as well after three or four days' immersion in the fluid.

Digitized by Google

After hardening for about five weeks in Müller's fluid, or a shorter time in Erlicki's solution, the organ is removed, washed, and placed in dilute alcohol, and gradually advanced through alcohols increasing in strength until absolute alcohol is used. When thoroughly dehydrated by the use of absolute alcohol, the organ is placed in a saturated solution of Japan wax in chloroform, and allowed to remain until the alcohol is displaced by the chloroformic solution. The advantage of chloroform as a solvent for this bath is, that the wax is deposited in an almost solid form after the evaporation of this solvent, whereas benzole and xylol leave it in a granular state.

The organ is now transferred to a bath of melted wax, and kept therein, at the melting-point, until thoroughly infiltrated. The time required will vary according to the size of the specimen, but about three days are required to infiltrate a hemisphere. When the specimen is infiltrated it is removed from the bath, the wax drains from the surface, leaving it smooth, and the organ is allowed to cool; it may then be varnished if desired, and, upon the varnish, it may be painted or lettered to suit the purpose of the operator. If the wax cannot be kept melted continuously during the process of infiltration, it is better to lift out the specimen and replace it in the chloroformic bath, as when cooled in large masses the wax has a tendency to crack, and if left in, the preparation might be injured. Cracking may be prevented by the addition of a small proportion of paraffine, with which the wax is perfectly miscible, and this will in no way interfere with the process.

The color of brains hardened in the chromic acid salts, and prepared by this method, is an olive green or bronze; those hardened in alcohol or chloride of zinc become slightly darkened in the wax. There is no odor to the specimens except that of the wax, which is not disagreeable.

By careful measurement I have found that no appreciable shrinkage occurs during the infiltration, and the shape is perfectly preserved. Whole organs, sections and dissections of any kind, and even entire animals, if small enough to be dehydrated, may be prepared for class demonstrations or for museums. The specimens are permanent in the air, and will stand more handling than wax models; if broken, they may be repaired by dipping the parts in the melted wax and placing them in proper position to cool.

The foregoing I believe to be the most satisfactory of all the methods of making *dry* preparations of the brain and other organs; but the process may be shortened and a very fair degree of success attained by dehydrating the specimen as far as possible in 95 per cent. alcohol and placing at once in a bath of melted Japan wax. It is necessary, however, to heat the wax to near the boiling-point of alcohol to secure permeation, and this is likely to cause shrinkage.

THE USE OF JAPAN WAX IN MICROSCOPY.

In an article in the *New York Medical Record*, April, 1885, Dr. Maurice N. Miller, of New York, called attention to a new imbedding material, which he called "bayberry tallow," and though this is a common name for Myrtle Wax, the product of Myrica cerifera, an entirely different wax, I think it probable that Japan wax was the one used.

In the article mentioned, Dr. Miller recommended the immediate transfer of the specimens from the alcohol used to preserve to the bath of melted wax, but as Japan wax is sparingly soluble in common alcohol unless heated almost to the boiling-point, a preliminary preparation, such as recommended for macroscopical objects, is to be preferred. The process used for interstitial imbedding is essentially the same as that for macroscopical preparations, but, of course, less time is required for the various steps. The pieces of tissue, when infiltrated, are fastened on cork by using the melted wax, or imbedded in blocks of wax or paraffine. They are then cut dry, into benzole

Digitized by Google

or chloroform, to free the sections from wax, washed in alcohol, to remove the solvent, and stained and mounted as usual. The sections may be cut as thin as by any other interstitial imbedding method, and the most delicate tissues are uninjured by it. The blocks may be preserved in the dry state, as in the paraffin method, but if kept for some time the tissue becomes slightly harder and more brittle; it is, therefore, better to cut the specimens soon after imbedding.

Dr. CHAS. WARRINGTON EARLE read a paper entitled—

PANCREATIC ANÆMIA, OR CIRRHOSIS OF THE PANCREAS.

ANÉMIE PANCRÉATIQUE, OU CIRRHOSE DU PANCRÉAS.

PANKREAS-ANÄMIE, ODER CIRRHOSE DES PANKREAS.

I desire to call your attention to a disease characterized by great loss of flesh, extreme whiteness of tissues, and, generally, pain at the ensiform cartilage. With these symptoms the appetite sometimes remains perfectly good, and the patient may take food up to the hour of death. The pulse and temperature give no evidence of the degree to which the system is becoming impoverished. An autopsy will, in a typical case, reveal every organ normal *except* the pancreas. This will be found hard and white, with its connective tissue increased and its glandular and secreting structure greatly obliterated. My conclusions are based on observations in five fatal cases, and one which I believed to be a partial disease of the organs, which passed from under my care before recovery or death.

I will give at this time the history of only one case, with the pathology of all and the microscopical appearances as far as possible.

CASE I.—In November, 1880, I was called to see Mr. Elwood Ewing, an American by birth, sixty-five years of age, a contractor and builder by profession. He had always been strictly temperate, had never had syphilis, and he came from an ancestry who were remarkable for longevity. Up to within a few months he had never been sick.

During the latter part of 1880, however, he commenced to emaciate, and, applying to a neighboring physician, he was given a prescription for some malarial trouble. During these weeks he had not cared to eat meat, but preferred milk, mush and oatmeal. He became very weak, very greatly emaciated, and, at my first visit, I noticed particularly his white, anæmic look. I gave him a very thorough examination, extending it through two or three visits.

There was nothing abnormal as regards his nervous system. His intellect was perfectly clear, and the only symptom referable to his head was a constant and continual roaring or noise within the brain. His lungs were without a trace of disease, but he complained of being short of breath on account of his great weakness. An anæmic murmur was heard with the first sound of his heart. The only point upon his entire body where pain or soreness was experienced upon pressure was at the epigastrium. He complained of thirst, and was occasionally sick at the stomach. There was no enlargement of any organ, and his urine was perfectly free from abnormal deposits. His legs were slightly swollen. His stools were black and thin, and occasionally contained small lumps of blood and mucus. I was not able to learn that there were fatty particles passed with the evacuations from the bowels. His pulse was 100, and he had no

Digitized by Google

fever. The most marked symptom was his extreme white color. His tongue was white, not coated, but anæmic, and his lips were without a particle of color. There was not the faintest tinge of yellow in the conjunctiva. I was unable to diagnosticate the case, but believed he was suffering from some disease of nutrition—the organ affected, however, I could not determine. I treated him for a few days with tonics, mineral acids, strychnia and a generous diet. There was no improvement, however, and Dr. N. S. Davis was called to see the case with me in consultation. There was no difference of opinion, and neither of us attempted to give a name to the disease. It appeared to us both that in some part of the digestive apparatus there was a very serious disease, which, in all probability, would cause death. He died one month after I made my first visit, having, however, passed into the hands of another professional gentleman. In company with this gentleman, a homœopathic physician, I made the post-mortem. There was nothing unusual to be seen in any of the principal organs of his body, excepting the very marked whiteness of all the tissues, and not until I had removed nearly the entire viscera within the abdomen, and come upon a white, indurated pancreas did I find the seat of the disease.

The gross and microscopical appearance of this organ are given by Dr. S. J. Holmes, Lecturer on Pathological Histology in Rush Medical College: "Pancreas contracted; consistency abnormal; fibrous to the touch; color white. Under the microscope the connective tissue is extensively augmented, with obliteration of certain glandular vesicles, and atrophy of others, induced by pressure on the newly-formed connective tissue, as it contracted at its maturity. There was a slight fatty degeneration of the parenchyma of the kidney."

CASE II.—Mr. S. K. R. had always been a healthy and powerful man. Sick about two years. Characterized by great pallor, loss of flesh and strength. Autopsy. In compliance with the wish of his friends, the abdomen only was opened. There was considerable fat between the integument and the abdominal muscles, but nothing abnormal as regarded position of any of the viscera. The whiteness of the tissues, so marked at the autopsy of Mr. Ewing, were also present in this case. With this exception, nothing pathological was found until the pancreas was reached. This organ was hard, evidently enlarged, and filled with white spots, plainly seen with the eye. The colon was, perhaps, somewhat contracted in its calibre in some places; but in the rectum, where so much pain was experienced up to a short time before his death, there was absolutely no trace of disease. There was no abrasion in the continuity of the mucous membrane of the stomach, and the hæmatemesis, which was so profuse a few days before his death, was caused by pressure of the enlarged head of the pancreas on some blood vessel, or it may have been from minute capillaries upon the mucous surface of the stomach.

The microscopical examination of this and the following three cases was made by Prof. Marie J. Mergler, Woman's Medical College, and resulted as follows: "The pancreas measured nine inches in length; its breadth was two and a half inches at the head and two inches at the lesser extremity, and it was one inch in thickness. The whole organ was denser than normal, and its surface presented numerous yellowish-white spots, from one-quarter to one-half inch in diameter. These light portions were considerably larger than the rest of the organ. On microscopic examination, an increased amount of connective tissue was found throughout the organ, especially in the lighter-colored portions."

CASE III.—This patient I saw through the kindness of Dr. Jos. Haven. Mrs. R., aged thirty-six; pregnant; died from exhaustion. Symptoms: great reduction of flesh; pale; pain at pit of stomach. Autopsy. Uterus enlarged to the extent of three months' pregnancy. All the other organs apparently normal, with the exception of the pan-

Digitized by Google

creas, which was hard and contracted, a portion of which was removed for microscopical examination.

In the history of this case, from beginning to end, there has been a lack of symptoms other than a general failing of the powers of life. There has been no pain or other symptoms to locate the trouble in any particular organ. An analysis of urine failed to establish renal complications. It is to be regretted that the character of the fecal dejections could not be ascertained as to the presence of fat. There is no family history of scirrhus, constitutional or wasting disease.

The microscopical changes are described as follows: "On microscopical examination the entire organ is found more or less altered. In some portions, the connective tissue between the acini is increased and many of the gland cells have undergone fatty degeneration; in others the broad bands of connective tissue encroach considerably upon the glands, and in place of their large polyhedral cells are found small, irregular, shriveled bodies, evidently atrophied gland cells; and still other sections do not even present a trace of gland structure; they consist entirely of connective tissue."

CASE IV.—Mrs. ———, twenty-one years of age. A history of emaciation and pallor. Appetite extraordinary. At the autopsy not an organ of the body was sufficiently impaired to cause any disease, except the pancreas, which was hard and white, and the microscopic examination of which gave us the exact results which we found in the preceding three cases—increased connective tissue and obliteration of the glandular apparatus.

CASE V.—Mr. D. B. C., aged fifty-nine. Symptoms of dyspepsia; ordered to Europe; great emaciation and pallor. Advised to return to this country by his European physician. His friends now remember that he had pain at the end of ensiform cartilage for two years. Immediate cause of death, stenosis of coronary artery. A white and hard pancreas was found, which accounts for his pallor, emaciation and pain.

I have come to believe that we have not, up to this time, paid as much attention to disease of this organ as we should, and that a considerable number of cases which we have called pernicious anæmia, leucocythæmia, general debility, etc., may be due to chronic disease of the pancreas.

I have placed these cases on record, hoping to elicit inquiry and observation from other members of the profession.

We frequently see cases in practice where there is very great emaciation, white color of all of the integument, showing want of blood corpuscles; and it may be that these changes are due, at least in some cases, to disease of the pancreas.

"It has been regarded highly probable that the red corpuscles are produced by a gradual metamorphosis of corpuscular elements derived from the so-called cytogenic organs,"* such as the spleen, lymphatic glands, marrow of the bones, etc. To this list should, it appears to me, be added the pancreas.

Anæmia with enlarged spleen is splenic anæmia; anæmia with hyperplasia of great numbers of lymphatic glands is leucæmic lymphoma; and a malignant form of these last-named glands is malignant lymphoma, or Hodgkin's disease. To these should be added, if future observations demonstrate the correctness of my views, pancreatic anæmia, due to chronic inflammation of this organ, producing increased connective tissue and obliteration of gland cells, and which deserves as much attention as Hodgkin's, Addison's or Basedow's disease.

---

* Ziemssen, Vol. XVI, p. 335.

Digitized by Google

Dr. A. B. PALMER, Ann Arbor, Mich., read a paper entitled—

THE PATHOLOGY OF RAYNAUD'S DISEASE, OR LOCAL SYNCOPE OR ASPHYXIA, WITH REPORT OF AN UNUSUAL CASE.

LA PATHOLOGIE DE LA MALADIE DE RAYNAUD OU ASPHYXIE LOCALE, AVEC RAPPORT D'UN CAS EXTRAORDINAIRE.

DIE PATHOLOGIE DER RAYNAUD'SCHEN KRANKHEIT ODER LOKALEN SYNKOPE ODER ASPHYXIE, NEBST BERICHT EINES UNGEWÖHNLICHEN FALLES.

Different observers, during the last two hundred years, have noticed a class of affections involving limited portions of the external and superficial parts of the body, oftenest of the extremities, characterized by marked changes of circulation and nutrition in the affected localities. These changes often come on very suddenly, sometimes paroxysmally, resulting in some cases in waxy paleness, in others in darker discolorations, in others in eruptions, in others still, in necrosis. The latter effect is sometimes confined to the skin, but it commonly involves deeper tissues as well. These local changes are generally attended with marked sensational phenomena, some of them with severe pain. These attacks have been variable in persistence and in their ultimate results.

Though these phenomena have been known to practitioners for so long a time, the essential pathology of such cases has not, until recently, received much attention, and may still be considered as obscure.

As pathological laws are more or less general in their application to different parts of the body, or at least as there are analogies between different parts, external and internal, it seems not improbable that changes of a similar character to those mentioned may occur in different internal viscera—in the brain, heart, stomach, intestines, liver and kidneys, and from similar causes, but producing more serious effects—and this consideration gives additional interest to this subject. If suspended circulation should occur in a portion of the brain or in the heart, however temporarily, marked symptoms, or even fatal results, would be likely to follow; and it is not improbable that some cases of sudden death are due to causes of this kind.

The following unusual case, tending to throw light upon this subject, is thought to be worthy of being here presented in considerable detail.

In December, 1886, there was brought to the Medical Clinic of Michigan State University Hospital, from Caldwell, Ohio, by her physician, Dr. O. O. McKee, Miss L. B., aged forty, presenting this history. She was born of healthy American parents, in a country town in Ohio. She was one of several children, with no apparent morbid hereditary tendency, and nothing in the case of the patient herself to produce the least suspicion of specific disease. She was entirely well until about twenty-one years of age, when she was reported to have had some nervous affection, supposed to have been hysterical. From that time up to the age of twenty-six, fourteen years ago, she is reported as having some "female complaint," the special character of which could not be ascertained. Her appetite, however, was good and her nutrition fair.

At this time, her history states, she had an attack of "paralysis," motor and sensory, limited to the *right* side, which confined her to the bed for several weeks, but from which she recovered. Five years afterward—nine years ago—she had another similar hemiplegic attack of the same right side, from which she again recovered, at least sufficiently to enable her to perform ordinary feminine labor up to October, 1886.

For the last fifteen years, commencing one year before the first hemiplegic attack, she has been addicted to the use of morphine, but to what extent has not been reliably

Digitized by Google

ascertained. Her bowels were during all this period often constipated, and the urine at times was particularly scanty.

Her present physician was first called to her in April, 1885, and the history of the case is chiefly taken from his letters. She was then suffering from what he thought an acute attack of periostitis of the index finger of the *right* hand. He did not again see her for a week or more, when he found "several sinuses" opening on the surface of the finger. The finger was so much affected that he advised amputation, and after the operation he found the "second and third phalanges necrosed." The stump healed rapidly, but the "flexor tendon sloughed and all came away up to the annular ligament."

Soon after this the most peculiar feature of the case appeared. An effusive eruption was seen on the *right* forearm, the vesicles not larger than a grain of wheat, but the parts involved became greatly swollen and were soon "covered with a great many sores." It is presumed these sores, at that time, were produced by the coalescence of the vesicles and abrasion of the surface. The swollen and morbid condition of the arm and hand continuing, disease of the metacarpal bone of the amputated finger was suspected; but upon an incision being made the bone was found healthy, and the wound healed without difficulty. The swelling of the arm subsided, but "sores" appeared on other parts of the *right* side, but never on the left. One appeared upon the neck and one upon the face, an inch and a half in diameter, and several on the right arm, the size of a silver quarter of a dollar. Then one, two inches in diameter, appeared on the right side of the chest, another on the right leg below the knee, and more recently, since the patient was at the clinic, one came upon the limb above the knee, for the first time.

From the time of the appearance of these "sores," in April, 1885, up to October, 1886, though sometimes with as many as twenty-five on her at once, she continued at her occupation, and did not cease to labor until they appeared on her right foot, and were accompanied with so much swelling and pain as to prevent walking about.

In the letters from Dr. McKee, the following account of the manner of coming on of these sores and the progress of the case is taken :—

"On seeing the patient on February 2d, 1887, she said 'a sore would appear at a certain spot, pointed out, to-morrow.' She knew so because the part felt as though a thousand little ants crawled over the place. On February 4th, as predicted, a sore was present, two and a half inches in diameter. The skin was of a dark brown, almost a black color, but so thin and nearly transparent that the subcutaneous vessels could be seen beneath it. In these sores, of which the one mentioned is a type, the surface of the affected part sinks below the surrounding skin ; a distinct line of demarcation soon appears and a process of suppuration begins at once; the dark filmy surface dissolves away and disappears with but very little suppuration. The process of restoration is hastened by removing, with a scalpel and forceps, the necrosed skin and some of the subcutaneous connective tissue and vessels, when the superficial cavity fills up readily and scars over. Within two weeks before this account was written, about ten sores appeared on the arm and leg and went through this process."

This patient appeared but once at the clinic, and as a positive promise of cure was not made, she returned to her home without entering the hospital, and therefore an opportunity of more fully investigating and watching the case was not afforded me; but the kindness of Dr. McKee has enabled me to add to my own observation this account.

The observation at the clinic showed a woman considerably, but not extremely, emaciated, rather tall, and of a nervous temperament. Her features expressed suffering, though modified by the use of morphine, but her expression and replies to questions indicated intelligence and sincerity. The diseased spots were numerous and in

Digitized by Google

different stages, all confined to the right side of the body, more numerous on the arm than other parts, but several were on the legs. She did not speak of any as about to occur, so that the appearance preceding the necrosis of the skin, whether blanched or congested, was not observed. Some of the spots were covered with patches of dead skin, dark in color, all with a line of demarcation separating them from the living cutis, the breadth of the red line depending on the age or advanced stage of the local change. From some the dead skin had disappeared, and a bright-red granulating surface was left exposed. Others were advanced to the stage of cicatrization, a thin, smooth cuticle forming over the granulations, while others were further advanced in the healing process. The necrosis in no place was deeper than the cutis vera, and in all the reparative process appeared to be actively taking place. The spots varied in size either way from an inch in diameter, and were not far from circular in form.

It was evident that the necrosis speedily occurred, and that as speedily the process of separation and repair commenced. The cause of the morbid change was evidently paroxysmal in its action, the paroxysms occurring at irregular intervals, and continuing but a short time as affecting each place. The arrest of vitality in the skin was very complete, as the appearances of necrosis of that tissue so speedily occurred.

I advised some eliminatives as correctives of the secretions, and of the stomach and bowels, and the use of as free doses of strychnine as could be borne, together with iron and quinine in tonic quantities.

On June 3d, 1887, Dr. McKee reported that after returning home in December, 1886, the sores continued to appear, three or four on the abdomen, for the first time in that situation, though a year previous, as before mentioned, a few appeared on the upper part of the thorax, and also upon the neck and face; but with these exceptions all have been upon the limbs, and without any exception *all have been upon the right side*, and never any upon a mucous surface.

The report continues: On February 12th the patient was put upon strychnine alone, which has been pushed to its full limits ever since. The sores continued to appear until March 10th. From that time to June 3d only two have occurred, and those upon the leg; one two inches in diameter, and the other small; though several times others were predicted from the usual sensations of crawling insects, but no necrosis appeared.

I have felt justified in describing this case at such length because of its exceeding rarity, if not its positive uniqueness; because of its relations to other forms of vasomotor and trophic diseases, and because of its throwing light upon the pathology of this subject.

The names which have been given to the class of cases mentioned at the beginning of this article, and of which the case related is a specimen, are, local asphyxia, local syncope, digiti mortui, and erythromelalgia; and because, in 1862, M. Raynaud described this class of cases more fully than had been done before, the name of "Raynaud's Disease" was given to it.

Previous to the case I have described, those of the class coming under my observation have been mostly obstructed circulation and necrosis of the fingers—*digiti mortui*—some in children, more in adults, and of the latter, mostly in women.

The nearest to the case here related which I have seen reported, is one by Dr. T. C. Fox, published in the New York "Clinical Society Transactions," and referred to by Dr. J. C. Shaw in the *New York Medical Journal* for December 18th, 1886.

In this case a woman, aged forty-one, mother of several children, after some exposure, was attacked at frequent intervals, with her fingers becoming "white, and dead, like wax," with excessive pain. Sometimes the fingers were slightly swollen and of various shades of lividity; and "occasionally the asphyxiated condition of one of the fingers will lead to the formation of blood blisters; the contents subsequently

Digitized by Google

become puriform, and on the rupture of the bullæ, an ulcerated surface is left which is very slow to heal."

Dr. B. W. Richardson, of London, in the *Asclepiad*, describes cases of what he calls "local syncope, or suspended life of local surfaces," occurring in paroxysms at considerable intervals; but in neither of his cases was the suspended life sufficiently complete or long continued to cause necrosis of the surfaces. Dr. Richardson also treats, in a subsequent article in the *Asclepiad*, of what he calls "chilblain circulation," as those having such an abnormal circulation are particularly liable to chilblains in cold weather. He details three cases of constitutionally feeble and nervous persons whose general circulation was deficient, but more particularly that of the extremities and surface, the hands and feet being habitually cold, with weakened vitality and reduced chemical action in the parts.

Dr. C. L. Dana, of New York, has given an interesting account of what he calls "vasomotor and trophic acro-neuroses," and has examined the history of a group of allied cases, referring to numerous authors, among them, of course, to Raynaud, but also to Nothnagel and various others in Europe, and to Drs. Allan McL. Hamilton, T. A. McBride, S. Weir Mitchell, Charles K. Mills, W. W. Johnson and others in America. None of all the cases reported presents a complete parallel to that which is related in this paper. The resemblance, however, is sufficient to bring them all into one general class.

The phenomena of these cases point to the nervous system for an explanation of the pathological condition. There seems to be a general agreement that these affections have their origin in this system, involving especially the vasomotor and trophic centres and nerves. The case which I have related, from the affection appearing only upon the right side, which had been previously affected with paralysis, probably from some cerebral change, would seem to indicate that the cause was in some centre within the *brain*, rather than in any ganglia or nerves without it. The paroxysmal character of the cutaneous asphyxia and necrosis, preceded by the peculiar sensations and followed by such rapid repair, suggests an analogy to epileptiform seizures and the allied nervous affections, that have their acknowledged seat in the brain.

That the vasomotor and trophic functions are particularly involved is evident; but whether through the influence of excitor or inhibitory actions, whether from paralysis of the excitor or stimulation of the inhibitory, is not so clear.

That, however, there is *depression* of the vasomotor system as an important factor in causation, seems indicated by the fact that strychnia, which is regarded as a special tonic to this system, is shown by experience to produce improvement, at least in the several cases which have come under my observation.

The whole subject of paroxysmal and nervous affections, including those producing trophic changes, is involved in obscurities which await the investigations of the future. Any new facts, however apparently unimportant in their individual character, but which may in any way throw light upon this subject or attract attention to it, are worthy of notice and record among pathologists.

Digitized by Google

Dr. L. SERVAIS, of Antwerp, presented a paper entitled—

PATHOLOGICAL ANATOMY OF THE SUPERIOR MAXILLARIES IN A PRACTICAL VIEW.

L'ANATOMIE PATHOLOGIQUE DES MÂCHOIRES SUPÉRIEURES.

DIE PATHOLOGISCHE ANATOMIE DER OBERKIEFERBEINE.

The superior maxillary differs from the other bones of the skeleton by the presence in it of a pyramidal cavity whose base is turned toward the nasal fossa, and whose summit corresponds with the malar bone. (Heath.) This cavity, narrow in youth, is clothed by a diverticulum of the mucous membrane. This sinus occupies the body of the bone, and in a healthy state communicates with the nasal fossa by a narrow orifice. The presence of this cavity does not in any way modify the essential character of those complaints of which the superior maxillary is sometimes the seat. (Heath.) The affections to which this cavity is liable are simple and exempt from practical difficulties. (Garretson.) One may class the affections special to the superior maxillary under the following heads :—

Abscesses, Cysts, and Tumors of various kinds.

It is with these last that we are about to occupy ourselves. It might seem, at first sight, that the physiological fact of the implantation of teeth upon these bones should modify their pathology, compared with those of other bones of the skeleton, but it is rare that dental diseases, common as they are, affect the bones which support the teeth. It is probable that no genuine neoplastic tumor has ever owed its origin to a genuine dental complication. (Fergusson.) Prof. Fergusson observes that the extraction of the teeth does not appear to influence the progress of a tumor, as one might be tempted to suppose.

The pathology of the maxillaries is the same as that of the other bones of the skeleton, and the same rules ought to govern the treatment of the tumors of which they are the seat. (Fergusson.)

One is no more justified in sacrificing the whole of a maxillary because it is the seat of a tumor which only occupies part of it, than one is justified in taking off the whole of an arm on account of an affection of the hand or even of the scapula. There is always time enough for such extreme measures, but yet the surgeon should not hesitate to cut a cancerous tumor from the superior maxillary any more than from another part of the body, in the hope, if not of curing, at least of warding off functional inconveniences, and above all, of assuaging. This last motive will be the most frequent guide. (Heath.)

By the word tumor, from *tumes*, I swell, we understand every increase in volume, whether that increase be in connection with the normal anatomy and physiology of the region where the swelling appears, or whether it be the result of a pathological cause, and so reveals the direct and constitutional origin.

The surgeon studies the means of controlling the tumors which present themselves to his investigation, while the physiologist studies their essential character and their composition.

From a clinical point of view, we may range tumors in two great classes—those whose explanation is to be found in the parts which support them, or in the functions proper to them, such as a sebaceous tumor, a ranula, a hernia.

The second class comprises all the others; that is to say, those whose presence is not explained by the anatomy of the parts which support them, or by the functions which

Digitized by Google

are their seat, such as nodules of syphilis, a tubercle, or the numerous expressions of cancer. (Garretson.)

All the tumors of the first class are mild; all the others indicate a dyscrasia.

Tumors of the first class, or those whose existence is explained by the place where they are met, only require a local treatment.

The others require a treatment directed against the vice to which they owe their appearance, accompanied by a local treatment in order to avoid the inconvenience due to their presence.

Tumors that have no history are cancers.

So far it has been impossible to decide exactly what is a cancer, but it is at least necessary for the modern surgeon to know what is *not* a cancer. If he does not, then woe to the patient who is under his guidance, and who confides his existence to him.

We offer some clinical considerations calculated to aid the surgeon in forming his diagnosis:—

1. Homologous tumors tend to isolate themselves and to preserve their special characters, while the malignant tumors invade the neighboring tissues or betray a tendency in that direction.

2. Homologous tumors increase regularly, and when they have reached a certain volume, either rest stationary or begin to suppurate; their treatment is purely local. Heterologous tumors behave in quite a different manner; they have a pronounced tendency to reappear. There are two means of resisting them:

(*a*) By combating the vice whose existence they reveal;

(*b*) By rendering the constitution unsympathetic to their invasion.

3. Homologous tumors are generally solitary; or, if they are multiple, they all appear in the same tissue; heterologous tumors, on the contrary, appear in different parts of the economy, and in no wise respect the limits of the tissues.

4. Homologous tumors cause pain in the region which supports them, while malignant tumors have a special and characteristic way of expressing pain.

Tumors of the superior maxillary take, as a rule, their birth in the subperiosteal connective tissue of the antrum of Highmore; they often invade and at times efface it; nothing, however, can give an idea of the persistence which this cavity shows in resisting every means of destruction. (Garretson.)

The causes of these tumors are unknown.

They may also take their rise in the alveolar periosteum, rarely in the bony tissue itself.

From the histological point of view one may divide tumors of the upper maxillary in the following manner:—

Carcinoma.	Sarcoma.	Osteoma.
Cystoma.	Fibroma.	Enchondroma.
Gelatinoid polyps.	Melanotic sarcoma and Carcinomata.	Angeioma.

These varied demonstrations, which owe their origin to the resemblance of those tumors with bodies of which these denominations recall the external features, have, practically, only a very secondary importance, for each one of them represents both a mild and malignant tumor. The clinic alone, in the present state of our knowledge, is able to enlighten us as to their real nature, and it will find the elements of a good diagnosis in the state, the progress and tendencies of the tumor, and the general manner in which it conducts itself.

As to their frequency: among 307 cases analyzed by O. Weber, there were found—

133 Carcinoma.	34 Sarcoma.
32 Osteoma.	20 Cystoma.
17 Fibroma.	7 Enchondroma.
7 Gelatinoid polyps.	5 Melanotic Sarcoma and Carcinomata.
1 Angeioma.	

Digitized by Google

This author observes that the sarcoma has often been confounded with the carcinomata, the first of these tumors being by far the most frequent. (Gross.)

APPLICATION OF GENERAL ANATOMO-PATHOLOGY TO TWO LARGE TUMORS OF THE FACE. TREATED SUCCESSFULLY BY REMOVAL.

The patient, G. V. C., aged nine years, and born and living at Wouwo, Holland, was of good constitution, but he had hemorrhage, which would soon have produced a catastrophe. The tumor which afflicted the little patient's face had equally invaded the two superior maxillary bones. This gave the patient a strange and repulsive appearance; one would have said, at first sight, that it was the highly-enlarged tongue which hung out of the mouth.

The tumor, when more closely examined, and when the patient let fall the inferior maxillary, had a lengthened form and filled the whole of the mouth, under the shape of a full, rough surfaced pear; its color was a bluish-red, and its surface was overrun in every direction by a network of thick vessels gorged with arteriosa-venous blood.

One might have confounded it with an erectile tumor; its length was nineteen centimetres and its circumference twenty-three centimetres.

The tumor, which had developed principally at the expense of the right superior maxillary, and which had thrown the other forward, while partially invading it and exhausting it by compression, had also acted upon the inferior maxillary—all the teeth were horizontally crushed forward and outward.

The right nasal cavity was invaded; that of the left effaced. On the right side the nasal process of the maxillary bone, the orbitary border and the malar bone were strongly upraised and thrown forward, the right turbinated palate bone was invaded, the greater part of the ethmoid bone and the os planum were also invaded.

The tumor was ulcerated in two points in the front; these ulcers were due to external violence; they supported rudimentary teeth which had been driven forward by the tumor; this part had the appearance of the groin of a well-known animal; the veil of the palate was upraised and driven against the roof of the pharynx. The functions of swallowing and of sound were preserved, and respiration took place by the mouth. Hearing was difficult. It is worthy of remark that the general state of the patient's health had not been influenced by this enormous tumor. It produced no suffering, but the weight was so great as to drag the head forward and to the right. The mucous membranes which covered it had remained healthy, except those which covered the extra-buccal portion and which were unprotected. There was a complete absence of swollen glands. The development had been very slow, for it was six years since it first appeared, under the form of a little motionless ball, in the left canine fossa. Its appearance was smooth and its substance suggested that of India-rubber; the exterior part of the mouth somewhat resembles a thick leaf of cauliflower.

Clinically, I attributed it to cancer, for nothing justified the apparition of this tumor; I hoped, however, to obtain a good result from a radical operation, both because the development had been very slow, and also because there had been hardly any tendency to invasion; it had, if I may use the expression, preserved a life of its own, and appeared as though grafted upon the being it afflicted.

CASE II.—This was the case of a young woman, A. C., from Bleharies Hainhault, near St. Armand, who had been some years isolated. Her disease began eleven years before, by the appearance of a hard and reddish lump in the canine fossa on the right side; this tumor, which was then the size of a pea, and hard and motionless, was, from its first appearance, the seat of intolerable suffering, comparable to a violent attack of dental neuralgia; it was on this account that the first practitioners that she consulted began by extracting all the corresponding teeth; the result was a continuation of the pain, and beyond that a more rapid development of the tumor, which, of a very vascu-

Digitized by Google

lar nature, was attacked successively by several surgeons; the frightful hemorrhage which resulted from these efforts obliged them on each occasion to abandon their task.

The caustics which were then recommended had no better success; after ten years of suffering the tumor had acquired a colossal development (the accompanying photographs only give a feeble idea of it), and altogether this tumor was larger than the head of an adult.

It bled frequently, and each hemorrhage helped to extenuate the patient. Perchloride of iron alone was able to momentarily arrest these losses of blood.

It was under these conditions that I saw the patient for the first time, and that I resolved to undertake the operation.

The antecedents of the family offered nothing noticeable; her parents were living and enjoyed good health, her brothers and sisters were robust.

Notwithstanding the loss of blood that the patient had undergone on so many occasions, her general state appeared good.

The effect produced by the appearance of this person was most painful; one would have said a human form surmounted by the head of a hippopotamus.

The right side of the upper maxillary region was considerably developed, and surpassed and masked the whole of the ear; the orbitary border had been upraised from beneath it until it had become horizontal; the nose was drawn outward and to the left; the left wing of this facial appendix corresponded with a line lowered perpendicularly from the middle of the lower left eyelid toward the opening of the mouth on that side; the bony apparatus of the face had altogether participated in the change of direction which this enormous development of the tumor impressed upon it; the very bones of the nose deviated, and the base of the nose thus spread out measured seven inches. The left nostril preserved a small opening sufficient to admit a soft and very thin probe.

The enlarged opening of the mouth measured 22 inches; from this opening proceeded an embossed tumor, which spread behind under the form of an oblique barrier, from left to right, and embraced all the solid parts of the palate.

The part outside the mouth was bi-lobed; each lobe was of the size of the head of a full-grown fœtus; a vertical groove two inches deep separated these two lobes at their extremities. This groove was due to the primary action of the inferior maxillary upon the tumor before it had grown out of the mouth cavity. This extra-buccal part was hard, embossed, smooth, and bluish, like a cartilage, and exhibited on its surface a network of blood vessels. It was the seat of erosions, and of ulceration also, for it bled at the least touch.

The intra-buccal part appeared to have developed particularly at the expense of the antrum; it had taken, under these circumstances, the proportions of the head of a full-grown fœtus; its lower roof had descended; the skin that covered its outer front was extremely extenuated; that which corresponded to the lips was tumefied, and overrun by arterial vessels as strong as the radials of an adult; thick, tortuous vessels agreed with them.

The blood was very thin, so that a pin prick in this region would have sufficed to bring about death by hemorrhage.

On the left side the tumor had invaded the nasal fossa, and had produced, by condensation, a compact bone, comprising the interior table of the superior maxillary, corresponding to the wall of the other, the flat bone and the ethmoid; the left nasal fossa was represented by a very minute canal.

The front wall of the tumor was formed by a thick, bony slab of half a centimetre at its upper part; starting from the left orbitary border, it englobed the nasal processes of both the superior maxillary bones, all the front face of the right maxillary, and part of the front face of the left, spread out in the form of a fan, and became thinner

Digitized by Google

in proportion to their distance from their point of origin, losing themselves at its extremity in a bony pellicle, which covered, or rather closed, the front wall of the solid tumor, this pellicle became confounded on the hind side with a harder bone forming the zygomatic surface of the superior maxillary; but that zygomatic process had descended still lower than the angle of the jaw, such as it would exist in a healthy subject.

Above and outside, the bony pellicle became mixed with the very much spread out and enlarged malar bone; below and behind it met an osseous blade which, being produced by the descent of the palate process, of the superior maxillaries, and of the incisive bone, formed an enormous outburst into the cavity of the mouth; in this manner it succeeded in furnishing a solid shell to this strange zöophyte.

The palate bones had done their share in the formation of this shell, but the tumor was clearly separated from the soft palate. The length of the tumor, measured from the middle of the orbitary border, was 15 inches.

Its limits were distinct, there being no trace of ganglion nor invasion of neighboring tissues.

The orbitary cavity had remained unaffected by the development of the evil, if we judged of it by the position of the eye and by the normal functioning of that organ.

It was not the seat of any pulsing or breathing. The function of swallowing was destroyed, but hearing was preserved.

Swallowing required, in its first stage, the employment of a narrow pipe, which only allowed of the use of liquid matter.

Here the question presented itself, What was the nature of this tumor?

It was composed of a bony shell, and of a fibrous tissue analogous to the construction of a wart; this fibrous tissue was overspread with knots of ossification.

It had taken eleven years to develop itself, without ever stopping in its evolution, or melting away by passing into a state of suppuration.

It had in no way affected the economy; there was nothing in it to justify its appearance; it was not a parasite, nor caries, nor a hypertrophy; neither was it an odontocle, nor a dropsy of the antrum.

It was not an accumulation of the plastic lymph; and it was not an abscess.

We had, therefore, before us one of those forms of cancer whose appearance, in the present state of our knowledge, is a mystery.

From the histological point of view, it was an osteo-fibroma.

Dr. C. F. Dight, Beirut, Syria, presented a paper on—

THE BILHARZIA HÆMATOBIUM.

Accompanying this brief paper on the Bilharzia Hæmatobium I send two mounted specimens of the ova and embryos of that parasite, which were passed, along with blood and mucous clots, in the urine of a young man—an Egyptian, aged nineteen years—who was under my treatment in the spring of 1884.

Both specimens are mounted in glycerine, and one of them, as you will notice, is stained with Beale's carmine staining-fluid. In both, the oval-shaped, granular-looking embryos are well shown, some of them inside their transparent envelope, others out of

Digitized by Google

it; it having been intentionally ruptured by pressure with the cover glass upon it in mounting. In some cases the embryo almost completely fills the envelope, in others it is retracted somewhat from the inner surface of the envelope, the space left between them being occupied by a transparent fluid.

The Bilharzia Hæmatobium, or Dystoma Hæmatobium as it is called, is now well known to be the cause of the endemic hæmaturia which occurs in certain parts of the Cape of Good Hope, in Brazil, in Egypt and in Mauritius. This parasite was discovered in 1851 by Bilharz, a professor of anatomy in the Egyptian Medical School, who, with Griesinger, found the worm in the portal, mesenteric and vesical veins. These observers found the parasite in 117 cases of autopsy out of 363, and they believed that probably more than half of the native inhabitants of Egypt suffered from it. Sonsino found it in 30 cases out of 54, and he believes that nearly all the natives of Egypt have it. A disease so common, and, as we will see, liable to be spread, is, therefore, worthy of careful study.

The Worm, which produces the ova and embryos—specimens of which I send you—is a white, soft-skinned, non-hermaphrodite trematode worm, measuring from three to four lines in length. The female is slightly longer than the male, and filiform. The male is thicker than the female, and has on its ventral surface a long canal, "gynæcophoric canal," into which the female is received during copulation. It inhabits the vesical, mesenteric and portal veins, and by its presence in their minute branches it gives rise to lesions of the mucous and other coats of the bladder, ureters, pelvis of kidneys and intestines, with a discharge of blood, mucus and ulcerative products from these positions, especially from the bladder, mingled with which are found large numbers of the ova and embryos, but never the worm itself, it is said, it being found only on post-mortem examination, in the circulatory system.

The Ova of the worm, which, in case of persons suffering from it, are found usually in the blood and mucus clots discharged from the bladder, are about $\frac{1}{170}$ of an inch long, half of that broad, quite oval in form, with a spike-shaped process at one extremity, as will be seem in the specimens. The envelope or vitelline membrane surrounding the ovum is transparent, and when examined with a magnifying power of 250 or 300 diameters—

The Embryo is seen within, having a distinct border of its own, and lying usually closely applied to the inner surface of the envelope, except at its two extremities. Often, however, there seems to be a considerable fluid everywhere between the surrounding envelope and embryo, separating them and floating the latter. The form of the embryo while at rest is also ovoid, having a bluntly pointed extremity which appears to be the head or mouth.

In some of the ova which appear to be in a more advanced stage of maturity, but not in size, the embryo may be seen to expand and retract itself in the fluid which surrounds it. The envelope may be ruptured and the embryo liberated by firm pressure of the cover glass upon it. This can be done while the eye watches the process from above; and it is interesting to see the egg broken and the living embryo emerging from the interior, and its cilia, which project from either side in rows which meet behind put into vigorous motion.

Occasionally, in fresh specimens of the discharged blood and mucus from the bladder, an empty envelope may be seen, and the escaped embryo racing across the field with almost incredible velocity. At other times the movements of the embryo consist of narrowing and stretching out the body to its full length and then retracting it to an oval or globular mass, or sometimes the head is retracted into the interior of the body.

The source of the Bilharzia Hæmatobium in Egypt is believed, by those who have given it most attention, to be infected and unfiltered water, especially that of the river

Digitized by Google

Nile. Sonsino adduces in support of this opinion the fact that Europeans and well-to-do natives never use the water of the Nile unfiltered, and they are exempt from the disease, while the lower classes use the water as it comes from the river, and they suffer accordingly. I am not aware, however, that any one has detected the parasite or its ova in the Nile water; but water infected with the discharges of patients suffering from the parasite may, as in case of the specific fevers, be a source of its communication to others. If this be true, it is, of course, a disease which, like cholera, might easily be introduced into places where it is now unknown, by immigration of persons affected, and Dr. Harley found that the ova hatch in water. In this light, it is as certainly a preventable disease as is typhoid fever, and a careful study of its cause and the means of its restriction and prevention seems eminently called for.

How the Bilharzia gains entrance into the circulation is still a mystery. Dr. Harley suggests that it may find its way through the urethra into the bladder and there colonize and produce the vesical symptoms. If this were true, it seems probable that a man infected with the parasite would infect his wife; but this did not occur in the case of a man who passed from his bladder numbers of the ova every day of his married life, and who had four healthy children. It seems to indicate, further, that the disease is not communicable from one person to another. Dr. Harley thinks, also, that it sometimes gains admission through the skin, by the minute animal attaching itself to the skin of a person while bathing or wading, and implanting its ova in some superficial vein. In this latter case the parasite would certainly enter the veins of the general circulation and should be found there; but it is said to be found only in the portal circulation, which would seem to indicate that it is taken into the stomach, and from there or the intestines enters the portal radicals distributed in their walls. But the worm, its ova or embryo, as we know them, are all too large, it would seem, to pass through capillaries, or even through the smallest arteries and veins, and to enter the circulation in that way—unless there be a lesion of the walls of the canal with rupture of the vessels or other change in them, which renders entrance of the parasite easy; and this seems a probable means of their entrance, especially since in these warm countries, where the parasite abounds, dysentery and other diseases of the alimentary canal prevail. Once in the portal circulation, we can understand why they should be mainly confined in that system of vessels, as they are too large to pass through the hepatic capillaries. Through the middle and inferior hæmorrhoidal veins, by way of the internal iliac, they could, however, reach the general circulation, and it seems probable that when they exist (alone, as it is said) in the portal system, they may by careful search be found in the general circulation also, and I have quite recently been informed that in an autopsy in Egypt the ova were found in the lung tissue, and probably gave rise to hæmoptysis, from which the deceased had formerly suffered.

It is thought by some that the starting point, so to speak, of the Bilharzia in man, is neither the worm nor its ovum, but that the latter, after being discharged, undergoes, like the ova of the tapeworm, certain intermediate changes—possibly in the body of fresh water fish or mollusca as its host, before it can again infest man.

The Lesions caused by the Parasite are the results of its local irritation upon the tissues in which it resides. They consist of congestion, extravasation, inflammation, exudation and sometimes ulceration, with their attendant symptoms. The most common seat of these lesions is in the wall of the urinary bladder—the ova and embryos of the parasite in time breaking through from the vesical veins which they inhabit, into the interior of the bladder or ureters, from which they may pass up into the pelvis of the kidney, exciting pyelitis, nephritis, or forming the nuclei of renal calculi. The hepatic tissue and intestinal walls are sometimes the seat of these lesions also.

The Symptoms produced, of necessity, vary in different cases, according to the position and extent of the lesions caused by the parasite, being vesical, intestinal (dysenteric),

Digitized by Google

hepatic, or renal, or a combination of two or more of these, as the case may be. The vesical symptoms are, however, by far the most common. Of these the earliest and most important are the passage of a small quantity of blood, varying from a few drops to a teaspoonful, just at the end of micturition, with a little pain in the loins or perineum, which continues for a short time afterward. Later on, riding horseback or unusual exercise, increases both the pain and the amount of blood passed. The frequency of micturition varies from a half a dozen to a dozen times a day, but the total quantity of urine passed and its specific gravity are normal. There is an excess of mucus, and in bad cases the urine is alkaline, with a precipitation of triple phosphates. A small amount of albumen exists, due to the presence of a little pus and the blood, and, as already stated, the ova and embryos of the Bilharzia are found mingled with the clots of blood and mucus, or sometimes floating free in the urine. There is usually more or less general anæmia and debility, with a feeling of fatigue after exercise.

Prognosis.—This disease rarely proves fatal in itself, but the debility induced, with the slow septic poisoning which may occur from the dead and dying animals, which are said sometimes to form plugs in the portal vessels, prepares the subject for intercurrent diseases which readily prove fatal. Sometimes the hæmaturia ceases, the parasite remaining, as is indicated, even years after, by the passage of calculi or other products containing the ova.

The Curative Treatment of the Disease—that of destroying the parasite—is most unsatisfactory. We absolutely know of no medicine upon which reliance can be placed. Turpentine, once thought by some to be almost a specific, has been used by myself and others in Beirut, both internally and by injections into the bladder in full doses and long continued, without destroying the worm. Oil of male fern, chloroform, carbolic acid, quassia, wormwood, iodide of potassium and other remedies have been used, either topically or by internal administration, with a like result, although by the proper use of these, especially the iodide of potassium, the expulsion from the bladder of the ova and products of the parasite may be facilitated. Inability to remove the cause of the disease should not, however, deter from alleviating, as far as is possible, the distressing symptoms.

The possibility of destroying the worm by the injection of large amounts of sulphureted hydrogen and carbon-dioxide into the rectum or bladder has been suggested to me by observing, only a few days ago, the effects of these gases upon the tapeworm and ascaris lumbricoides, as follows: I injected at once about three pints each of sulphureted hydrogen and carbon-dioxide into the rectum of a man in the St. John's Hospital, in Beirut, who was under treatment for phthisis. They were absorbed, and so rapidly eliminated by the lungs that the patient at once complained of smelling and tasting the sulphureted hydrogen, and after a few hours he passed a tapeworm and a number of the ascaris. This seems to indicate that the gases were the cause of the destruction and discharge of these worms, and it suggests a trial of this treatment for them, and for the bilharzia also, since these gases, by being injected into the rectum or bladder will be absorbed directly into the hemorrhoidal, vesical and mesenteric veins and meet the parasite there. An almost unlimited amount of these gases may in this way be brought to bear upon the bilharzia, probably without injuring the patient, since they enter at once the portal circulation, are carried to the right heart and to the lungs, where they are largely eliminated without passing on and entering to any great extent the arteries of the systemic circulation.

The Prophylactic Treatment is a matter of great importance to the community among whom the parasite is found, and it is suggested by our knowledge of the cause of the disease.

It consists of the prompt destruction by disinfection of the ova passed in the urine; of the adoption of measures which prevent contamination of drinking water or of food

Digitized by Google

with the urine or other discharges which contain the ova; of avoiding, if possible, the use of water already contaminated, or when it must be used. as in case of the Nile, it should be thoroughly filtered; of thoroughly cooking all fresh-water fish or mollusca of the district where the parasite exists, before they are eaten, since these animals are probably the host in which the parasite undergoes its supposed intermediate or transition stage of development. Finally, raw cabbage, lettuce or other vegetables which may entangle small mollusca containing parasites, should be carefully cleansed before being eaten.

Dr. JOHN NORTH, Keokuk, Iowa, read a paper on—

THE PATHOLOGICAL RELATION OF PTOMAINES AND LEUCOMAINES.

LES RELATIONS PATHOLOGIQUES DES PTOMAINES ET DES LUCOMAINES.

DIE PATHOLOGISCHEN BEZIEHUNGEN DER PTOMAÏNE UND LEUKOMAÏNE.

Ptomaines are artificial alkaloids or bases found in decomposing animal matter, supposed to be the result of the action of certain micro-organisms upon protoplasm. They are only found in dead animal matter. Leucomaines are artificial alkaloids or bases found in the living animal, supposed to be either of bacterial origin or the breaking up of animal matter under the influence of metabolism, either physiological or pathological.

It is of importance to the pathologist to know whether these artificial alkaloids are the cause of diseases in which they are found, or are generated as the result of the diseased process.

The discovery of microörganisms of definite character in the body in certain diseases is well recognized. There is no limited region of natural science which, at the present time, attracts more zealous investigation in so great numbers as the microscopic determinations of the conditions of life and propagation of these microörganisms. Chemists, histologists, physiologists, pathologists, normal and pathological anatomists, surgeons and hygienists contend in the race in this realm of investigation.

The microscope has, to a certain extent, overshadowed the test-tube in the investigations of disease. In certain diseases, the pathological microscopists have been able to discover organisms, and at once jump at the conclusion that the disease is a bacterial disease, forgetting that these organisms may act as ferments or putrefactive agents, and the chemical change produced be the cause of the disease. Microscopy has done its work well, and physiology and pathology are greatly indebted to it for their present high positions. Without simultaneous chemical investigations microscopy cannot make great advances.

The chemist finds a large number of nitrogenous bases, consisting of the natural alkaloids, artificial alkaloids, ptomaines, leucomaines, extractive matter, and the ordinary nitrogenous basic substance found in the human body. We are able to give the chemical composition of all of these that have been found in sufficient quantity to be analyzed, but we know nothing of their construction. Nitrogen may be present in organic compounds in three forms, viz.: ammonia, cyanogen, and nitric acid, or derivatives of these compounds.

It is in this class of compounds derived from ammonia (NH_3) that we find the ptomaines and leucomaines. All the nitrogenous compounds and bases in the animal

Digitized by Google

body are supposed to be ammonia derivatives. They are produced from ammonia by the displacement of the hydrogen of the ammonia and the substitution of some organic radical or residue in place of the hydrogen. We have two general divisions of these compounds, the *amines* and *amides*.

Amines are compounds derived from ammonia by the removal of a part or all of its hydrogen, and an alcoholic radical or hydrocarbon residue substituted for the hydrogen. Amines contain nitrogen, hydrogen, and carbon, but not oxygen, and resemble ammonia in their chemical properties. They are basic substances, combine with acids directly, and form salts without the elimination of water.

Amines derived from one molecule of ammonia (NH_3) are called *monamines*, those from two molecules *diamines*, those from three molecules of ammonia, *triamines*. When only one atom of the hydrogen has been displaced, they are called primary amines. When two atoms, secondary amines. When all three atoms of the hydrogen have been displaced, they are called tertiary amines. Amides are compounds derived from ammonia by a substitution of all or a portion of its hydrogen by an acid radical. Thus:—

AMMONIA.	ACETAMIDE.	DIACETAMIDE.	CARBAMIDE OR UREA.
$N\left\{\begin{matrix}H\\H\\H\end{matrix}\right.$	$N\left\{\begin{matrix}C_2H_3O\\H\\H\end{matrix}\right.$	$N\left\{\begin{matrix}C_2H_3O\\C_2H_3O\\H\end{matrix}\right.$	$N\left\{\begin{matrix}H\\H\\CO\end{matrix}\right.$ $N\left\{\begin{matrix}H\\H\end{matrix}\right.$

Amides also resemble ammonia in their chemical properties, yet to a less extent than amines, because the acid radicals have a tendency to neutralize the basic property of the ammonia. They contain nitrogen, hydrogen, carbon and oxygen.

Amines and amides may both be regarded as compounds derived from ammonical salts, by the loss of one or more molecules of water.

Thus, if one molecule of water (H_2O) is abstracted from carbolate of ammonia ($NH_4C_6H_5O$), we have aniline ($NH_2C_6H_5$), which is an amine.

$$NH_4C_6H_5O = \left\{\begin{matrix}C_6H_7N \text{ or}\\NH_2C_6H_5\end{matrix}\right\} + H_2O.$$

If two molecules of water are removed from carbonate of ammonia $(NH_4)_2CO_3$, we have urea, CH_4N_2O, remaining, which is an amide.

$$(NH_4)_2CO_3 = CH_4N_2O + 2H_2O.$$

Amines and amides may also be formed by the action of the chloride or iodide of an alcoholic or acid radical upon ammonia.

The terms vegetable or animal alkaloids have been given to the amines and amides found in vegetables and animals. When prepared in the laboratory they are called artificial alkaloids. When found in animal tissue undergoing decomposition they are called ptomaines or cadaveric alkaloids, the term leucomaines being applied to the artificial animal alkaloids formed during life. The attraction which the vegetable alkaloids have always possessed for the medical chemist is easily accounted for. Composing, as they do, so very small a portion of the plant in which they are found, and yet representing in many cases the whole virtue and activity of such plants in their action upon the animal body, it is very natural that their composition should have been very carefully studied, with a view to explain the changes by which they are produced in the plants, and, if possible, to imitate those changes in order to obtain the valuable remedies by artificial means. In this study, however, the chemist has to contend with difficulties of no insignificant character; for even in the determination of the ultimate composition of these alkaloids, their high molecular weights, and comparatively small proportion of hydrogen render the exact determination of this element a matter of great difficulty, so that even at the present time the composition of some of the less known alkaloids can hardly be said to be definitely established.

Digitized by Google

At the present time chemists are attempting to produce the vegetable alkaloids artificially. So far, they have not been successful except in a few cases. They have been able to produce some compounds with the same ultimate composition. We are not able at the present time to understand their molecular construction. When we find out the exact radicals composing these alkaloids, and learn nature's method of putting them together, then we can make all the alkaloids to order.

A number of the animal alkaloids have been prepared in the laboratory of the chemist. In 1828, Wöhler accomplished the synthetical formation of urea from cyanic acid and ammonia; for the first time urea, an animal alkaloid, which had previously been known only as a normal product of the process of life, was formed out of its inorganic elements. The cyanate of ammonia, a salt, differs only from urea in its molecular construction :—

Cyanate of ammonia, $NH_4\ CNO$.
Urea, ... $C\ H_4\ N_2O$.

We are well aware that the forces of nature are constantly at work in the human body. We have to do with forces as well as matter. Prof. Le Coutt says: "There are four planes of natural existence, which may be represented as rising one above another. These are: First. The plane of elementary existence. Second. The plane of chemical compounds or mineral kingdom. Third. The plane of vegetable existence, and, Fourth. The plane of animal existence." It is in the fourth or upper plane that we find our ptomaines and leucomaines, from some chemical change taking place in the tissues or proximate principles of the living body, either decomposing or rearranging the structure of the molecules. The force that operates on the first plane and changes it into the second is chemical. That from the second to the third is chemical force modified by vital force. That which operates on the third plane and converts it into the fourth plane is chemical force, also modified to a greater extent by vital force. That force which operates upon the fourth plane of material existence, that which produces the phenomena of life, is vital force influenced by chemical force. A great number of our eminent scientists contend that vital force is only transformed nascent affinity. That in all cases vital force is produced by decomposition of tissue or food.

Several forms of decomposition take place in the living animal body—metabolism, fermentation, putrefaction, and suppuration.

Metabolism is the transformation of matter under life forces. That which we call protoplasm, or the cell contents, is an albuminous substance in some form, tough and viscid before undergoing subsequent change. It coagulates under heat, or upon the death of the cell, as in *rigor mortis*. It swells up and becomes gelatinized by water, but is insoluble in water. It is from this cell contents or protoplasm that, by metabolism, are formed the normal nitrogenous compounds and bases of the animal body. What especially characterizes the nitrogenous substances is their instability of structure, that is, the readiness with which they may be broken up into new compounds, a large number of which are basic in properties and belong either to the amines or amides, being built upon the ammonia type.

When this natural transformation is interfered with in the living cell and some microbe comes in contact with it, then we have putrefaction taking place, and then, instead of the albuminoid contents being changed by metabolism into harmless and in most cases useful nitrogenous bases, they are converted into artificial bases or alkaloids, called ptomaines and leucomaines. Anything that interferes with the metabolism of the normal nitrogenous bodies or bases, may give rise to ptomaines and leucomaines.

The following amines and amides are found in the human body:—

Urea, CH_4N_2O. Its formula may be written, $\left.\begin{matrix}NH_2\\NH_2\end{matrix}\right\}CO$, in which one atom of hydrogen has been removed from two molecules of ammonia, and replaced by the biva-

Digitized by Google

lent radical carbo-oxide (CO). Urea is isomeric with the carbonate of ammonium (NH_3NHCO), and the cyanate of ammonium (NH_4CN). The source and properties of urea are too well known to require any further notice at present. In certain pathological conditions urea may undergo change and give rise to a more poisonous basis, which is the cause of the so-called uræmic poisoning.

Leucin ($C_6H_{13}NO_2$). The leucic acid radical ($C_6H_{11}O$), replacing two atoms of hydrogen in one molecule of ammonia hydrate (NH_4HO), $\left.\begin{matrix}NH_2\\C_6H_{11}O\end{matrix}\right\}O$. Leucin is interesting, physiologically, as being one of the antecedents of urea, and pathologically, from its presence in the urine in certain diseases of the liver.

Tyrosin ($C_9H_{11}NO_3$), in which two atoms of hydrogen in ammonia hydrate is replaced by the group composed of (C_9H_9O), as follows:—

$\left.\begin{matrix}NH_2\\C_9H_9O\end{matrix}\right\}O$. Tyrosin, when present in the urine, is always associated with leucin, although the latter may be present without tyrosin. It is found in small quantities in the spleen and pancreas, and is one of the products of the action of trypsin on albuminous matters.

Creatin ($C_4H_9N_3O_2 + HO$) is an antecedent of urea, and is found in the juice of the flesh. It is converted into creatinin, and, under some circumstances, is decomposed into urea and sarcosin.

Creatinin ($C_4H_7N_3O$) is derived from creatin by the removal of two molecules of water. It is a very powerful base.

Sarcosin ($C_3H_7NO_2$) is not found in the animal body in a normal condition. It is one of the substances obtained by the decomposition of creatin:—

$$\underset{\text{CREATIN.}}{C_4H_9N_3O_2,HO} = \underset{\text{UREA.}}{CH_4N_2O} + \underset{\text{SARCOSIN.}}{C_3H_7NO_2}.$$

In addition to these given above, we have other nitrogenous bases, among them:—

Glycocin ($C_2H_5NO_2$).
Neurin ($C_5H_{15}NO_2$).
Xanthin ($C_5H_4N_4O_2$).
Hypoxanthin ($C_5H_4N_4O$).
Allantoin ($C_4H_6N_4O_3$).
Caruin ($C_7H_8N_4O_3$).
Guanin ($C_5H_5N_5O$).
Uric acid ($C_5H_4N_4O_6$).
Hippuric acid ($C_9H_9NO_3$).
Taurin ($C_2H_7NSO_3$).
Cystin ($C_3H_7NSO_2$).
Lecithin ($C_{44}H_{90}NPO_9$).
Protagon ($C_{160}H_{300}N_5PO_{35}$).
Indol (C_8H_7N).
Indigo ($C_{16}H_{10}N_2O_2$).
Indican ($C_{52}H_{62}N_2O_{34}$).

A larger number have been discovered, but not in sufficient quantities to determine their composition. All those nitrogenous compounds are capable of undergoing chemical changes and forming ptomaines or leucomaines.

Fermentation is that form of decomposition produced, in non-nitrogenous material, by the action of some ferment which is a nitrogenous substance. The products of fermentation are usually free of offensive odors, and are not poisonous. The ferment grows and develops at the expense of nitrogenous matter in the air; if the material for fermentation consists of carbohydrates, or if the fermentation takes place in the animal body, then the growth of the ferment must take place at the expense of some nitrogenous matter in the body.

Fermentation cannot take place between two hydrocarbons or between two carbohy-

Digitized by Google

drates. A number of ferments are found in the human body, and it is possible, by the breaking up of these ferments, that we may have amines and amides formed of such a nature as to act as poisons.

True putrefaction takes place only in the presence of some microbe, and is the result of bacterial growth and development. The putrefactive agent or germ must come in contact with the nitrogenized material under the proper conditions. Among these conditions we have warmth, moisture and oxygen.

In addition to these artificial alkaloids, there has been discovered in both living and dead animal matter, non-crystallizable nitrogenous matter, called extractive matter.

Lengthened putrefaction destroys the ptomaines.

Panum was the first to isolate a putrefactive alkaloid, but Neucki was the first to investigate methodically the products of putrefactive decomposition. He was the first to isolate and make a quantitative analysis of a putrefactive alkaloid from gelatine, to which he got the formula ($C_8H_{11}N$). Gautier and Etard obtained the same base from putrid mackerel, together with another homologous base ($C_9H_{13}N$). Guareschi and Mosso analyzed a ptomaine from putrid fibrin, which gave the composition ($C_{10}H_{15}N$). Breiger, in his recent work on ptomaines, gives the result of his investigation of the ptomaines as follows:—

1. The ptomaines of gastric fibrin.
2. The ptomaines from the putrefaction of albuminoids.
3. The ptomaines from the putrefaction of mammalian flesh.
4. The ptomaines from the putrefaction of fish.
5. The ptomaines from the putrefaction of cheese.
6. The ptomaines from the putrefaction of gelatine.
7. The ptomaines from the putrefaction of yeast.

He also gives a full description of his chemical results and physiological experiments. The following are the names and chemical composition of the ptomaines examined by him:

(*a*) *Peptonized Fibrin.*—A toxic substance was obtained from this called *peptotoxine.* Its chemical formula was not discovered.

(*b*) *Albuminoids.*—The same substance was discovered during the putrefaction of albumen.

(*c*) *Putrid Mammalian Flesh* gave two ptomaines. Neuridine, ($C_5H_{14}O_2$) and neurine ($C_5H_{13}NO$), both of which are crystallizable and form salts.

(*d*) *Putrid Fish*, from which five ptomaines were obtained:—

1. Neuridine.
2. Ethylene diamine, $C_2H_4(NH_2)_2H_2O$.
3. Muscarine, $C_5H_{15}NO_3$.
4. Gadinine, $C_7H_{17}NO_2$.
5. Triethylamine $N(CH_3)_3$.

(*e*) *Putrid Cheese.*—This yields the following bases:—

1. Neuridine.
2. Trimethylamine.

(*f*) *Putrid Gelatine* gives three ptomaines.

1. Neuridine.
2. Dimethylamine, $(CH_3)_2HN$.
3. Isophenylethylamine, $C_8H_{11}N$, (but not well established.)

(*g*) *Putrid Fish* yields dimethylamine.

Prof. Vaughan, of the University of Michigan, has discovered a ptomaine in putrid milk, to which he has given the name of tyrotoxicon.

Ballard has found that flour which has been kept for some time in sacks contains traces of alkaloids, and the older the flour the greater the quantity of the alkaloids.

Digitized by Google

After extracting the flour with ether and evaporating the solution so obtained, the fatty residue is acid, and has an unpleasant penerating odor and an acrid taste. Alkaloids may be detected in the aqueous extract of the residue by means of the usual reagents. Flour from twelve to eighteen months old contains appreciable quantities of alkaloids, and the extract proved fatal in several hours to sparrows to which it was administered, while extracts from fresh flour had no such effects.

At present Dr. Brieger is engaged in an endeavor to ascertain what are the specific products of definite microörganisms, and settle the question whether the products depend upon the kind of nitrogenous matter undergoing decomposition, or upon the kind of microörganism causing the putrefaction.

The majority of these artificial alkaloids have been produced synthetically. Neuridine is abundantly distributed, being not only met with among the products of putrescence of mammalian flesh, fish, casein and gelatine, but also in the eggs and brain substance. Neurine was only met with in the products of putrefaction of mammalian flesh, and dimethylamine in putrid gelatine and yeast.

In almost all cases the alkaloids and extractive matter, the result of putrefaction, are poisonous in their effects. From the effect of the ptomaines and from eating of putrid meats and other nitrogenous matter, I have compiled the following symptoms:—

Brain.—Headache, throbbing of temples, vertigo, delirium, stupor and coma.

Face.—Livid, pale, flushed.

Eyes.—Red, suffused, lids swollen, pupils dilated.

Tongue.—Fiery red, heavily coated.

Throat.—Dry, burning sensation, extreme thirst.

Stomach.—Want of appetite, repeated eructations, nausea, retching, violent vomiting, both bilious and alimentary.

Bowels.—Tender, painful, griping, diarrhœa, violent colic, gastro-enteritis, very offensive stools, bloody stools, dark tawny liquid from bowels, stricture of rectum and colon, tenesmus, prolapse of rectum.

Respiration.—Alternate strangulation and paralysis of lungs.

Temperature.—Normal, 101° to 106° F., or below normal.

Pulse.—Weak and slow, thin, thready, 100 to 150.

Integument.—No heat of skin, skin hot and dry, profuse sweating, fiery red eruption all over the body, intolerable itching, rough skin, greenish or yellowish patches.

Kidneys.—Urine scanty, loaded with urates, no sugar nor albumen.

Muscles.—Convulsive tremors, all movements painful, cramps of thighs and legs, epileptiform convulsions, partial paralysis of extremities.

General Symptoms.—Cross, irritable, great languor, extreme weakness, typhoid condition, cold extremities, etc.

We find most of these symptoms in cases of poisoning by the narcotic alkaloids of hyoscyamus, conium, stramonium, etc.

Considerable has been said and written in favor of the auto-intoxication as the cause of disease, before the French Academy of Medicine, and in opposition to the bacterial origin of disease.

M. Gautier and M. Peter have suggested the name of leucomaines for those artificial alkaloids which are developed during the life of the animal body. Some writers use the term ptomaines to include all the artificial animal alkaloids, whether formed in living or dead animal matter. Some few of the leucomaines are very poisonous, and others are harmless. In the liquid from suppurative peritonitis, Spica obtained bodies, some of which were oily and volatile, with a strong alkaline reaction and capable of forming crystalline salts, and having the odor of conine. The chloroform extract was extremely poisonous in its action on frogs, and in its physiological effects resembled

Digitized by Google

that of curarine. An alkaloid resembling atropine has been found in patients dying from typhus fever.

M. Peter read a paper before the Academy of Medicine of Paris, in 1886, in which he considers the animal alkaloid and also the extractive matter found in animal bodies, which are very poisonous. In considering these bodies M. Peter says: "We naturally turn to the chemical side. Here chemical experience steps in and shows that in proportion to the poisonous effects of these two kinds of bodies, we have a corresponding difference in heat. Poisoning by the extractive matters produces an increase in temperature, while poisoning by the animal alkaloids produces a decrease in temperature. We may see in the same organism an association or alternance of increase or lowered temperature, according as there is an association or alternance of the different poisons."

M. Peter believes that diseases are produced by animal alkaloids and extractive matters, and that microbes are mere accidents in the diseased process. That the series of partial deaths which make up life is the result of the working of the organs of animal life. That by the use of any organ of the body both alkaloids and extractive matter are liberated. By brain-work we have neurine liberated. Muscular movement causes creatine to be liberated. In the normal condition these materials are eliminated from the body, but if from any cause they are generated more rapidly than nature can eliminate, then a disease is established, according to the kind of animal alkaloid and extractive matter generated. "Let us suppose," he says, "that a certain quantity—say ten units—of extractive matters and alkaloids are produced in the organism of one man, and that he only eliminates eight per diem; obviously in twenty days he will have retained in his system forty units, which is sufficient for intoxication and disease. Henceforth medical intelligence will not hesitate between the parasite doctrine, full of dark hypotheses, and this new doctrine, as luminous as precise, which explains the normal and abnormal phenomena of life by life itself in action."

Microörganisms are found in air at all times. In breathing we draw them into the lungs, they collect upon the mucous membrane. In most cases, when the membrane is normal, they are thrown out of the system and do no harm, but under some circumstances they invade the system by passing through the membrane. They also enter the body with the food we eat. Whenever bacteria come in contact with animal matter and they commence to develop, we have artificial alkaloids and extractive matter formed. The septic process found when bacteria are present is not simply the bacteria themselves that do the mischief, but it is the products of their action upon tissue. The bacteria of decomposition brought into watery extracts of flesh in the presence of oxygen decompose albuminous matter, creatin, sugar and lactic acid into leucin, hydroparacumarsic acid, indol, skatol, ammonia, carbon dioxide, hydrogen and sulphureted hydrogen. Here we have some of the nitrogenous bases formed, and we have the ammonia, alcoholic and acid radicals in a favorable condition to form our artificial alkaloids, either amines or amides. Under other circumstances all of these changes do not take place, but ammonia is *always* produced, and ptomaines and leucomaines are ammonia compounds.

The action of water alone has the power of splitting up hippuric acid, and some of the nitrogenous bases in the living animal body under certain conditions, in that way producing the conditions and substances necessary to produce these artificial bases. There can be but little doubt that animal alkaloids and extractive matter do produce intoxication and disease, that these substances are always present where microörganisms are found. We also have the best of reasons to believe that these same, or similar, alkaloids and extractive matter may be found in the animal body without being of microbial origin. In a number of the recognized infectious diseases microörganisms have been discovered—one for each disease; with these alkaloids have been found. The question arises, "Do the microbes produce the alkaloids and extractive matters,

Digitized by Google

or are the alkaloids and extractive matters auto-infectious, and give rise to proper soil for the microbes to grow and multiply?" It has been shown that in some cases the tuberculous bacteria have disappeared from the lungs, yet the disease has progressed and resulted in death.

In my investigations and study of the ptomaines and leucomaines, I find that there are a few things in connection with this subject that we know, and that there are a great many more things that we do not know, and that the entire subject of the pathological relations of the artificial animal alkaloids is surrounded by hypotheses and theories, so that it is almost impossible to arrive at the exact amount of knowledge we possess in regard to them. But it is only by theories that we are able to arrive at a conclusion in some cases. A theory alone, not supported by facts and experiments, is of very little use to the pathologist and chemist. Of the known facts in regard to the animal alkaloids, we know: First. That we have nitrogenous basic substances found in the animal body during health as well as in disease. Second. That these basic substances have the properties of alkaloids, and that they are built upon the ammonia type, and derived from ammonia. Third. That they are either amines or amides. Fourth. That some of these are found in the human body in large quantities during health, and are not poisonous, but that some of them are unstable compounds and are capable of being broken up into other compounds which are poisonous. Fifth. That a number of poisonous alkaloids have been found in the human body during the progress of disease, or immediately after death. Sixth. That ptomaines are found when microbes have been at work upon animal tissue or product. Seventh. That nitrogenous bases or ptomaines have been found when no microbes have been discovered. Eighth. That nitrogenous extractive matter has been found in the animal body in disease when microörganisms have been found, and also in disease when the microbe has not been discovered.

It has been supposed that some of the known diseases depend upon the presence of microbes, and that the ptomaines were only the result of their action upon the cell and its contents, and that the poisonous extractive matter found in these cases was an excretion or secretion of the microbes, and that the intoxication and disease come from the ptomaines and extractive matter. By some, that the symptoms arose from the destruction of the cell and its contents, and not from the products of this destruction. Others believe that these ptomaines, leucomaines and extractive matter are found in the body as the result of normal tissue change, and that if from some cause they are liberated more rapidly than nature can eliminate them, or from defect in the eliminative process, they accumulate in the system and produce intoxication and disease; that the presence of microbes at these times are mere accidents; that the soil being of proper condition, the microbe makes its appearance, but that it is not the first cause of the disease.

There are a number of things that must be proven before we can arrive at a positive conclusion as to the pathological condition of the ptomaines and extractive matter. A series of experiments must be performed (some of which I hope to be able to perform at an early date), to prove whether any known form of microörganism, if separated from all alkaloids and extractive matter, and introduced into the system, will give rise to the disease or symptoms of the disease in which these same microörganisms are found. As far as I have been able to learn, the ptomaines and extractive matter have been introduced with the microbe, but the evil results following have always been attributed to the microbe. The alkaloids and extractive matter found in connection with these microbes must be separated from the microbes and then introduced into the system, to see whether we get the disease or symptoms of the disease. Then the alkaloids must be separated from the microbe and extractive matter, and it introduced and its effects watched; and again the alkaloid must be prepared synthetically, it being intro-

Digitized by Google

duced and its effects watched. The extractive matter should be separated from the microbe and alkaloid, and it introduced into the system and watched to see if it could act as the cause or ferment of the disease.

Dr. M. GREELEY PARKER, Lowell, Mass., read a paper entitled—

PHOTO-MICROGRAPHY THE BEST MEANS OF ILLUSTRATING AND TEACHING ANATOMY AND PATHOLOGY.

LA PHOTO-MICROGRAPHIE LE MEILLEUR MOYEN DE DÉMONTRER ET D'ENSEIGNER L'ANATOMIE ET LA PATHOLOGIE.

DIE PHOTO-MICROGRAPHIE DAS BESTE ILLUSTRATIONS- UND LEHRMITTEL DER ANATOMIE UND PATHOLOGIE.

Photo-micrography is the art of producing enlarged pictures of microscopical objects, by projecting the image of the object through the microscope, or combination of lenses, and catching this image many times enlarged on the sensitive film.

Micro-photography is a term sometimes used to express the same thing, but erroneously, as it is the art of producing miniature pictures of objects so minute as to require a magnifying glass for their examination. Thus we see that photo-micrography is the reverse of micro-photography: the one being the art of obtaining a large picture of a minute object, the other the art of obtaining a minute picture of a large object, while photography is the art of fixing the image produced by the camera, or combination of lenses, on a sensitive film.

Photo-micrography dates back to the discovery of Daguerre, published in 1839. After Daguerre came Reade, Donné, Hodgson, Kingsley and Talbot, early workers in this interesting field. After these, many in France, Germany, England and America took up this most interesting study; among the latter Woodward stands preëminent. His advantages in Washington enabled him to obtain photographs of diatomes which have never been surpassed. Koch used photo-micrography to delineate bacteria; his first publication was in 1877, in which he speaks in the highest terms of the method.

By photo-micrography we can demonstrate more clearly and teach more accurately the minute changes constantly taking place in disease than by any other means.

Photo-micrography is entirely void of any mental impression conveyed by the observer, through the engraver, to the picture.

A photo-micrograph is the true picture engraved by the constant, faithful, all observing artist "Light," as the lens sees it, and we all know that the lens sees vastly more than the human eye, therefore it should be the instrument used to record scientific investigations.

The oculist tells us that all eyes are not alike, and consequently do not see alike. The result is sometimes difference of opinion, and under such conditions the photo-micrograph is of value to settle the question.

It is of the greatest aid in microscopic research, as we saw yesterday, when used to illustrate on the screen the valuable papers read before this Section. Among the illustrations were some beautiful and attractive colored microscopical specimens, but all depended on photo-micrographs to illustrate the detail of disease; for in no other way can this be brought out as well.

Digitized by Google

The photo-micrographs and transparencies for the above illustrations were made by the aid of an ingenious and cheap heliostat, arranged by Dr. Cushing, of Boston, which can be manufactured by any tinsmith at a price within the means of all.

I shall not in this paper describe the process of making photo-micrographs, as this has been so well done in this country by Woodward,* Sternberg,† Walmsley, ‡ Piersoll, ‖ Y. May King § and others, but say to the beginner much time can be saved, fewer plates spoiled, and more satisfaction obtained, by taking a few practical lessons from a good instructor at first, rather than work the art out from books alone, although good results will be obtained if one follows the directions given by the above writers.

In focusing an object accurately, one will find some difficulty, owing to the great difference in eyes. It is true that blue glass or a solution of ammoniated sulphate of copper will greatly assist in getting a sharp chemical focus. The sharpness of this focus is what gives brilliancy and detail to the picture.

In my opinion, it is incorrect focusing rather than jarring that gives a blurred picture. What else can it be when the image is caught in the small fraction of a second? This is further illustrated by the individual who possesses an eye color blind as to greens and reds. For such an eye the blue glass is of little assistance, as it sees the chemical focus unaided, and in this branch of science possesses great advantages over the normal eye, which sees the many variations of color.

For this work, the color-blind eye has a great advantage over the normal eye—a fact I have not seen mentioned before.

There are many difficulties in photo-micrography yet unsolved, but good work can be done up to several hundred diameters, depending greatly on the specimen. The thinnest specimens, finely colored with alum cochineal, give admirable results; thickness of specimen or diffuse coloring are fatal to clearness and brilliancy of detail.

Having obtained a good negative of any valuable microscopical specimen, it is a satisfaction to know that the picture from this negative can be reproduced by various processes and at a price within the reach of all.

Everything considered, the stippled electrotype plate, prepared from the negative for presswork, is probably the best. This is used in illustrating many of our best magazines. It would be an invaluable addition to our medical journals, and would have a far reaching effect on the study and demonstration of pathological questions.

In studying microscopical objects it is impossible for several to examine the same object at the same time, unless the image is projected on a screen; even then it is interesting to know that the image would be found out of focus for any one who happened to be color blind. For the color blind a different adjustment must be made.

All microscopists know that with high powers the slightest turn of the fine adjustment may cause the image to present different lights and shades or disappear entirely. The same instrument is not used by all microscopists, consequently they do not all have the same illumination and the same amplification. One may work with a large diaphragm, the other with a small; one with a condenser, the other without. These differences and many others occur, even when the same specimen is passed around

---

* Woodward. *American Journal of Science and Arts*, Vol. XLII, Sept., 1866. Reports to the Surgeon-General, U. S. A., 1870–71.

† Sternberg. "Photo-micrographs and How to Make Them," published by Osgood & Co., 1883.

‡ Walmsley. Anthony's *Photographic Bulletin*, Dec. 25th, 1886, Jan. 8th–22d and Feb. 12th, 1887.

‖ Piersoll. *Medical News*, Phila., June 19th, 1886; *New York Medical Journal*, June 26th, 1886.

§ Y. May King. *New York Medical Journal*, July 2d, 1887.

Digitized by Google

for examination. Should each person prepare his own specimens a greater difference will occur. The method of preparing will not be the same, the cutting and staining will vary, and the mounting medium may not have the same refractive power.

Under all these varying conditions, how can we expect all to agree as to what they see, and more especially when the objects have to be magnified from 100 to 200 diameters before they are seen at all.

With these conditions, does any one wonder that varied opinions prevail, or that even the existence of some things is denied?

Under the existing circumstances it is impossible that it should be otherwise, and to settle the disputed points without the aid of something more is absolutely impossible. All this unrecorded microscopic study is lost to science.

Fortunately, we have in photography a method of recording that is most accurate and valuable; for no matter how careful one may be, his own drawings carry his own personal impressions, and when one attempts to record through the engraver, as we have before mentioned, he is no better off.

A good photo-micrograph is often more valuable than the original specimen, as it places the object before the observer in such a way that he can compare, measure and make his own observations, and draw his own conclusions. This is not possible with drawings.

Again, the photograph gives the picture with its lights and shadows. If these are not good in the specimen, one cannot improve them by photography. This, however, is not true with drawings; one can make a very good drawing from a very poor specimen, and by so doing give no idea of the value of the specimen. Not so with photography.

Photography reproduces the image so faithfully that in this reproduction it criticises the preparation of the specimen itself, and thereby claims its own superiority, a claim most valid, and one I think all will concede.

To illustrate this difference of representation by photo-micrograph and engraving, I submit Figs. 1 and 2, both being reproduced from the negatives by the same stippled electrotype process.

Fig. 1 is a copy of a wood-cut photographed from a book illustration, with its characteristic diagrammatic appearance of the arteries and columnar epithelium.

Fig. 2 is a copy direct from the photo-micrographic negative, not so much enlarged, but representing the same pathological change.

Digitized by Google

FIG. 1.

FIG. 2.

Digitized by Google

Dr. Pouzet, of Cannes, read a paper entitled—

DE LA PIGMENTATION DE LA PEAU AU NIVEAU DES ARTICULATIONS DES PHALANGES DANS LA CHLOROSE.

ON THE PIGMENTATION OF THE SKIN UPON THE LEVEL OF THE PHALANGEAL ARTICULATIONS IN CHLOROSIS.

ÜBER DIE PIGMENTATION DER HAUT AUF DEM NIVEAU DER FINGERGELENKE IN DER CHLOROSE.

Je viens attirer votre attention sur un signe clinique fourni par l'examen des doigts dans la chlorose.

Depuis longtemps M. le Professeur Bouchard de Paris, chaque fois qu'une chlorotique entre dans son service hospitalier, fait remarquer à ses élèves une coloration brunâtre spéciale de la peau sur la face dorsale de la main, au niveau de la deuxième articulation des phalanges.

Dans la pratique, j'ai eu fréquemment l'occasion de vérifier cette coïncidence, et recémment, j'ai pu examiner, dans des ateliers où on travaille la soie, les mains d'un nombre considérable d'ouvrières atteintes de chlorose.

Presque chez toutes, j'ai observé la pigmentation indiquée.

Pour constater cette coloration il suffit d'examiner avec soin la face dorsale de la main, en la présentant un peu obliquement au jour, si c'est nécessaire.

Au premier abord, les doigts, paraissent sales, on peut les faire laver, savonner avec soin, brosser, et après avoir attendu un instant pour laisser à la rougeur provoquée par cette petite opération, le temps de se dissiper, on aperçoit très distinctement une coloration brun-noirâtre sur toute la partie blessée de la peau qui recouvre l'articulation phalango-phalanginienne des doigts. Lorsque les doigts sont rapprochés, cette coloration est encore plus apparente, et l'on distingue comme un air de cercle brunâtre qui coupe les quatre doigts dans leur longueur.

Nous avons observé ce signe aussi bien chez les jeunes filles qui ne se livrent à aucun travail manuel, et qui portent habituellement des gants, que chez les ouvrières.

Parmi celle-ci, nous l'avons constaté chez quelques unes qui travaillent une partie de leur journée dans l'eau.

Ni la pigmentation de la peau, ni la couleur des cheveux ne parait créer une prédisposition.

Nous n'essayerons pas de donner l'explication de ce fait. Est-ce une pigmentation spéciale de la peau qui se fait dans ce lieu d'élection, ou bien, ses parties restent-elles pigmentées, tandis que les tissus voisins se décolorent sous l'influence de la chlorose ?

C'est ce que nous ne savons pas. Il nous a seulement paru intéressant de signaler ce fait de simple observation clinique, et d'ajouter ainsi une indication nouvelle à celles que nous fournit l'examen des doigts.

Nous devons déjà à M. le Professeur Bouchard de Paris, la connaissance des nodosités des doigts dans la dilatation de l'estomac, nous lui devons aussi celle de la pigmentation des doigts dans la chlorose.

Digitized by Google

SECTION X—DISEASES OF CHILDREN.

OFFICERS.

PRESIDENT: DR. J. LEWIS SMITH, New York.

VICE-PRESIDENTS.

HENRY ASHBY, M. D., Manchester, England.
Dr. W. B. ATKINSON, Philadelphia, Pa.
Dr. A. BLACKADER, Montreal, Canada.
WM. D. BOOKER, M. D., Baltimore, Md.
E. BOUCHUT, M. D., Paris, France.
WM. D. HAY, M. D., London, England.
Prof. AD. D'ESPINE, St. Malo.
Dr. CADET DE GASSICOURT, Paris, France.
Dr. DE SAINT GERMAIN, Paris, France.
JAS. FRED'K GOODHART, M. D., London, England.
Prof. H. HIRSCHSPRUNG, Copenhagen, Denmark.
Herr Prof. HOFRATH, Vienna, Austria.
Dr. ADONIRAM B. JUDSON, New York.
Dr. MONCORVO, Rio de Janeiro, Brazil.
Herr Prof. ALVIS MONTI, Vienna, Austria.
Dr. JNO. MORRIS, Baltimore, Md.
J. P. OLIVER, M. D., Boston, Mass.
MARTIN GAY BLACK OXLEY, M. D., Liverpool, England.
JULES SIMON, M. D., Paris, France.
Dr. EUSTACE SMITH, London, England.
WM. STEPHENSON, M. D., Aberdeen, Scotland.
NICOLAS TOLMATOCHOW, M. D., Kasan, Russia.
ALFRED VOGEL, M. D., München, Germany.
Dr. CHARLES WEST, London, England.
Dr. HERMAN WIDERHOFER, Vienna, Austria.
OSCAR WYSS, M.D., Zurich, Switzerland.

SECRETARIES.

DILLON BROWN, M. D., New York City.
HENRY COGGESHALL, M. D., New York City.
LUCIEN DAMAINVILLE, M. D., New York, N. Y.
I. N. LOVE, St. Louis, Mo.

COUNCIL.

Wm. T. Belfield, M. D., Chicago, Ill.
Edw'd H. Bradford, M. D., Boston, Mass.
H. Charlton, M. D., Seymour, Ind.
W. H. Conklin, M. D., Canton, Ohio.
W. J. Conklin, M. D., Dayton, Ohio.
Wm. H. Doughty, M. D., Augusta, Ga.
Jos. O. Dwyer, M. D., New York, N. Y.
C. W. Earle, M. D., Chicago, Ill.
Wm. D. Haggard, M. D., Nashville, Tenn.
M. P. Hatfield, M. D., Chicago, Ill.
J. A. Hodge, M. D., Henderson, Ky.
W. F. Holt, M. D., Macon, Ga.
A. A. Horner, M. D., Helena, Ark.
G. K. Johnson, M. D., Grand Rapids, Mich.
H. H. Middlekamp, M. D., Warrenton, Mo.
W. P. Northrop, M. D., New York, N. Y.
Jno. S. Reed, M. D., New York, N. Y.
B. H. Riggs, M. D., Selma, Ala.
Chas. A. Leale, M. D., New York, N. Y.
O. W. Sherwin, M. D., Woodstock, Vt.
A. L. Steele, M. D., St. Louis, Mo.
Edwin Walker, M. D., Evansville, Ind.
Frank E. Waxham, M. D., Chicago, Ill.
Edmund C. Wendt, M. D., New York, N. Y.
Leroy M. Yale, M. D., New York, N. Y.

Digitized by Google

FIRST DAY.

OPENING ADDRESS OF THE PRESIDENT.

GENTLEMEN:—The First International Medical Congress, held in Paris in August, 1867, originated and guided to a successful termination by distinguished physicians of the French metropolis, continued two weeks. Inasmuch as many subjects of universal interest are discussed at such a Congress, and the observations and experiences of physicians under very different circumstances are compared, a broader and more accurate knowledge of diseases is imparted, and discoveries of therapeutic interest and value become more widely known, so important is the work of an International Medical Congress, and so great its influence in advancing the highest interests of the profession, that it is, perhaps, a matter of regret that it has been thought best to limit its duration to one week instead of allowing two weeks, as in the First Congress.

This Section will have nine sessions, and twenty minutes will be allowed for the reading of each paper, in compliance with the rules of the Congress. The Executive Committee have provided for the probable lack of sufficient time in the discussions by a resolution, allowing any member present at the reading of a paper, and not having opportunity to discuss it, to send in writing the remarks which he desires to make, to one of the Secretaries, and if the Council of the Section consider them appropriate and worthy of publication, they will appear in the printed *Transactions*. Those who participate in the discussions will recollect that the narration of accurately-observed cases will throw more light on obscure or disputed subjects than does the dogmatic expression of opinions unsupported by clinical experience. Clinical facts or statistics upon matters of importance always have permanent value.

It will be seen by the programme that most of the papers to be read and subjects discussed are of a practical nature, and the profession will watch with interest the proceedings of this Section, and be benefited and instructed by the opinions expressed here. It cannot be doubted that the papers in this Section will promote a more accurate knowledge of several of the most important diseases of childhood, and lead to a better treatment of them. Some of these papers will, in my opinion, have a permanent place in pediatric literature.

In behalf of the Council, I wish to acknowledge our indebtedness to the foreign members of the Section who will actively participate in it, men known and honored in both hemispheres for their contributions to medical science. Though actively engaged in professional duties at home, and their writings eagerly sought for elsewhere, they have consented to prepare papers for this occasion. Seven papers have been promised from Great Britain, five from France, two from Switzerland, two from Germany and three from South America, in response to the invitation to aid in this great international gathering. A few of the papers will, probably, not be received in time to be read in the Section, but they will, of course, be printed in the *Transactions*.

Digitized by Google

I may mention, in particular, among those who have aided in furthering the work of the Section, Dr. Eustace Smith, of London, and Prof. Grancher, of Paris. Nor should I forget to speak of Dr. Bouchut, whose classical treatise on diseases of children, translated into various languages, was a text-book in the schools when the seniors in our profession were students. He has sent us two instructive papers; one of them upon a subject new to the profession, and he writes to us of his great disappointment, on account of ill-health, in not being able to be present to participate in the discussions. His interest in this Congress is greatly increased by the fact that it will aid in establishing throughout the world a mode of treatment of a most painful and fatal disease which he recommended thirty years ago, but which, encountering strong opposition and adversely reported on by a committee of the most distinguished physicians of the French Academy, fell into disuse and was forgotten. Intubation, in the treatment of membranous croup, revived and greatly improved through the genius and perseverance of an American member of this Section, will soon, in the opinion of all who have witnessed its simplicity and the marvelously quick relief which it gives, be practiced in all countries where there is a medical profession. To acquire a knowledge of this operation and the manner of using the tubes is one of the chief inducements which we offer to the foreign members for undertaking long voyages and incurring many inconveniences and sacrifices in coming to this Congress.

The history of intubation is interesting and instructive. We shall see from M. Bouchut's paper that in the few instances in which he practiced it, with rude and imperfect instruments, it relieved dyspnœa and prolonged or saved life. But it was opposed and even ridiculed by Trousseau and his compeers, who were bringing forward tracheotomy, and it died out and was forgotten. The dictum of the great masters of the profession, at whose feet we sit and learn, has more influence than the plain teaching of the senses. To M. Bouchut the credit is due of demonstrating the fact that the larynx tolerates the presence of a tube, although it exhibits such extreme irritability when a foreign substance accidentally enters it.

Intubation, as a means of relieving, not only membranous croup, but other forms of laryngeal stenosis, has, like vaccination, survived the opposition which it encountered in its earlier days, and the name of M. Bouchut will always be honorably mentioned in connection with this operation, while that of O'Dwyer will go down to posterity, along with that of Jenner, holding a conspicuous place among the benefactors of mankind. In justice to my countryman, I may say that he was ignorant of Bouchut's work in the same direction until, after many disappointments and failures, he had so improved his instruments that success was certain.

It is with pleasure that we are able to welcome South America to the sisterhood of countries represented in this Congress. Among the first of the foreign papers designed for the Congress, which arrived, were five from South America. South America, from whose flora we obtain some of our most useful medicines, will, if we may judge from our experiences in this Congress, have, in the near future, a medical literature of its own, and will contribute its full share of interesting topics for discussion to future Congresses. The papers which we have received from South America are the more interesting and important because we in the Northern Hemisphere have heretofore been very ignorant, not only of the diseases, but of most other matters of medical interest pertaining to this great Continent.

The first paper to which we will have the pleasure of listening relates to diphtheria, which is gradually extending throughout the civilized world, and is permanently established in many of our large cities. In New York City from 1000 to more than 2000 children die of this disease every year, and if we include those who die of

Digitized by Google

diphtheritic croup, which is a form of diphtheria, the number is one-third larger. Any facts or observations which will throw light on the nature of this fatal and now prevalent malady, or aid physicians in its treatment, will have a great and permanent value.

AN INVESTIGATION TO DETERMINE WHETHER THE ABSENCE OF SEWERAGE AND OF WATER POLLUTION DIMINISHES THE PREVALENCE AND SEVERITY OF DIPHTHERIA.*

INVESTIGATION POUR DÉTERMINER SI L'ABSENCE D'ÉGOUTTAGE ET DE LA POLLUTION DE L'EAU DIMINUE LE POUVOIR ET LA SÉVÉRITE DE LA DIPHTERIE.

EINE UNTERSUCHUNG, UM ZU ENTSCHEIDEN, OB DIE ABWESENHEIT VON ABZUGSKANÄLEN UND VON WASSERVERUNREINIGUNG DIE HÄUFIGKEIT UND STÄRKE DER DIPHTHERIE VERMINDERT.

BY CHAS. WARRINGTON EARLE, M.D.,

Professor of Diseases of Children, Woman's Medical College; Professor of Obstetrics, College Physicians and Surgeons, Chicago, U. S. A.

It may truthfully be said that no one cause has up to this time been named as producing diphtheria which has been universally accepted by the great mass of the profession.

All kinds of dirt—all kinds of emanations from every kind of filthiness—atmospheric influence and germs have been included in the etiology of this disease.

It has been claimed by many that imperfect sewerage has been the cause, and the people, urged on by the opinion of the doctors, frequently blame a sewer for poisoning a family and producing diphtheria, when, in my judgment, the cause should be placed elsewhere. It is much better for us to recognize the true cause, if it is possible to find it, rather than to attack an imaginary one, for it is possible that while we are fighting the supposed gas as the cause, we are losing sight of the real enemy which should engage our attention.

Jacobi says that cases of diphtheria which are traced to exhalations from sewers, or even to filthy habits of life, are very frequent. This opinion, especially in regard to sewerage, has been reiterated by scores and hundreds of physicians. It represents the prevailing idea of American physicians.

---

* The following named physicians have kindly contributed information embodied in this paper:—

Drs. J. S. Kreychie, I. R. Spooner, R. A. Cushman, from Dakota.

Drs. G. G. Tyrrell, Charles Ambrook, H. C. Croweler, C. M. Bates, F. H. Payne, from California.

Drs. W. W. Hammond, J. D. Carnahan, August Boucher, Gilbert Gregor, F. S. Kahler, A. C. Ormsby, Jno. Alfred, from Utah.

Drs. E. A. Spaulding, Herbert Hulburd, from Minnesota.

Drs. G. B. Snyder, E. E. Hazlett, J. M. Wade, from Kansas.

Drs. A. D. Kibbie, J. K. Dubois, from Idaho.

Dr. J. C. Watkins from Wyoming.

Drs. B. B. Kelly, Alex McLeod, from Montana.

Digitized by Google

English practitioners are particularly the advocates of sewer-gas theory. In the *British Medical Journal*, December 1st, 1883, J. Emmett Holt gives an account of the disease as it occurred in Canterbury. Investigation showed the well water to be extensively contaminated with sewer gas in every instance. In addition to the drinking water being polluted, there was found a drain running under the parlor, letting sewer gas enter the living room of the family.

I cannot hope to have discovered the one cause of diphtheria, or to prove that sewer gas does not in many cases influence this disease. But I have been impressed for many years that undue importance was given it as a causative factor, and have thus been led to investigate the prevalence of this dreadful disease in locations remote from any sewer-gas influence.

It appeared to me that investigations to demonstate my theory should be conducted at great distances from sewers, and the location must have a pure air with pure water, and that in the great Northwest of the United States of America these conditions could best be found. In the following briefly described States and Territories I concluded to make my inquiries:—

Minnesota.—Population (1885), 1,117,798; square miles, 83,531. Rolling prairies, dotted with lakes and belts of timber. The summers are cool and pleasant, and while the winters are cold, the air is dry and clear.

Dakota.—Population, 415,263; square miles, 150,932. In the main prairie; climate mild and genial; winters cold and dry; bracing and invigorating air; average rainfall, twenty inches.

Montana.—Population (1880), 39,159; square miles, 143,776. Climate milder than would be supposed from the altitude; soil exceedingly fertile; a great grazing country.

Wyoming.—Population (1880), 20,789; square miles, 97,980; rich in minerals, and its surface is broken by mountain ranges and deep river canyons.

Kansas.—Population (1880), 996,096; square miles, 81,318. The surface, which is made up of prairies and river bottoms, slopes from the Rocky Mountains to rivers on the east. The climate is healthful.

Utah.—Population (1880), 143,963; square miles, 84,476. An immense plateau between the Rockies and Sierras, consisting of smaller mountains, plains, valleys and basins. Climate mild and dry.

Idaho.—Population (1880), 32,610; square miles, 86,294. Climate cool and healthful. This territory is rich in minerals, but the cereals flourish abundantly. It contains vast forests of fir, pine and hemlock.

California.—Population (1880), 864,694; square miles, 188,981. Climate variable, on account of extent of territory along the Pacific coast. Temperature ranges from 50° below in the north to 70° above at the south.

These States and Territories help to make up the great Northwest of our country. Part of this immense region is traversed by our highest mountains and watered by rapid rivers. Much of it is prairie. The purest air in the world is abundant, and many of the habitations, particularly in the early times, were built on posts some distance from the ground. There is not a sewer within a hundred miles of some of these houses, and yet diphtheria is found. To the physicians in this great country I addressed a communication with the following questions:—

1. Does diphtheria occur frequently in your vicinity?
2. What season of the year to the greatest extent?
3. How far are you from any sewers?
4. Have you ever attended cases exceedingly remote from any possible source of infection?
5. Have cases occurred in your practice in habitations raised from the ground so that no possible decomposing material under the house could act as a cause?

Digitized by Google

6. Have cases occurred in your practice among the mountains, where the water supply is from pure springs or running brooks, where pollution from cesspools, water-closets or sewers was absolutely impossible?

7. Does proximity to barns or stables seem to predispose to the disease?

From among many who have kindly answered my communication, I select the following as having some bearing on the questions involved :—

Dr. Hulburd says, "our cases are almost without exception traced to some infected person or clothing." He also gives another cause, which I think demands attention, that is, railroad cars as a means of conveying the contagion from one place to another, and as the place where the contagion may be taken into the systems of little people who are riding in the coaches.

Dr. Spalding says that he passed through a severe epidemic with great fatality two years ago. "Our drinking water is free from all organic matter, and the habitations are upon high ground."

Dr. Cushman says that cases appear sporadically, and then another case appearing miles away with no visible connection. "Cases occurring in sod houses and houses situated on low ground seem more virulent."

Dr. Spooner, whose experience has been great, according to the testimony of others, answers my questions very fully, to such an extent that I produce nearly his entire letter :—

LAKE PRAIRIE, KINGSBURY COUNTY, DAKOTA, July 14th, 1887.

DEAR DOCTOR :—

I am pleased to receive your letter of inquiry, and will endeavor to answer to the best of my ability.

Query 1. We have cases of diphtheria in almost every month in the year. Since May 9th last, there have occurred in my practice in this vicinity eight cases. In the practice of others, four cases, and two cases that were undoubtedly diphtheria that were here, making fourteen cases. Two of them undoubtedly contracted the disease in transit from Norway, as one of the same family, a babe, died on shipboard from the same disease, if the statements of the parents are to be relied on. Five of my cases contracted the disease after coming to Dakota. So the disease, so far as we are able to ascertain, really originated here in one-half these cases only. In the four treated by others the origin of the cases is unknown. In the three remaining cases treated by myself, one visited a family where a patient had had the disease and was supposed to be convalescent; but that was the only possible source of infection, so far as I could gather from carefully questioning the patient and friends. The remaining two cases of mine were patients who had had the disease before; origin at this time unknown.

Query 2. I cannot say positively, but the majority of cases have occurred from January to September in this territory, in my practice.

Query 3. We are forty miles from any sewers; so far that it does not enter into the consideration of this question at all. We may say, perhaps, that a lack of proper drainage does furnish suitable conditions for either the development or the propagation of the diphtheria germ.

Query 4. I have attended cases where it was impossible to decide the cause of the disease; and, as far as being remote from any "possible source," it would be exceedingly difficult to decide, but I can say they were remote from sewers; in fact, all cases coming under my observation have been those where the sewerage did not enter into the consideration of the case at all.

Query 5. In a number of instances the cellars have been perfectly clean and free from decaying vegetables. In at least six or seven instances of separate families it has been the case that there has been no *possible* source of infection from the presence of decomposing material under the house.

Query 6. I have never practiced in mountainous districts, but some of my most violent and malignant cases have occurred on the highest points of land in the country. The cause, to my mind, was evident, *i.e.*, an unclean cistern, with no attention to hygiene indoors or out.

Query 7. My opinion is that proximity to barns and stables enhances the liability to the disease. I cannot say it causes it, but believe, the diphtheria germs being present as the prime

Digitized by Google

factor, these other conditions are potent factors in the propagation of the disease and in the continuation of it.

In Dakota my practice has been on the prairie exclusively, as there is nothing but prairie here. Our prairie has scattered all over it ponds of water during a wet, or moderately wet, season. We are in close proximity to a number of lakes, which are really the head waters of the Vermillion, a stream one hundred miles in length, a tributary of the Missouri. We are on a point of land which has nearly the greatest elevation in Southern Dakota, unless it be the Washington Hills. The surface is undulating. Winds prevail from the south and southwest. The soil is a black sandy loam; underlying this a yellow clay, except in the vicinity of the lakes the clay is shallow and more sand appears in the soil. The atmosphere contains less moisture than in Iowa or Minnesota; is what is termed a dry atmosphere.

The wells in this vicinity are generally dug through the loam, and the water is found in either sand and gravel or clay, at a depth varying from eight or ten feet to thirty. The majority of wells average about twelve feet.

Dr. Kreychie says that he has seen cases in houses where potatoes and other vegetables were stored under the building. Several gentlemen seem to think that decomposing material, such as vegetables, especially with moisture, predispose to the disease. Two or three children will die in the same house within a very short period. Indeed, the virulency appears quite as terrible as in our large cities. The same gentleman narrates the history of a family of five persons living in a sod shanty with filthy surroundings, a cesspool and a lot of manure in front of the shanty. Three children were taken with the disease, and all died within the space of about ten days. Some miles from his home there live a few families on the banks of a sluggish stream of water that flows only in the spring. There has not been a spring since he settled there that some of those families have not had a case of severe diphtheria occurring.

Dr. Kelly has attended cases of malignant diphtheria remote from sewers and all possible sources of infection. He attended one case five miles from any other habitation. He believes proximity to stables where there is a good deal of filth seems to predispose to the disease.

Dr. Watkins says diphtheria is rare in Wyoming; that he has seen only one case in eight years, and that of a very virulent type. He thinks that was caused by the child's stirring up the town dump of manure.

Dr. Hazlett says that in one epidemic in his county, in a town on the high prairie, he had a number of cases, many of which died in spite of all that could be done. In one of his families, where all possible pains had been taken in regard to cleanliness and to escape any effect of decomposition, the house standing on raised ground and some distance from barns, fatal cases occurred in his practice.

Dr. Ormsby writes that he has found diphtheria in houses miles from any others, at the mouth of a canyon, where the purest of mountain air prevailed and no possible chance for decomposition existing. He does not see how sewer gas or decaying vegetables can have anything to do with the disease.

Dr. Gregor says that the epidemic of 1882 was particularly fatal in a little settlement near the top of the mountain, at an elevation of 9000 feet; this settlement consisted of probably a dozen houses built around what used to be a small lake, but at that time dry. The water had been drained off from the lake by a mine below. The water used by these people, however, for household purposes, is perfectly pure, coming from water-courses from above, and where it was not possible that contamination existed.

Dr. Kahler says that it occurs frequently in elevated situations where no possible decomposing material existed. It takes place frequently in mountainous districts where the water is pure and no cesspools or water-closets are present.

Dr. Dubois, 290 miles from a railroad, in a vicinity where there are no sewers, has

Digitized by Google

passed through one epidemic of diphtheria. Water supplied from springs, and the climate is particularly unfavorable to the conditions usually producing this disease.

Dr. Croweler says that the worst case he ever saw was in habitations upon the sandy soil in the hills and fifteen miles away from any point of infection. He knows from experience that it will and can exist in all its malignancy where there are no sewers.

Dr. Frank H. Payne says: "I have had considerable experience with diphtheria, especially during the winter of 1885–6, when about seventy cases occurred in my practice. I found that the disease occurred under *every* condition and circumstance of life; sometimes as divergingly different as could possibly be. In miserable, filthy hovels, with poor sewerage and every condition favorable for its propagation, and again in well-ventilated, well-sewered houses where disinfection is constantly practiced and cleanliness observed.

"I have had isolated cases in the mountains where no sewers existed and every condition seemed favorable to health. But I must confess that it is very difficult to stamp out the disease when it exists in an unfavorable, bad sewered locality. Such conditions seem to favor its spread, and I am also inclined to believe have some influence upon the disease as to its virulence.

"In a region swept by the fumes from an acid factory (sulphuric acid) I have never had cases except once, when they were directly traceable to exposure to a virulent form of the disease while in the city."

Dr. Tyrrell says that, inasmuch as he is a strong believer in the specific cause of diphtheria, he thinks that sewer gas has nothing to do with it. His belief is, no germs, no diphtheria.

Everything, however, points to the germ theory as the cause of this disease. However much we may try to avoid accepting the theory, there is coming to be such a mass of evidence in favor of it that it is almost irresistible.

CONCLUSIONS.

1. Diphtheria occurs in the mountains and prairies of the great new northwest with the same malignancy as in cities.

2. Diphtheria takes place with equal virulence in vicinities remote from sewers.

3. Diphtheria once present, the inhabitants in damp sod houses, those living over cellars containing decomposing vegetables and in proximity to manure heaps and poorly constructed sewers, seem to be in surroundings which tend to increase the severity of the malady.

4. The fact is again demonstrated, although developed incidentally, that the contagious element may be carried or transported thousands of miles in a manner difficult to understand.

5. Testimony is abundant that the poison may be transported by means of cars and steamers. This fact calls for increasing watchfulness and more efficient means of disinfection than have been heretofore practiced by our railroad companies.

6. To such a degree is this terrible disease contagious, a few in the profession refusing to acknowledge it, and the majority of the people persistently refusing to isolate their children, it appears to me that the only method by which we can prevent the spread of diphtheria is by the enactment of laws compelling the people to assume some responsibility in regard to contagious diseases.

DISCUSSION.

Dr. F. E. Waxham, of Chicago, said:—I believe that diphtheria is due to the absence of sewers rather than to their presence. It is true that in our large and crowded cities we meet with the disease far more frequently among the poor, who

Digitized by Google

live in districts without sewers, than among those living in well-sewered districts. That the disease is contagious there is no question, and that filth, decomposing vegetable matter, impure air and bad hygienic conditions are important factors in its production, I cannot doubt. Recently it was my privilege to attend five children with diphtheria in a house consisting of a cellar and two flats. There were three children in one family and two in the other. In the cellar was found a large quantity of decomposed vegetable matter around which the children had been playing every day.

Absolute cleanliness, which means disinfection as well, is our great remedy in preventing this disease.

REMARKS BY THE PRESIDENT.—The interesting and instructive paper of Dr. Earle opens for discussion a subject of the greatest importance to the public as well as physicians. That is, how to prevent the spread of diphtheria. His statistics show that it desolates families in salubrious rural localities where there is no impurity in the air or water. They demonstrate the fact of its extreme contagiousness from person to person and through infected articles, either of wearing or merchandise. On the other hand, we in the cities have abundant and melancholy proof of the causative relation of foul air, whether arising from the sewers or stagnant filth, to diphtheria. Dr. Sternberg, in his recent prize essay, published by the American Public Health Association, remarks as follows: "It seems extremely probable that the diphtheritic poison—germ—is capable of increase, independently of the sick, in damp, foul places, such as sewers, damp cellars, and especially under old houses in which the floors come near the surface of the ground, leaving a damp, ill-ventilated space. At all events the disease often clings to such houses, in spite of the application of the usual means of disinfection. There is no doubt as to the influence of bad hygienic conditions in maintaining the infection when the disease has been introduced, and it is possible that such conditions may, in certain cases, originate it." These views, as regards the causative relation of foul air to diphtheria, are abundantly sustained by clinical observations in the cities where diphtheria is established. Thus, in New York, prior to 1850, although foul sewers and insanitary conditions existed, there was no diphtheria; but in the decade following 1850 diphtheria was introduced; its germ found its way into the sewers, where, in the filth underground, it obtained a nidus favorable for its development, and for many years, in every street in this city, wherever sewer gas escapes into the houses children fall victims to this disease. "Sanitary plumbing," although it probably has diminished the number of cases, has failed to prevent the occurrence of diphtheria.

Dr. Earle recommends legal enactments to check the spread of diphtheria, and certainly the most stringent enactments should be passed in the face of a pestilence that is desolating so many of our families. Still, I think that the disease is beyond the control of law makers. One of the most common ways in which diphtheria is propagated is by walking patients. Children with slight diphtheritic sore throat go to the dispensaries and sit among other children, to the schools and infect a whole school room, and to the doctor's office and expose his children and other children who may be waiting. I was once summoned by a little girl from my office to see her brother, who had diphtheria. It occurred to me to examine her throat. She was in the street with her playmates. She did not complain and seemed well, but a diphtheritic patch was found upon her fauces. Physicians, whenever diphtheria occurs, have not only a professional but a deep personal interest in arresting its spread, for wherever it is established it desolates more physicians' families, from the peculiar manner in which they are exposed, than do all other contagious diseases together.

Digitized by Google

The praiseworthy endeavor to prevent diphtheria by excluding affected children from the society of others is only partially successful, on account of the large number of mild, undetected and unsuspected cases. Children mildly affected and under no restraint communicate the disease, in numberless instances, to well children, who sicken, and their parents are ignorant of the time and mode of exposure.

ON THE RATE OF GROWTH IN CHILDREN.

DU DEGRÉ DE CROISSANCE DANS LES ENFANTS.

ÜBER DEN GRAD DES WACHSTHUMS BEI KINDERN.

BY WM. STEPHENSON, M.D.,

Professor of Midwifery and the Diseases of Women and Children, University of Aberdeen, Scotland.

From the self-apparent truth that the presence of active growth and development is the distinguishing characteristic of childhood, little advance has been made in our knowledge of the facts of that growth, or of the influences, for good or for evil which such activity exerts. Nothing, as yet, is known of the clinical bearings of the relative height and weight of a child, and many important questions remain unsolved.

What is the relative activity of growth one year with another? Is it a uniformly diminishing quantity from the first year of life onward, or are there periods of increasd action following upon others of comparative rest? Children do grow more rapidly at times than at others, but do these times follow any law, or do they occur in irregular and uncertain spurts?

If a boy adds to his weight, in a certain year, double or treble the number of pounds which he does in another, is he, on account of this increase of cell activity, the more or the less able to bear a strain, such as school pressure or physical labor?

Again, what influence has the activity of growth on the predisposition to disease? What effects in after life may arise from the process of growth being disturbed in the earlier years? How far is this process swayed by the occurrence of temporary ailments and the variations in surrounding circumstances?

Here there is a comparatively unknown territory in Pediatrics—a field which cannot be explored by any one man, but requires the accumulation of the work of many observers in different countries, and extending over many years.

The reason why our knowledge of the subject is so defective is, that till recently we did not possess the data necessary to furnish a basis of observation and a guide to the proper methods to be employed. The basis of observation required, is *a reliable standard of development and growth for each year of life, and the deduction therefrom of a proper method of comparing one child with another.* By the exertions and labor of Professor Bowditch, in America, and of the Anthropological Committee of the British Association, we now possess material from both nations for forming such a standard. Each group of statistics—American and British—is in itself sufficiently large to supply reliable results. Taken separately they act as checks, one on the other, in the elimination of accidental appearances, and in confirming the results where both agree. Taken together, the averages may confidently be accepted as representing A TYPICAL STANDARD FOR THE ENGLISH-SPEAKING RACES.

Unfortunately, the observations for the years under five are not sufficiently numerous to yield reliable results; they are, therefore, not included in this paper. The present investigation has also stopped at the eighteenth year, as the American statistics do not as

Digitized by Google

yet extend beyond. The observations on girls, especially the British, are not so numerous as could be desired; the results, therefore, should be accepted as so far tentative.

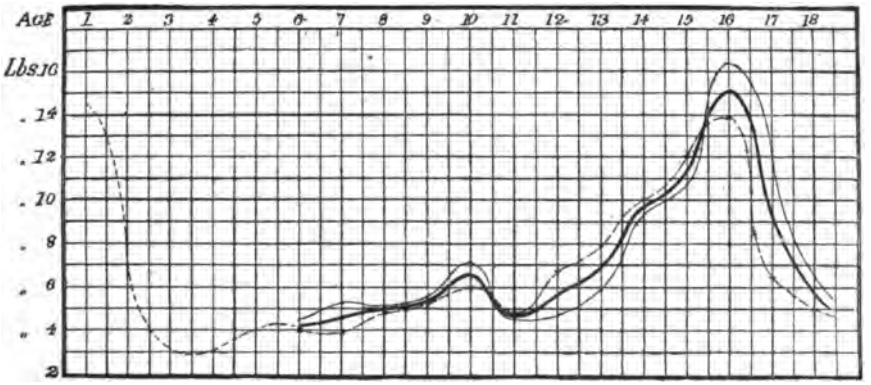
In the tables appended will be found the averages of all the observations of the height and weight of American and British boys and girls for each year of age, which may, therefore, be regarded as the *typical height and weight of the English-speaking races.* From this is easily obtained—

THE RELATIVE ACTIVITY OF GROWTH ONE YEAR WITH ANOTHER.

The numbers are given in the tables, but a clearer conception of the relative growth, or the difference of one year with another, can be obtained from the graphic representation given in the charts. The annual increase in weight is taken as the measure of the activity, and the actual number of pounds weight is used in preference to a percentage, as being equally serviceable and more ready of reference in after use when applying the standard to individual cases.

The American measurements, it will be seen, correspond very closely with the British. The object at present is, however, not to compare the one race with the other

ANNUAL INCREASE IN WEIGHT IN BOYS.



as to height and weight, but only as to time. The exact accordance of the periods of rise and fall in both nations, as shown in the charts, convincingly proves that *the relative rate of growth, one year with another, is dependent upon a distinct law, and is not arbitrary or uncertain as to time.*

The graphic line representing the annual increase in weight presents a definite curve, of a similar type in girls as in boys, but differing in the times of maxima and minima. In both a maximum activity occurs in the first year of life; it then rapidly falls to a minimum between the third and fourth years. After various changes a second maximum is reached, in girls in the thirteenth year and boys in the sixteenth. Thereafter it falls rapidly, but in girls more gradually than in boys.

A secondary rise occurs, in the eighth year in girls and the tenth in boys, followed by a marked fall, in the ninth and eleventh years respectively. (See charts.)

These features are so constant, not only in the American and British statistics separately, but also in the various sections of society, such as the professional and artisan class, that they must be considered as facts, and not due to imperfections in the observations.

These curves of growth furnish a clearer conception than can otherwise be obtained

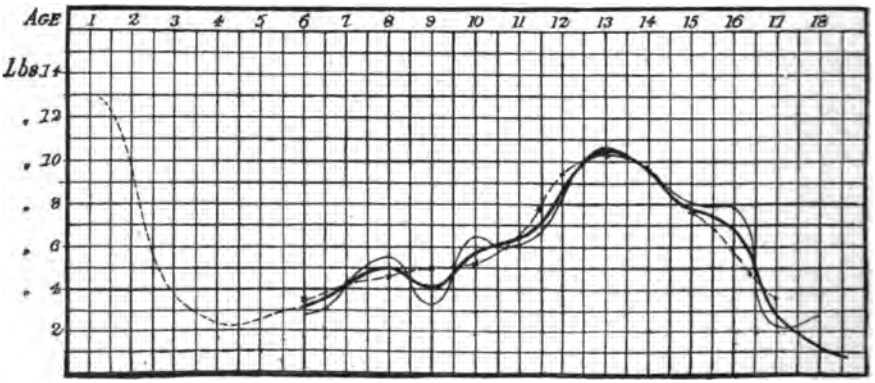
Digitized by Google

of the activity of growth at different ages, and the differences which exist between male and female. They show how important it is that in all statistics the data should be given for each year, and not in quinquennial or decimal periods. They point out wherein the growth of males differs from that of females, and that, therefore, boys should not be compared with girls of the same age, but that, for example, the sixteenth year in boys corresponds with the thirteenth in girls, the eleventh with the ninth, and so on. We thus reach what may prove to be a general law, that as physiological periods differ in the female from the male, in man they are in the ratio of four to five.

The well-marked retardation of growth in the tenth and eleventh years respectively, is a fact to which attention has not previously been drawn, but will doubtless be found to have important clinical bearings. So, also, with the striking peculiarity of the thirteenth and the sixteenth years. Is a boy or girl less able, during this period of rapid growth, to bear the strain of school work? At first sight one is disposed to think that such would be the case, but there are grounds to doubt this; the question is one which has still to be worked out.

The critical or trying character of the period known as puberty has always been associated with the idea that it was dependent upon the development of the generative

ANNUAL INCREASE IN WEIGHT IN GIRLS.



organs. The sexual development has, however, hardly begun, and the charts show that puberty more properly coincides with the demand made on the system to meet the increased activity of growth of the body, which is to take place before the reproductive function is called into action.

Having now obtained a typical standard of the height and weight for each year of age, the question arises, How are individual cases and the statistics of the different countries and classes of society to be compared with this standard? The want of a proper method of comparison has hindered the practical outcome of the extensive researches that have been made. The subject is complicated by there being three factors—age, height and weight. The ratio of weight to height increases with age in a proportion easily calculated. But children of the same age vary considerably in height, and one child may be equally well developed as to weight with another of the same age who is six inches taller. Some method, therefore, is required, whereby we may calculate the proportional weight to the varying height at each age. The following law has been deduced from an analysis of the tables:—

Let H = height, W = weight, N = age, and introduce a fourth term, m = a standard number which can be readily found for each year of age. The law may now

Digitized by Google

be stated as follows: *$m - n$ varies directly as the square of the height and inversely as the weight, or by the formula* $m - n = \frac{H^2}{W}$.

The tables supply the standard H and W for each year, so the value of m can be readily found for each age. Then by the formula $\frac{H^2}{m-n} = W$, we can obtain the typical weight for each inch of stature for the respective years.

Tables III and IV have in this way been calculated, and enable us readily to compare any sets of observations, or determine in individual cases whether the weight is duly proportioned to the height.

The reliability and value of these tables can be tested by a comparison therewith of the statistics of the various classes of society.* The results may be stated shortly as follows:—

The English statistics are divided into four classes:—

1. *The Professional Class (Town and Country).* Here the heights for the several years exceed the standard by 1½ to 2 inches, but this is accompanied by a deficiency in weight of from three to six pounds, the highest number occurring from the twelfth to the fifteenth year inclusive.

2. *The Commercial Class (Towns).* The heights do not vary from the standard more than a fractional part of an inch, the difference, however, being on the plus side. The weights, as in the former class, again show a deficiency, but to a lesser extent, viz., from one to four pounds. The larger number here also occurs from the thirteenth to the fifteenth year inclusive.

3. *The Artisan Class (Towns).* Here the stature is *under* the standard by 1 to 1.8 inches, but the weights are exactly proportional until after the thirteenth year. Thereafter they fall short by one to four pounds.

4. *The Labor Class (Country).* The stature here, like the artisans in towns, is below the standard. The more favorable circumstances for the promotion of growth is shown in the constant excess of their weight over the standard, of from one to four pounds.

The variations in these cases are instructive and interesting. What may be termed the advantages of life, or affluence, tends to a relative increase in height, but do not promote weight till after the activity of growth in height is completed. The less favorable circumstances which surround the laboring classes diminishes their stature, but influences less the increase of weight. The fall in relative weight after the 13th year in the town artisan class indicates the influence of occupation, as that is the age at which they begin their trades.

The maximum efficiency in weight in the more favored classes, *i. e.*, the professional and commercial, occurs, as has been noted, from the twelfth to the fifteenth years. In the latter it falls in the sixteenth year from 4.6 to 1 pound, at which it continues; in the former the fall is from 6½ to 5 pounds only, at which it continues for the next three years. These facts seem to point to an educational influence or school pressure. In the commercial class boys leave school at about 15, and thereafter the defect diminishes, whereas, in the professional class education is carried on to 18, and further, and its influence is marked by the continuance of the depressing effect in a high degree up to that year.

The American statistics at our command permit of division into two classes: the "American born" and the "Anglo-American." They cannot properly be compared with the English divisions, but the results are interesting.

The boys "American born" accord with the standard very closely, varying only a small fractional part of an inch in stature throughout. In weight there is a tendency to be under the standard, but not to the extent of a pound, until the thirteenth year,

* Trans. Brit. Ass., 1881, p. 243.

Digitized by Google

when the increased activity of growth which then occurs is well met by an excess of three and four pounds in the fifteenth and sixteenth years. In the seventeenth and eighteenth years they are one and two pounds light.

In the Anglo-Americans a difference is perceptible. They show from the sixth to the ninth years, inclusive, the same tendency to exceed in weight which is noticed in the English artisan class, but not the corresponding deficiency in height. After the tenth year this excess disappears, and thereafter they accord closely in weight with the standard.

Among girls "American born" the standard is kept to a fractional degree throughout. The statistics of English girls, as yet obtained, manifest the tendency to excess in weight in the years before 12, and the falling short after that period, which characterised the artisan class of English boys, a circumstance which points to the probability that the statistics have chiefly been derived from the less favored class of society. These remarks apply to the total numbers and not to individuals of each class.

Individual cases may in like manner be examined. Thus, a boy seven years old is 49 inches in stature and 61 pounds in weight. By reference to table III he is found to be fully three inches above the standard in height, and four pounds heavier than the proportional weight, therefore, a well-developed youth. His sister, on the other hand, who is 12 years old, 53 inches in height and 64 pounds in weight, is two inches under the standard and six pounds too light for her stature.

In the selection of youths for the public services this method of comparison will be found of great and ready service. The results of observations have yet to be compared with those of clinical experience, and enough has been given to show that the subject of this paper opens up a very large field for future research.

TABLE I.—TYPICAL STANDARD OF GROWTH.

AVERAGES OF COMBINED OBSERVATIONS OF HEIGHT AND WEIGHT OF AMERICAN AND BRITISH BOYS.

Age.	Height. Inches.	Gain in Height.	Weight. Pounds.	Gain in Weight.	Ratio. $\frac{W}{H}$	Ratio. $\frac{H}{W}$
5	41.30	...	40.49	...	.98	1.02
6	43.88	2.58	44.79	4.30	1.02	.98
7	45.86	1.98	49.39	4.60	1.07	.92
8	47.41	1.55	54.41	5.02	1.14	.87
9	49.69	2.28	59.82	5.41	1.20	.83
10	51.76	2.07	66.40	6.58	1.28	.77
11	53.47	1.71	71.09	4.69	1.32	.75
12	55.05	1.58	76.81	5.72	1.39	.71
13	57 06	2.01	83.72	6.91	1.46	.68
14	59.60	2.54	93.46	9 74	1.56	.63
15	62.27	2.67	104.90	11.44	1.68	.59
16	64.66	2.39	120.00	15.10	1.85	.53
17	66.20	1.54	129.19	9.19	1.95	.51
18	66.81	.61	134.97	5.78	2.02	.42

Digitized by Google

TABLE II.—TYPICAL STANDARD OF GROWTH.

AVERAGES OF COMBINED OBSERVATIONS OF HEIGHT AND WEIGHT OF AMERICAN AND BRITISH GIRLS.

Age.	Height. Inches.	Gain in Height.	Weight. Pounds.	Gain in Weight.	Ratio. $\frac{W}{H}$	Ratio. $\frac{H}{W}$
5	41.05	...	39.63	...	.96	1.03
6	42.99	1.94	42.84	3.21	.99	1.00
7	44.98	1.99	47.08	4.24	1.03	.96
8	47.09	2.11	52.12	5.04	1.10	.90
9	49.05	1.96	56.28	4.16	1.14	.87
10	51.19	2.14	62.17	5.89	1.21	.82
11	53.26	2 07	68.47	6.30	1.28	.77
12	55 77	2.51	77 35	8.88	1.38	.72
13	57.96	2.19	87.82	10.47	1.51	.65
14	59.87	1.91	97.56	9.74	1.62	.61
15	61.01	1.14	105.44	7.88	1 72	.57
16	61.67	.66	112.36	6.92	1.82	.54
17	62.22	.55	115.21	2.85	1.85	.54
18	62 19	...	116.43	1.22	1.87	...

TABLE III.

STANDARD WEIGHT, IN POUNDS, FOR EACH INCH IN HEIGHT, IN BOYS FROM 6 TO 18 YEARS OF AGE. CALCULATED BY FORMULA, $\frac{H^3}{M-N} = W$.*

Inches.	Age.														Inches.
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	
40	37	37						64	64						50
41	39	39	40	41				66	66	68					51
42	41	41	42	43	43			69	69	71					52
43	43	43	44	45	45	46		72	72	73					53
44	45	45	46	47	47	48		74	74	76	78				54
45	47	47	48	49	49	50	50	77	77	79	81				55
46		49	50	51	51	52	52	80	80	82	84	89			56
47		51	52	53	53	55	55	83	83	85	87	92			57
48		53	54	56	56	57	57	86	86	88	91	96	99		58
49			57	58	58	60	60	89	89	91	94	99	102	106	59
50			59	61	61	62	62	92	92	94	97	103	106	109	60
51				63	63	65	65	95	95	98	100	106	109	113	61
52				66	66	67	67		98	101	104	109	113	116	62
53				68	68	70	70		101	104	107	113	116	120	63
54				71	71	72	72		105	107	110	117	120	124	64
55					73	75	75			111	114	120	124	128	65
56						78	78			114	117	124	128	132	66
57						81	81			118	121	128	132	136	67
58						84	84			121	125	132	136	140	68
59							87				128	136	140	144	69
60											132	140	144	148	70
61											136	144	148	152	71
*M =	48	49	49	49	50	50	51	51	52	52	52	51	51	51	N = Age.

The dark line across each column indicates the average height and weight for the respective years.

Digitized by Google

TABLE IV.

STANDARD WEIGHT, IN POUNDS, FOR EACH INCH IN HEIGHT, IN GIRLS FROM 5 TO 18 YEARS OF AGE. CALCULATED BY FORMULA $\frac{H_2}{M-N} - W$.*

Inches.	Age.														Inches.
	5	6	7	8	9	10	11	12	13	14	15	16	17	18	
38	33														48
39	35	35	35					60							49
40	37	37	37					62	65						50
41	39	39	39	40				65	68						51
42	41	41	41	42	42			67	71						52
43	43	43	43	44	44	44		70	74	78					53
44	45	45	45	46	46	46		73	76	81	83				54
45	47	47	47	48	48	48		75	79	84	86				55
46		49	49	50	50	50	51	78	82	87	89	92			56
47		51	51	52	52	52	53	81	85	90	92	95	98		57
48			53	54	54	54	56	84	88	93	96	99	102	102	58
49			55	57	57	57	58	87	91	96	99	102	105	105	59
50				59	59	59	61	90	94	100	102	105	109	109	60
51				62	62	62	63		97	103	106	109	112	112	61
52				64	64	64	66		101	106	109	113	116	116	62
53					66	66	68		104	110	113	116	120	120	63
54					69	69	71		107	113	117	120	124	124	64
55						72	73			117	120	124	128	128	65
56						74	76			121	124	128	132	132	66
57						77	79				128	132	136	136	67
58							82					136	140	140	68
*M —	48	49	50	50	51	52	52	52	51	50	50	50	50	51	N — Age.

The dark line across each column indicates the average height and weight for the respective years.

DE L'IRRITATION CÉRÉBRALE CHEZ LES JEUNES ENFANTS, ET SURTOUT CHEZ LES ENFANTS À LA MAMELLE. DES DANGERS AUXQUELS ELLE EXPOSE LES BEBES. DE SA CURABILITÉ ET DE SON TRAITEMENT.

ON CEREBRAL IRRITATION IN YOUNG CHILDREN, ESPECIALLY IN CHILDREN AT THE BREAST. THE DANGERS TO WHICH IT EXPOSES THE BABIES. ITS CURABILITY AND TREATMENT.

ÜBER HIRNREIZ BEI JUNGEN KINDERN, HAUPTSÄCHLICH BEI SÄUGLINGEN. DIE GEFAHREN, DENEN ER DIESELBEN AUSSETZT. SEINE HEILBARKEIT UND BEHANDLUNG.

PAR LE DOCTEUR JULES SIMON.

Médecin de l'Hôpital des Enfants, à Paris.

Je me propose, dans la très-courte communication que j'ai l'honneur de présenter à la Section des maladies des enfants, d'attirer l'attention des praticiens sur un état névropathique spécial *l'irritation cérébrale* des jeunes enfants et surtout des enfants à la mamelle qui me parait être, non seulement sous la dépendance de l'hérédité, de l'al-

Digitized by Google

coolisme, et de la syphilis des parents, mais encore sous l'empire des excitations et des écarts de régime et d'hygiène auxquels on abandonne les bébés.

Cet ébranlement nerveux, qui n'est pas intermittent comme les signes d'un tempérament nerveux, qui ne s'accompagne ni de fièvre, ni de paralysie, ni de contracture comme dans la lésion du système nerveux, est caractérisé par une excitation permanente de l'encéphale, des sens, et des actions réflexes.

Capable de saisir l'enfant dès le berceau, il prépare le terrain au développement de la sclérose, de la méningite, et de l'épilepsie.

Quand elle frappe les bébés, cette irritation maladive le rend excitable par toutes les sensations. Le bruit, la lumière l'agitent au suprême degré. Le sommeil est léger fréquemment interrompu ; l'appétit capricieux, le développement lent, irrégulier, et les actions réflexes exagérées au point que des vomissements, des soubresauts, des convulsions éclamptiques se produisent au moindre prétexte. L'intelligence parait vive, et, chez des enfants de six mois, le visage, la mimique expriment des sentiments au-dessus de leur âge, mais sous coordination ensuite.

Doué, parfois, d'une précocité effrayante, le bebé irrité cherche à traduire la crainte, la joie, l'affection, tout en étant animé de mouvement perpétuels. Son corps, ses membres s'agitent sans cesse, sans laisser à l'enfant le calme, la certitude du nouveau né bien équilibré. Il semble en proie à des excitations internes, perpétuelles, et il subit des actions réflexes hors de proportion avec la cause apparente qui les a fait naître.

Quand l'enfant a dépassé deux à trois ans, il est plus facile encore d'analyser ces phénomènes étranges que les gens du monde attribuent à une origine privilégiée, ou à l'évolution dentaire.

L'enfant remue sans trève en repos, le regard inquiet, le visage indifférent. Il est en quête de connaître tous les objets qu'il vient de toucher déjà, et de voir avec une sorte de curiosité, mais dont il a oublié à l'instant les caractères qu'il cherche de nouveau à retenir. La mémoire est presque nulle, le langage discordant, le caractère irritable, et l'intelligence qui parait vive par certains côtés, n'est capable ni d'application ni de développement. Aussi, même quand elle se manifeste par des signes indéniables, a-t-elle toujours de la tendance à s'amoindrir avec le temps.

Cette irritation cérébrale du nouveau-né et de l'enfant plus avancé en âge peut apparaître dès les premiers mois de l'existence ; elle se déroule ainsi pendant plusieurs années avec des périodes d'accalmies ou d'aggravation, et se termine vers l'âge de cinq ans, ou par la guérison, ou le plus souvent par la sclérose cérébrale, l'épilepsie, ou la méningite.

L'hérédité, l'alcoolisme, la syphilis des parents en sont les causes les plus fréquentes. Je possède sur cette question des documents absolument certains, mais il est une autre cause toute moderne, toute contemporaine que je tiens à mettre en évidence. C'est le déplorable élevage des enfants à la mamelle. Au lieu de les laisser dans l'isolement de la nourricerie, ou en fait un objet de distraction, on les excite par le bruit, le chant, les lumières, et quelquefois aussi par des boissons excitantes, (café, thé, spiritueux) qu'ils prennent directement à la table commune ou avec le lait de la nourrice mercenaire. L'agitation fébrile qui se répand autour du berceau de l'enfant, dans les sociétés modernes, se répercute dans ces cellules cérébrales, y communique une vibration continue, et traduit un ébranlement maladif qu'il est malaisé de calmer par la suite. Le médecin a fort à faire quand il cherche à s'opposer à ces pratiques déplorables. Les efforts se heurtent aux habitudes contractées dans la lutte pour la vie.

Son devoir est cependant d'instituer une hygiène spéciale et sévère, et un traitement anti-nerveux.

Dès les premiers symptômes il faut s'appliquer à supprimer toutes les sources d'excitation ! Le bruit, la lumière, les réunions de famille, les aliments excitants. La

Digitized by Google

rie au grand air, lourd des grandes villes, du bord de la mer contribuera à ramener le calme dans ces cervelles irritées.

À cette médication, on ajoutera l'emploi du bromure à la dose de 0.10 à 0.17 centigramme chez les enfants au dessous d'un an; à celle de 0.30 centigramme chez l'enfant d'un an; à celle de 0.60 entre un et de deux ans. À partir de cet âge, c'est avec un gramme et plus qu'il faut opérer. À condition toutefois de suspendre le médicament au bout de trois jours, pour le reprendre trois jours après.

Grâce à cette hygiène, et à ce traitement bromuré, aidé des laxatifs où arrêtera quelquefois une disposition fâcheuse à contracter des névroses, et des lésions cérébrales.

En résumé, mon but a été de démontrer qu'il existe chez l'enfant, et même chez l'enfant à la mamelle un état névropathique qui précède les altérations du système nerveux, et qui peut être souvent mis sur le compte de l'hérédité, de l'alcoolisme et de la syphilis, mais qui peut prendre la source dans le genre de vie de la famille moderne, et dans l'inobservation des lois les plus élémentaires de l'alimentation des nouveau-nés dont le développement régulier exige le calme, et l'absence d'excitation du cerveau, des sens, du système nerveux en général.

[ABSTRACT.]

CEREBRAL IRRITATION IN YOUNG CHILDREN, AND ESPECIALLY IN INFANTS AT THE BREAST.

BY DR. JULES SIMON,

Physician to the Hôpital des Enfants Malades, Paris, France.

I propose, in the very short communication which I have the honor to present to the Section of Diseases of Children, to draw the attention of practitioners to a special neuropathic state (cerebral irritation) of young children, and particularly of infants at the breast, which appears to me to be not only due to heredity, alcoholism or syphilis of the parents, but, in a still greater degree, to be caused by nervous excitation and the absence of proper regimen and hygiene.

This nervous disorder, which is not intermittent, like the symptoms of a nervous temperament, which is accompanied by neither fever, paralysis nor contraction, such as occurs in injuries of the nervous system, is characterized by permanent excitement of the brain, the senses and reflex actions. Commencing in the child even in early infancy, it prepares the way for the development of sclerosis, meningitis or epilepsy. When it seizes the infant, this unhealthy irritation makes it excitable to all sensations; noise and light agitate it to a supreme degree; the sleep is light and frequently interrupted; the appetite is capricious; the development slow and irregular, and reflex actions are exaggerated to the point of vomiting, of subsultus, of eclamptic convulsions. These reflex actions are produced by the slightest exciting causes. The intelligence seems active, and in infants of six months the face has the expression of sentiments beyond its age, but without coördination. In short, endowed sometimes with an

Digitized by Google

alarming precocity, the excited infant tries to express its fear, its joy, its affection, and its endeavor is attended by perpetual movement. The body and limbs are agitated without ceasing, not allowing the infant the rest and calmness which characterize the healthy equilibrium of the new-born. It seems a prey to extreme and constant excitement, and suffers reflex action out of proportion to the apparent cause producing it.

When the child has passed beyond the second or third year, it is more easy to analyze these strange phenomena, which the laity generally attribute to a special origin (origine privilégiée) or to dental irritation. The child moves without respite in sleep; the look is inquiet, the countenance indifferent. It is eager to investigate the objects which it has already handled, and views them with a sort of curiosity, but forgets their characters in the instant that it seeks for something new. The memory is almost *nil*, the language discordant, the character irritable, and the intelligence, which seems bright in certain directions, is capable neither of application nor development. Even when it manifests itself by undeniable signs, it always has a tendency to grow less by time.

This cerebral irritation of early life may appear in the first months of existence; it can also develop itself during several years, with periods of arrest and exacerbation, and it terminates toward the age of five years in recovery, or, more often, by cerebral sclerosis, epilepsy or meningitis. Heredity, alcoholism and syphilis of parents are the most common causes. I possess on this point statistics absolutely certain; but there is another cause, entirely recent, entirely cotemporaneous with the child, which I desire to notice. It is the deplorable management of children at the breast. Instead of leaving them in the quietude of the nursery, they are distracted and excited by loud noises, as singing, bright lights, and sometimes by exciting drinks, as tea, coffee and spirits, administered undiluted or mixed with the milk of the nurse. The feverish excitement which surrounds the cradle of the infant in modern society reflects itself in the cerebral cells, produces a continuous vibration, and causes an unhealthy disturbance, which, in the end, is with difficulty overcome. The physician has much to do when he endeavors to oppose these deplorable practices. His efforts are opposed to habits contracted during the life-work of the nurse. It is his duty, nevertheless, to secure a special and strict hygiene and a treatment anti-nervous. From the first symptoms it is necessary to endeavor to suppress all the sources of excitement—noise, light, family gatherings and stimulating nutriment. Life in the open air, at a distance from the great cities, at the seashore, contributes to restore tranquillity to the irritated brain. To this treatment may be added the use of bromides, in the dose of 10 to 15 centigrammes (1½ to 2¼ grains) for a child under one year, 30 centigrammes (4½ grains) for a child of one year, and 60 centigrammes (9 grains) for a child between the ages of one and two years. For children above this age we may prescribe a gramme (15 grains) or more, on condition always that the medicine be suspended after three days, to be resumed three days subsequently.

Thanks to this hygiene and to the bromide treatment, aided by laxatives, we are enabled, sometimes, to check the dangerous predisposition to contract the neuroses and cerebral lesions.

To recapitulate: my object has been to show that there exists in the child, and even in the infant at the breast, a nervous state, which precedes anatomical alterations in the nervous system, and which can often be attributed to heredity, alcoholism or to syphilis, but which may also originate in the mode of life of modern society and in disregard of the most elementary laws relating to the nutrition of the infant, whose normal development requires quietude and the absence of excitement of the brain, the senses and the general nervous system.

DISCUSSION.

Dr. S. H. CHARLTON, of Indiana, said:—Although agreeing with the opinion of the distinguished French physician whose paper we have listened to, that cerebral

Digitized by Google

irritation in the infant may result from heredity, particularly from alcoholism or syphilis in the parent, I am of opinion that the anti-hygienic conditions which surround the child are the common exciting causes. An indigestible and irritating diet, prolonged atmospheric heat, noise and excitement, the lack of sufficient sleep, may be mentioned among the causes which disturb the nervous system and produce cerebral irritation, even when there is no history of syphilis or alcoholism in the parents. According to my observations, cerebral irritation of the infant is more common in midsummer, when the air is impure from the presence of marsh miasm and noxious vapors, than at other seasons of the year.

Remarks by the President.—The paper by M. Simon, to which we have listened, relates to a matter of great importance, and one which is not understood by a large proportion of experienced physicians. That the state which the writer designates cerebral irritation is common in the infant all will admit. Fretful, wakeful infants, with limbs constantly in motion when not asleep, tremulous and excited by sudden noises or the approach of a stranger, having sudden jerking or twitching of the muscles, premonitory of eclampsia, from slight causes—such infants are often met in the American household. Frequently, indeed, in such infants eclampsia occurs from slight exciting causes. The old explanation of the etiology of these nervous symptoms, that they are due to dentition or the presence of worms, though it may satisfy the mother or nurse, has been long discarded by intelligent physicians. The gum lancet and vermifuges do harm to the great majority of these infants, increasing the cerebral excitement, and they are no longer employed by intelligent physicians. I accept M. Simon's opinion, that the cause of the cerebral irritation may be inherited. I think that it may sometimes be traced back to alcoholism, syphilis, privations, great mental worriment or excitement, or irregularities in the parents; but more frequently the cause of the cerebral excitement exists in the present bad management of the infant. The habits of modern society interfere with the necessary quietude of the nursery, and irregularities in the nursing or feeding or sleep are quite sufficient to cause cerebral irritation in the delicate organism of the infant.

The fact, mentioned by M. Simon, that cerebral irritation in the infant, attended at first by no structural lesions, may, if it continue, end in grave central disease, as sclerosis or meningitis, should be known in every household. For many years, when called to a patient exhibiting the symptoms so clearly and forcibly described by M. Simon, I have been in the habit of prescribing one of the bromides, usually that of potassium, and I am sure that I have averted many times, by the use of this agent, eclampsia, if not more serious disease. In all those cases of cerebral excitement or irritation, the bromide, with hygienic measures, has seemed to me to be the sovereign remedy. It will relieve cerebral irritation, when not dependent on organic disease, as quickly and surely as quinine relieves the diseases due to marsh miasm, but it is occasionally best to use, in addition, cool lotions to the head and a warm foot bath. It has been my practice to prescribe two to three grains of the bromide of potassium to an infant of six months, and four grains to one of twelve months, every two to four hours, until the symptoms abated. The larger dose employed by M. Simon is to be noted, but I presume he does not administer it so often. His recommendation to employ it in periods of three days, with intervals of three days, is, I presume, to avoid bromism. If the views expressed by M. Simon were widely disseminated, and the measures recommended by him generally followed, the nervous diseases in early life would certainly be less frequent.

Digitized by Google

LA DILATATION PRÉPUTIALE. L'IGNIPUNCTURE DES AMYGDALES.

PREPUTIAL DILATATION. IGNIPUNCTURE OF THE TONSILS.

DIE DEHNUNG DER VORHAUT. DIE IGNIPUNKTUR DER MANDELN.

PAR LE DOCTEUR DE SAINT-GERMAIN.

Chirurgien de l'Hôpital des Enfants, Paris, France.

Si l'on doit accorder un grand mérite au chirurgien qui invente une opération, on ne saurait refuser un très-petit mérite à celui qui en supprime deux du manuel chirurgical pour leur substituer deux procédés moins dangereux, tout aussi sûrs et d'une exécution plus facile.

C'est à ce titre que j'ai l'honneur de présenter au Congrès l'exposé de ma pratique pour ce qui touche à la dilatation préputiale et à l'ignipuncture des amygdales.

La circoncision m'ayant parfois donné des accidents tels que hémorrhagie difficile à arrêter, gangréne partielle, diphthérie de la plaie, j'y ai à peu près complètement renoncé, et la réserve pour les cas vie la dilatation est impraticable, (ces cas sont dans la proportion de un sur 300.)

Je n'emploie que la dilatation. Cette opération imaginée par Nélaton, reprise depuis par la plupart des chirurgiens consiste dans l'introduction, dans l'orifice préputial d'une dilatation à deux, et non à trois branches comme le pratiquait Nélaton, et dans la divulsion graduelle lente de l'orifice.

Cette opération qui se complète par le décollement des adhérences à l'aide de la sonde canelée et par le massage quotidien obtenu par le décalottement du gland et son recalottement m'a donné les résultats les plus satisfaisants et les plus durables. Quant à l'ignipuncture des amygdales, elle est destinée à remplacer avec avantage l'amygdalotomie. Ces accidents de l'amygdalotomie peuvent être mortels, et pour ne citer que l'hémorrhagie incoercible, l'envahissement de la plaie par la diphthérie, on voit que cette opération n'est pas aussi anodine qu'on pourrait le croire.

Krishaber avait déjà essayé de lui substituer la cautérisation à l'aide du thermocautère ; mais son mode de procédé tout superficiel éternisait, pour ainsi dire, la durée du traitement.

Aussi, ai-je imaginé d'introduire à l'aide du bâillon de Smith modifié, le thermocautère en forme de crochet pointu, et l'enfermant dans chaque tonsille à un centimètre environ.

Cette opération répétée tous les huit jours, comprend de deux à quatre séances ; au bout de ce temps on voit les tonsilles évidées, ratatinées ne plus présenter qu'un petit moignon insignifiant. Je n'ai jamais eu d'accidents par ce procédé, et je ne compte que des succès. Si, en conséquence, on considère la fréquence des deux opérations susdites, ainsi que le nombre de guérisons obtenues, sans que le malade ait couru le moindre danger, on est en droit de se demander si la double substitution adoptée n'est pas un progrès chirurgical appréciable.

Digitized by Google

[ABSTRACT.]

PREPUTIAL DILATATION. IGNIPUNCTURE OF THE TONSILS.

BY DOCTOR DE SAINT-GERMAIN,

Surgeon to the Hôpital des Enfants, Paris, France.

Since we accord great merit to a surgeon who invents an operation, we cannot refuse a little merit to him who substitutes for two surgical operations, two methods of treatment less dangerous, quite as sure, and more easy of performance.

In virtue of this right, I have the honor to present to the Congress an account of my practice relating to preputial dilatation, and ignipuncture of the tonsils.

PREPUTIAL DILATATION.

Since circumcision is sometimes followed by accidents, such as hemorrhage, difficult to control, partial gangrene, diphtheria of the wound, I have almost entirely given it up, and reserve it for those cases in which dilatation is impracticable (these cases are in the proportion of one in 300).

I employ only dilatation. This operation, devised by Nélaton, and since adopted by the majority of surgeons, consists in the introduction into the preputial orifice of a dilator of two branches, and not three, as practiced by Nélaton, and in the gradual and slow dilatation of the orifice.

This operation, which is completed by separating the adhesions by the aid of a grooved director, and by daily movement of the prepuce, by which the glans is alternately uncovered and covered, has given me the most satisfactory and durable results.

IGNIPUNCTURE OF THE TONSILS.

As to ignipuncture of the tonsils, it is destined to replace with advantage tonsillotomy. The accidents of tonsillotomy may be fatal. Not to cite uncontrollable hemorrhage, the occasional invasion of the wound by diphtheria makes it clear that the operation is not so harmless as we may have thought.

Krishaber had already endeavored to substitute in place of this operation cauterization by the aid of the thermo-cautery, but his mode of proceeding, too superficial, rendered his treatment, so to speak, perpetual. Therefore, it occurred to me to introduce, by the aid of the modified Smith's gag, the thermo-cautery in the shape of a pointed hook, and thrust into each tonsil to the depth of about a centimetre.

This operation, repeated every eight days, requires from two to four sittings. At the end of this time the tonsil will be found evacuated and shriveled up, presenting only a little insignificant stump. I have never had any accident from this process, and I only enumerate successes.

If, consequently, one considers the frequency of the two operations mentioned above, and also the number of cures obtained, without the patient incurring the slightest danger, he has the right to ask if the double substitution adopted is not an appreciable surgical improvement.

DISCUSSION.

REMARKS BY THE PRESIDENT.—Of two operations for the relief of the same ailment, the one that is attended with less pain, less mutilation of parts, less liability to accidents or complications is, of course, to be preferred if the result be equally good.

Digitized by Google

That circumcision has resulted disastrously in not a few instances is well known. Ordinarily a simple and safe operation, it may result badly through carelessness or untoward circumstances. In a recent number of the *British Medical Journal*, Mr. Phillips states that a child of thirteen months was circumcised; the same instruments being used that were employed a few hours previously for tracheotomy in a case of diphtheritic croup. Four days later the pseudo-membrane appeared upon the prepuce, producing much infiltration and œdema, and retention of urine. In the Liverpool *Med.-Chir. Jour.*, January, 1887, Dr. E. Zula Greves relates that a boy, who had been circumcised for phimosis, was admitted into the Liverpool Infirmary with an unhealthy-looking prepuce that had not healed after the operation. Weak and anæmic at the time of admission, he continued to sink, and died of cardiac failure. The wound and subjacent tissues were infiltrated with micrococci, like those in the pharyngeal pseudo-membrane. The fact that hemorrhage, diphtheria, and other forms of blood contamination, may result from circumcision, even if it be through carelessness, constitutes an argument against the operation, as Saint-Germain has said. An important part of the treatment of phimosis, without which it is incomplete and unsuccessful, is the removal of the preputial mucous membrane from the surface of the glans, and the washing away of the smegma, which, acting us a foreign substance, produces a low grade of inflammation, a balanitis. The removal of this smegma and inflammation is essential to recovery, and, according to my observations, the agglutinated mucous surfaces can be more readily separated if the foreskin remain attached to the mucous layer underneath; in other words, if dilatation be practiced rather than circumcision.

The foreskin in its normal state is very distensible. It can be slowly stretched at one or two sittings, so as to slip over the glans, even when the orifice was previously a pinhole. Saint-Germain says that circumcision is not required for phimosis in more than one case in three hundred; dilatation being employed instead. Our experiences in the Out-door Department at Bellevue correspond with his, although we have not preserved statistics. I have employed for dilating the prepuce, with the best results, the long, slender forceps used for removing foreign substances from the ear. When closed, these forceps are blunt-pointed and scarcely larger than a probe. The foreskin being drawn forward from the glans and the point of the forceps pressed into the orifice, and the child firmly held in a horizontal position, dilatation can be accomplished in a few minutes, without apparently much pain, by gradually and slowly separating the blades. When the dilatation is sufficient, the next step is the retraction of the prepuce, which can be best accomplished by the fingers, dry, not oiled; but the two preputial surfaces are so agglutinated that I have in most instances employed a probe, grooved director, or similar instrument, to facilitate their separation.

Dr. Holgate, sixteen years physician to the class of children's diseases in the Out-door Department at Bellevue, states that he has treated hundreds of children with narrow prepuce and preputial adhesions, in which symptoms were so pronounced that surgical interference was required, and in no instance has he found it necessary to circumcise or use the knife, even when the entire prepuce was adherent. He uses no instrument but the probe. The patient being firmly held in a reclining position, the prepuce is pressed backward by the thumb and forefinger of each hand until the glans begins to protrude. The adhesions are then separated by the blunt end of an ordinary silver probe, which is carried in a circular direction around the glans. By the backward pressure of the foreskin the glans protrudes more and more and is soon uncovered. The parts are then oiled, the smegma removed, and the foreskin returned. Every day or second day during the ensuing week the glans is

Digitized by Google

uncovered, oiled, and the foreskin replaced. The first operation, Dr. Holgate says, does not occupy more than three minutes. We have, therefore, in the children's class in the out-door department pursued essentially the same treatment of phimosis as that recommended by Saint-Germain.

Dr. A. D. ROCKWELL, of New York, the celebrated electro-therapeutist, has kindly consented to add the following remarks on Saint-Germain's paper relating to ignipuncture of the tonsils:—

The author's description of his method of ignipuncture of the tonsils is too brief and vague to render possible any satisfactory discussion of the subject.

I take it, however, that the idea intended to be conveyed is, that the two hooks (platinum, of course, although the kind is not mentioned), after having been thrust into both tonsils, are attached to a galvano-cautery apparatus, and then heated to a white heat. From personal experience I cannot speak of this method in its application to the tonsils, but only as I have attempted it in small external growths, and from the common knowledge that we all possess as to the destructive powers of heat. Its action upon these various external growths, vascular and non-vascular, in causing them to wither and shrivel away, renders the statement of Saint-Germain in regard to its effects upon enlarged tonsils sufficiently plausible, and coming from such a reliable source, the operation ought to be well tested. Especially is this the case when we consider certain positive advantages that the galvano-cautery possesses over the knife, in that it saves all, or nearly all, hemorrhage, leaving but little pain after the operation, besides being less liable to be followed by pyæmia.

We might expect that ordinary electro-puncture would prove of service in reducing enlarged tonsils, not only through the actual disintegration of tissue that takes place, but through that well-known process of absorption that is set up and continues for a long time after the cessation of the electrolytic action. It is doubtful, however, whether children could be induced to tolerate its use, since it is very painful, and each seance would have to be five or six minutes in duration in order to obtain effects.

Ignipuncture, on the contrary, is almost instantaneous in its action, and causes much less pain.

If further experience is favorable to the feasibility of the operation so far as it relates to the readiness with which it can be performed on children, and its efficiency in reducing the hypertrophied tonsils, the two advantages it possesses over tonsillotomy, viz., freedom from possible hemorrhage and far better healing, are of no small importance.

REMARKS BY DR. ROBERT NEWMAN.—I have had some experience in treating enlarged tonsils by thermo-cautery, and the result has been satisfactory. Success in treating the hypertrophied prostate by this agent led to the belief that enlargement of the tonsil might be reduced by the same treatment, since there is similarity in the structure of the two organs. The approval of so high an authority as Saint-Germain increases the confidence in the operation. My experience has shown that a child of a tender age will go through the operation without flinching and without complaint of pain, and he opens his mouth and submits to subsequent operations with more confidence than at first.

The instrument used by me has the shape of a catheter, of smooth, polished metal, with a short curve. At the end beyond the curve is a fenestrum containing the platinum wire to be heated. The other straight end of the instrument, forming the handle, contains two distinct metal pins for connection with the electrode cords of

Digitized by Google

the galvano-cautery battery. The current breaker is attached to the handle, and by pressing it down by the finger electricity is evolved and the burner instantaneously heated, giving a sudden light which illuminates the fauces. A white film forms upon the tonsil. With this superficial cauterization the operation may be repeated in two or three days. In one case I did it on the following day. With the deeper cauterization performed by the instrument of Saint-Germain, of course longer intervals between the operations are required. The deeper and more effective burning of the tonsil by Saint-Germain would require intervals of a week. This treatment diminishes the tonsils until they return to their normal size, without, so far as I know, any ill effect in any case in which it has been practiced. My experience leads me to agree fully with the opinion of Saint-Germain.

ON THE DELETERIOUS RESULTS OF A NARROW PREPUCE AND PREPUTIAL ADHESIONS.

SUR LES RÉSULTATS DÉLÉTÈRES D'UN PRÉPUCE ÉTROIT ET DES ADHÉRENCES PREPUTIALES.

DIE SCHÄDLICHEN FOLGEN EINER ENGEN VORHAUT UND DER PRÄPUTIALEN ADHÄSIONEN.

BY LEWIS A. SAYRE, M.D.,

Professor of Orthopædic Surgery in Bellevue Hospital Medical College, New York.

In 1870 I published my first paper on this subject in the *Transactions* of the American Medical Association, under the title of "Partial Paralysis from Reflex Irritation Caused by Congenital Phimosis and Adherent Prepuce," illustrated by cases.

This paper produced a marked impression on the profession, and was extensively commented on by various medical journals both at home and abroad.

D. Campbell Black, M.D., F.R.C.S., Edinburgh, in his very valuable work "On the Functional Diseases of the Renal, Urinary and Reproductive Organs," published in 1872, quotes very extensively from this paper, and concludes by saying, on page 213: "I offer no apology for thus giving considerable prominence to the foregoing cases. I attach to them immense importance as disclosing, possibly, a frequent source of infantile paralysis, and the numerous indications of nervous irritability in childhood, while, as far as known to me, Dr. Sayre's cases are unique in medical literature."

I, therefore, feel that I can justly claim to have been the first to draw the attention of the profession to this important subject.*

In 1875 I published a more elaborate paper on the same subject in the *Transactions* of the American Medical Association for that year, entitled, "Spinal Anæmia, with Partial Paralysis and Want of Coördination, from Irritation of the Genital Organs."

This paper was also illustrated by a number of cases which had been relieved of

---

* I have recently been informed that Mr. Stanley published on this subject in the *Medico-Chirurgical Transactions*, Vol. XVIII, p. 260, 1833. But on referring to his paper I find he reports some cases of *reflex* paralysis from abscess of kidney, which had been supposed to depend on disease of the vertebræ and spinal cord, but which were found healthy on post-mortem examination, and I find no case reported from congenital phimosis.

Digitized by Google

their paralysis, and various other nervous symptoms, including want of coördinating power, etc., by simply removing the *constriction* from the glans penis, and the retained and concrete smegma from behind the corona, and so arranging the prepuce that it could glide *easily* to and fro over the glans without any constriction, thus permitting the parts to be kept perfectly clean, and thereby removing one great source of irritation.

In this proper arrangement of the prepuce, it became necessary in some cases to perform circumcision, or an actual removal of a small portion of the prepuce, and in some cases to dissect it from its attachments to the glans, which were not simply by agglutination, which is the normal condition, but the attachments were by actual *adhesion*, an abnormal condition, and entirely different from the normal agglutination. Unless there is a great redundancy of the prepuce with constriction of the elongation there is no occasion to remove any of the tissue; but in the great majority of the cases the object to be obtained can be easily accomplished by simply passing a grooved director into the narrow orifice of the prepuce, gliding it between it and the glans, with the groove toward the prepuce, and pressing it with the thumb and finger as far back as possible, glide into the groove a pointed curved bistoury, and divide the contracted tissue sufficiently to enable you to tear it back and uncover the glans; then a slight nick with the scissors or bistoury, through the thickened fold of the edge of the frenum; after which you can, with the thumbs and forefingers of the two hands, easily tear down the frenum and other adhesions so as to expose the entire glans, and in the sulcus behind the corona will often be found a hardened smegma, sometimes containing chalky concretions, which have been one of the sources of the irritation, and which can be readily wiped away; the whole operation can thus be completed without the sacrifice of any tissue, and with very little loss of blood.

In some cases it may be advisable to take a stitch on either side of the incision, between the integument and mucous membrane; in others no stitching is required; but in either case the foreskin is carried well back, uncovering the glans, and a little styptic cotton, or a rag wet in persulphate of iron, is wound around the wound and left in situ for eight or ten days, when the wound will be entirely healed, and the organ in a normal condition, with the glans partially covered with its prepuce, but which can be easily uncovered, as it should be, and not mutilated and disfigured, as I have frequently seen it, by a too free removal of the prepuce, thus leaving the glans entirely unprotected.

While, therefore, I may be responsible for bringing this subject so prominently before the profession, I wish to raise my voice in protest against this unjustifiable mutilation, as well as against the indiscriminate performance of the operation in cases where it can be of no avail.

This unnecessary mutilation in some cases, and the unwise and injudicious promises of great relief in others, where it can do no good, is likely to bring the operation into disrepute, and thus deprive the cases that might be benefited by its performance of receiving that relief which in many cases it certainly will afford.

While some over-zealous advocates have recommended and performed the operation of circumcision in all cases of infantile paralysis, even those dependent on some central lesion of either the brain or spinal cord, where it can do no good, others have gone to the opposite extreme, and deny the existence of a paralysis, or even muscular incoördination from *reflex* genital irritation.

The object of this paper is to harmonize these two extremes, and show that there are some cases of very *anomalous* and *extraordinary nervous manifestations* entirely dependent upon some irritation of the genital organs, in which an operation is not only justifiable, but absolutely demanded, and that in many instances the relief from all the strange symptoms has not only been immediate, but permanent after the operation, without any other medical or surgical treatment.

Digitized by Google

While I am convinced that the observation just made is absolutely correct, I am also equally well convinced that any attempt to relieve a nervous disturbance by an operation on the genital organs, when the cause of that nervous difficulty was some organic change in the brain or spinal cord, would result in no benefit whatever, and in such cases an operation is unjustifiable and should be condemned.

I shall not in this paper refer to any of the numerous cases that have been under my own personal observation and treatment for the past 15 years, although they have been numerous and many of them exceedingly interesting, fully corroborating all the statements made in my two original papers, but I shall take only a few of the cases for illustration from the numerous reports that have been made to me by various physicians and surgeons in different parts of the country, thus showing that other competent observers have arrived at similar conclusions with myself, and have had the same satisfactory results in practice, when they have operated upon properly selected cases.

The first case is from the distinguished Professor of Surgery in Jefferson Medical College, and is a beautiful illustration of the rapid improvement that sometimes occurs :—

1112 WALNUT ST., PHILADELPHIA, February 7th, 1882.

DEAR SAYRE :—

The case to which Lewis* heard me allude in my clinic last December, was that of a child two years of age, whose gait simulated that of locomotor ataxia, being uncertain, precipitate, irregular and jerking. There was no paralysis of any muscles; but the trouble seemed to be due to defective coördinating power over the muscles. On the second day after removing a contracted and adherent prepuce, the trouble had entirely disappeared and I found my little patient running about the room. Yours truly,

S. W. GROSS.

The following case from Dr. A. R. Mott, Jr., Resident Physician of Randall's Island, is a good illustration of the injurious effects upon the nervous system by girdling or strangulation of the glans penis, and the *immediate* relief afforded by the removal of the same.

The results are the same upon the nervous system whether the partial strangulation is from an adherent and contracted prepuce, or from a string tied around the organ.

RANDALL'S ISLAND, January 18th, 1880.

DEAR DOCTOR :—

I have been unable to find the report of the case of which I spoke to you on Friday, but know it was made in the Hospital *Gazette* of January, 1879.

The following, however, is an outline of the history, taken from the case book of the Workhouse Hospital on Blackwell's Island, of which I was at the time House Physician.

Very respectfully, A. R. MOTT, JR., M.D.

To LEWIS A. SAYRE.

CASE.—*Ino English*, age forty-six, native of England, widower, clerk; admitted to Workhouse Hospital (B. I.) Dec. 23d, 1878. Patient had been at work for a week, as a prisoner. On the 23d was noticed to be restless and uneasy, and finally, in the evening, he fell from his bunk in a fit. During the next forty-eight hours he had several (eight or nine) convulsions, and during the intervals lay in a semi-comatose condition, showing no consciousness except to stir a limb when pinched. Pulse 120, temperature 101½°, respiration 18—swallowed nothing and passed fæces in bed. Continued in this condition to evening of December 25th (temperature having fallen to 100), when a string was discovered passed twice around the penis behind the corona and tied, the long prepuce serving to conceal it from observation. While not sufficiently tight to occlude the urethral canal, still a firm indurated band remained after the string was cut, and did not disappear for four or five days.

* My son, who had been present at his lecture.

Digitized by Google

Within one hour after the removal of the string the man sat up and asked for milk, and from this time remained perfectly well (was under observation for three months).

He declared that he remembered nothing that had taken place during the past three days—had never had "fits," denied venereal, was moderately addicted to drink, but had led a "virtuous life since the death of his wife, two years before."

Though seemingly healthy, there was nothing to indicate incorruptible virtue or veracity.

The following case from Dr. James S. Green, a very distinguished physician of Elizabeth, N. J., is exceedingly interesting as showing how rapidly in some instances cases of almost complete paralysis are at once restored to nearly full muscular power.

342 Union Avenue, Elizabeth, New Jersey.

My Dear Doctor:—Of a number of cases of reflex paralysis upon which I have operated and performed circumcision, I desire to send you the account of one demonstrating remarkably the rapid curative effect of the operation. Some time in April last a boy, fourteen years of age, was brought to me, supposed by his friends to be suffering from chorea.

There was intense hyperæsthesia of the skin of the whole body, very marked want of coördination of motion in the arms and hands and great difficulty in walking.

The youth was so uncertain in his gait and had fallen so frequently, that he was afraid to attempt the act alone. He had been suffering in this way for three years, becoming gradually worse, and was mentally below the average of boys of his age.

Having taken medicine in large quantity and sugar of milk adulterated infinitesimally for a long while, his attendants were surprised, after I examined his penis, with my opinion that circumcision would cure him. The operation was performed that afternoon; the next morning the boy was relieved of his hyperæsthesia, and in forty-eight hours he recovered entirely the use of his limbs.

He is now at school, which he had not been able to attend in three years, and was so altered in appearance when I met him on the street last week that I scarcely knew him. I have the notes of a number of other similar cases, less striking in their features and rapidity of cure, but all demonstrating the correctness of the views you hold of the pathology of the disease.

Very truly yours,

Dr. Lewis A. Sayre. James S. Green.

The next case is one of paralysis, and reported to me by Dr. Wirthington, of Livermore, Pa.

PARALYSIS FROM PHIMOSIS.

Livermore, Pa., Dec. 19th, 1881.

Dr. Lewis Sayre:—I have often thought of giving you the history of a case of paralysis from congenital phimosis, being a parallel to your "Pee-Pee Case" (page 14).*

After reaching the house I found a beautiful boy (except he looked pale and worn out) of whom it was said he never was sick.

He was three years old and had been paralyzed in his lower extremities for six months previous to my seeing him. The doctor who had attended him had used all the ordinary drugs given in such cases, and in full accord with his diagnosis. Meeting the father of the boy one day, he asked me for an opinion as to the cure of the child. Of course he had stated the fact of his boy being paralyzed. Not feeling at liberty to venture an opinion, I was asked to see him. The "Pee-Pee Case" made it an easy matter to diagnosticate, and I told the father, accordingly, that his child could be cured, and that he would require no medicine. After the parents and grandmother had held their consultation, I was called to take the case. The next day, October 1st, 1878, I operated on the boy, after first putting him fully under the influence of ether.

The prepuce was adherent almost all the way around the glans penis.

Behind the corona was a solid cake of sebaceous matter. I never gave a single drop of medicine, and the case made a happy and rapid recovery. Before the sore was completely well, all symptoms of paralysis had disappeared. I am respectfully yours,

F. J. Wirthington, M. D.

* Referring to case reported in my Lectures on Orthopædic Surgery.

Digitized by Google

As I was anxious to know the ultimate result in this case, I wrote to the Doctor in about two months, and received the following reply :—

LIVERMORE, PA., February, 24th, 1882.

DR. L. A. SAYRE :—

Dear Sir:—I have given you the history of that child so far as I know, except one fact; the father tells me to-day that when the child was born he was considered the biggest and finest boy that had been born in the community for a long time. At the age of about two years he began to be contrary, fretful and peevish, etc., and would fall down without the slightest cause. This state of things went on for some time, until an M. D. was called in, who told them their child was paralyzed, and treated him with the usual nerve tonics and with electricity. Notwithstanding all this, the boy went steadily down, and the paralysis continued until I saw him, October 1st, 1878, when he was unable to walk. He was pale, lean and anæmic. The doctor who had examined him could find nothing which he thought caused the paralysis. I operated on this boy October 1st, 1877, and before the wound that I had made was healed, my little paralyzed patient could *walk*. He can walk, and has run and jumped ever since. He is hearty, and is the very picture of health.

I believe he began to improve as soon as he was operated upon, from the fact of his having passed his water in a round, full stream, a thing he had not done for months. It dribbled away, while he cried all the time.

He never took a particle of medicine, and has not to this day, February 24th, 1882. He is well, and has never had a sign of paralysis since.

Very respectfully yours,

F. J. WIRTHINGTON.

The following case from Dr. Leech, of Indiana, is interesting as illustrating the various nervous disturbances that can be caused by genital irritation, and the immediate relief afforded by a proper operation.

LEECH'S CASE OF PHIMOSIS.

ATTICA, FOUNTAIN CO., IND., August 31st, 1875.

DEAR DOCTOR :—

I send you the following report of a case, as you requested the members of the American Medical Association to do :—

Mr. John Maguire and wife, of our town, have but one child, a boy of fourteen months, who has been the terror of all that part of town for six months, as he cried constantly, except when asleep or nursed by his mother. He would lay perfectly still and squall, not showing any disposition to sit up, nor did he like to be raised up. He was very nervous, and would have times when his limbs would be rigid. This state of things grew worse until Wednesday last, August 25th, when the family physician was called in, and found the child with fever and suffering from great nervous excitement. He pronounced it a case of remittent fever, and gave medicine accordingly, but to no purpose, as the fever and excitement increased, and the child had spasms frequently, and particularly when his bladder needed emptying. At that time the child would strain and cry, giving evidence of great suffering, and had a tendency to prolapse of rectum. It would have several spasms at these times, which would come on rapidly, and they grew more severe at each succeeding time. Late Saturday night, August 28th, in the absence of the family physician, I saw the child, and found paralysis of the lower half of the body, not very marked, but sufficiently to be easily diagnosticated, and spinal irritation. I at once examined the penis, hoping to find a case of phimosis, and, sure enough, I did. I had the family physician called as soon as possible, and before leaving the house that night I introduced a small groove director into the prepuce and laid it open with a bistoury. This exposed the glans penis enclosed in a sheath of mucous membrane, the latter agglutinated to the gland so tightly that I had to use a sharp instrument to get under it, and even after I got a place started, I could hardly separate the two. I persevered, and finally exposed behind the corona glandis a roll of sebaceous matter, of a cheesy consistence and about the size of a cotton shoe string, and extending almost around the penis. I removed this, and after making a free incision of the deep mucous mem-

Digitized by Google

brane, I washed away the remaining débris and applied water dressings, and left the child in a few minutes almost asleep, not nearly so nervous, and seemingly in a condition of general well feeling. I used no internal remedies, and now, three days later, the child has made marked improvement, and bids fair to make a rapid and complete recovery. The glans, while in the grasp of the mucous membrane, was about one-third its natural size, and I think the contracted scar tissue, the result of inflammation, was the cause of the whole trouble.

If you desire, will report the progress of the case.

Very truly yours,

DR. L. A. SAYRE. T. F. LEECH, M.D.

ATTICA, FOUNTAIN CO., IND., December 1st, 1875.

DEAR DOCTOR:—

The boy I wrote you about in August last has remained perfectly well ever since the operation. Has had no spasms or other nervous disturbance, and runs and jumps as active as any boy of his age.

Very truly yours,

T. F. LEECH, M.D.*

The following interesting case of phimosis, with reflex effects, chorea, etc, is from J. H. Pooley, M.D., Columbus, Ohio, Professor of Surgery, who writes:—

Some years ago I was consulted with regard to a child about a year old, who presented some curious nervous symptoms. He was a large, finely developed boy, and until three months or so before my seeing him had always been in perfect health.

His condition was one of localized chorea, manifesting itself in constant convulsive movements of the head. They were nodding, or antero-posterior movements, alternating with lateral or shaking and twisting motions. By the time I saw the patient these convulsive movements had become almost constant during his waking hours; there was no grimace or distortion of the features, no choreic movements of the extremities; indeed, the whole affection consisted in the nodding and shaking movements of the head referred to. These were almost incessant, sometimes slow and almost rhythmical, then for a minute or two rapid and irregular, seeming to fatigue the little fellow, and accompanied by a fretful, whimpering cry.

The child had been subjected to a variety of treatment, but without any benefit or effect of any kind. Upon the most careful examination of the patient and his history and antecedents, I

---

* I wrote to Dr. Leech in July, when I was preparing this paper, to learn the present condition of his patient; but he did not receive my letter in time, and has just sent me the following letter, which seems of so much importance that I add it as a footnote.

JUDSON, PARK CO., INDIANA.

MY DEAR DOCTOR:—

Your letter of July 14th last is just at hand, having lain in the Attica, Ind. P. O. all this time. The case of phimosis you refer to was an interesting one, and the operation was a grand success. The boy is now almost a man, and helping his father in the express office; and his father took great pride in telling me last summer, when I was in Attica, what a noble fellow he was, and how he owed his life to me, etc.

The profession is under lasting obligations to you for originating this proceeding and bringing it before the world so prominently. I know you must often feel disgusted and amused to find how slow some surgeons are in finding it out. My experience with the operation would make a book worth reading. In the first place, I used to treat those cases as paralysis from dentition, as Prof. Gross taught me, and I would apply the actual cautery to the lumbar region, etc. One case, Franklin Robinson, a banker's son in Clinton, Mo., had a balanitis come on, and I liberated his glans penis, wiped out the smegma, and cured my case of paralysis without knowing why or how.

This was in 1868, and your paper read at the Association in 1870 was a "light from heaven" to me; in fact, I saw a great light; and since then, the case you refer to is only one of many.

Yours truly,

To DR. L. A. SAYRE. T. F. LEECH, M.D.

Digitized by Google

could not discover anything that seemed to throw any light upon the case, except a condition of well-marked phimosis. Acting upon this, I immediately circumcised him, and from the very day of the operation the spasmodic action began to diminish, and in two weeks he was entirely well, without any other treatment of any kind. There has been no return.

This case was published in the *Toledo Medical and Surgical Journal*, January, 1883, and copied in *Braithwaite's Quarterly Epitome of Medicine and Surgery*, Part XIII, March, 1883.

Some years since, Dr. W. R. McMahon, of Huntington, Indiana, reported to me two cases of epilepsy caused by congenital phimosis, and that he had entirely relieved by operation As I was anxious to know whether the disease had returned, I wrote him again a few months ago, and received the following reply :—

HUNTINGTON, INDIANA, July 22d, 1887.

PROF. L. A. SAYRE, New York :—

Dear Sir :—In answer to your letter concerning the cases of epilepsy caused by genital irritation, and which I relieved by operation on the penis, and of which I sent you a full account some years ago, I will say, that they never had an epileptic seizure since the adhesions were broken up. They are now young men of sixteen or seventeen years, and in good health.

I have seen one other case of congenital epilepsy in a boy of ten years, in which there was very firm preputial adhesions and a high grade of inflammation of the parts. I separated the adhesions (gave no medicine) six months ago, since which time he has not had a single convulsion. Yours very respectfully,

DR. W. R. MCMAHON.

The following very instructive case of convulsions and paralysis from phimosis was operated upon with perfect success by Dr. I. D. Griffith, Kansas City, Mo. :—

CONVULSIONS AND PARALYSIS FROM PHIMOSIS CURED BY CIRCUMCISION.

CASE.—In 1883 I received a letter from a very intelligent gentleman of Missouri, from which I make the following extract:—

Our little boy is now two years and eight months old; is a very large and fully developed child, with large head, but well formed; general health good; he is fat and full of life. About the middle of June, 1882, one night, when he was put to bed, we noticed he had a high fever, which we thought was the result of a couple of insect bites on each foot, which caused the feet and legs to be considerably inflamed. About 11 o'clock he was taken with a very hard convulsion, which lasted for thirty minutes or more; we sent for our physician, and he thought the convulsion was caused by a high state of nervous excitement (his nervous system being very highly developed); for four or five days the convulsions continued at intervals, having from three to six a day, after which he seemed perfectly well, until December 2d, 1882, when again, after a high fever, convulsions followed, but only had two, and was perfectly well again until February, 1883, when he was again taken with convulsions, which continued for a week or more, at longer or shorter intervals. During this spell we had several physicians to see him, but could make no satisfactory diagnosis. They all thought he had too much brain, and it might be the result of some nervous derangement. All trouble, however, again passed off, and he was seemingly perfectly well during the *daytime* (and right here I will say that he has *always* been exceedingly *restless at night*), until in August, when he was again taken with convulsions (this time he had no preceding fever), which continued for several days. Our physician said the convulsions were the result of malarial poison, and put him on heroic doses of quinine, giving five-grain doses every three hours for a week. During this spell a new symptom manifested itself, viz.: paralysis of the hips and legs, rendering him unable to walk at all. The convulsions continued to come on at shorter and shorter intervals. Now, Doctor, I will, as nearly as I am able, describe the spasms: They have been variable, but mainly a jerking of the muscles; sometimes the face has gotten dark; he has never foamed at the mouth; breathing labored, pupil of the eye dilated,

Digitized by Google

though the eye was fixed, and usually the spasm would pass off with a cry, as if in pain, but often would pass off without any stupor, though sometimes he has gone to sleep just after a spasm. He sometimes would fall backward, and often fall over on his face. Spasms have not been preceded by a shriek, as a rule, though sometimes he has cried out just as a spasm came on. He does not bite his tongue, as a rule, though he did it once. Can anything be done to relieve this terrible cloud hanging like a pall over us?

Let us hear immediately, and, if necessary, will bring him to you as soon as possible.

With much respect, I am, in suspense, most truly yours,

GEO. W. HALLOWAY.

I immediately wrote to Mr. Halloway, requesting him to ask his physician to carefully examine the child's genitals, and if he found phimosis, to circumcise him at once.

I received a letter from the father a few months after, stating that Dr. Griffith, of Kansas City, had circumcised his boy, with the most satisfactory results. He had entirely recovered from his paralysis, and had had only one convulsion since the operation, which the father thought was caused by indigestion. As I was anxious to know the permanency of the cure, I wrote to Mr. Halloway a few weeks since, and in response received the following letter from Dr. L. D. Griffith, of Kansas City:—

KANSAS CITY, MO., July 29th, 1887.

DR. L. A. SAYRE:—

Dear Doctor.—Mr. Halloway was in to see me, and asked that I should write to you something of the history of his little boy from the time of the operation for phimosis. He, at the time I saw him first, was, I think, the most intensely nervous and restless young one I ever saw.

The least noise was most distressingly startling to him, almost causing convulsions; these last had become so frequent that his facial expression had become noticeably changed, and his mother seemed to be distressed whenever she looked at him. The day I operated (to give you an idea of the frequency) he had fifteen, and this was not unusual at all, as, whenever the least drop of urine would attempt to pass, priapism would immediately set up and a convulsion follow. From the moment of the operation (and, by the way, the foreskin was adherent all around, and a large roll of caseous matter just back of the corona) his convulsion ceased and he has had none since, with the exception of one, and this from eating half of a watermelon, or something which overloaded his stomach. Now, his expression has changed and with it his entire disposition. He recovered from his paralysis almost immediately after the operation.

I was called, not long since, to a neighboring town to see a little one with hip-joint disease, and with the opening up of the foreskin and dissecting it from the head of the penis, I removed the extension from the limb and the joint disease (?) disappeared. Beside these, I have seen ten other children suffering from convulsions relieved after I operated.

Yours truly,

J. D. GRIFFITH.

The following letter explains itself:—

MOBILE, ALA., 106 ST. FRANCIS ST., May 29th, 1887.

PROF. LOUIS A. SAYRE, M.D., 285 Fifth Avenue, New York:—

Dear Doctor.—Having read with much interest some articles of yours on reflex irritation, and one article on "Spinal Anæmia, with Partial Paralysis and Want of Coördination, from Irritation of the Genital Organs," permit me to thank you for this valuable contribution to our profession, and to ask your views as to my diagnosis and my proposed treatment of a case met with to-day in my practice.

W. E——e, male, white, aged six years, parentage good; very sallow, with black eyes and dark hair; head large, good intellect; teeth decayed; lower extremities atrophied; feet inverted, the right more than left; too much bending of knees backward when standing; the feet can be easily abducted to normal position, and if done with child standing, will remain by weight of body; can only walk short distance without falling; when walking, the feet are turned inward, the knees project forward and inward so as nearly to touch, with legs divergent; the gait unsteady

Digitized by Google

and inability to move in a straight line. Penis very small, not larger than a child six months old, prepuce long, with orifice so small as scarcely to admit a silver probe of pocket case, and preternatural redness of same. Frequent priapism, which has been observed since one year old; only partial control of sphincter ani; in other words, is not able to resist evacuation of rectum for a minute when nature calls. This the mother has noticed "ever since child commenced sitting up." The height of child is three feet four inches. In catechising the mother, she says: At birth the child was large and well-developed; when about nine or ten months old a weakness of neck was noticed, and he was not able to sit alone until eleven months old; after he learned to crawl, he continued to do so until three years old, when he was first able to stand alone; was four years old before he walked any. The atrophy of lower limbs first attracted attention in his efforts to walk. Appetite "not very good," very few things agree with him. Is this a case of "spinal anæmia with partial paralysis and want of coördination, from irritation of penis"? Such is my diagnosis. The treatment proposed—circumcision, nutritious diet, electricity, strychnia, cod-liver oil, friction, phosphorus, and, in short, assist nature to build up; an instrument to keep limbs in place while walking. Now, my dear Doctor, you have my case; I would send him to you, but the parents are not pecuniarily able to visit New York, and hence my letter to you for any suggestions you may favor me with. I have had very little experience in mechanical appliances, and you will do me a favor by specifying particularly on this subject.

I am very respectfully, yours,

A. J. Reese, M.D.

I immediately wrote to the Doctor, stating that I thought his diagnosis correct, and advised him to circumcise the child at once.

As I was anxious to know the ultimate result of the case, I again wrote to the Doctor in June, 1887, and received the following reply:—

Mobile, Ala., July 28th, 1887.

Prof. Louis A. Sayre:—

Dear Doctor:—Your letter of inquiry regarding a case about which I wrote you, May 29th, 1877, has been received, and should have been answered more promptly, but unavoidable circumstances prevented.

The case of which you speak, if I remember correctly (for I had the misfortune to lose my notes of about fifteen cases) was a boy, six years old. I operated, with remarkable results; digestion improved rapidly, perfect control of sphincter muscle, coördination all right. Saw him not long since, in fine health; a little parrot toed, otherwise all right. I am glad, my dear Doctor, to have the opportunity to thank you for your valuable contributions to the healing art. Since the above mentioned case I have operated about 25 times for this class of troubles, and never with regret; five cases within last few months, *i. e.*—1st. Pat. McM., aged two years and six months; one of twins; blonde, with large brain; unable to walk; the lower extremities not developed as body and upper extremities. The mother said she thought he had gravel, as he complained whenever he made water, as if in pain, and was very nervous. I examined genitals; found adherent prepuce, except the portion immediately surrounding meatus. Circumcised; removed hardened smegma (which was very abundant); dressed with oil. This constituted treatment. The child was soon walking; the limbs developed rapidly; no more gravel. 2d. Circumcision in one case, of infantile onanism, the first and only case I have ever seen. There was adhered prepuce in this case. The mother insisted that I should avoid circumcision if possible. Broke up adhesion, removed smegma, and with oil and forceps dilated prepuce. Child improved but was not entirely cured. I circumcised and had no further trouble. 3d. Was called to child eight months old (by attending physician) of good parentage, whose case simulated hip disease of right side; any motion of the limb gave great pain. My medical friend, having exhausted his resources and the child growing worse, sent for me; on examination found adhered prepuce, small preputial orifice; priapism when penis was handled (which we generally find in these cases); the movement of limb producing considerable excitement and pain. Diagnosed genito-reflex trouble; operated at once; result, "hip disease" cured. Never has had any more trouble. 4th. A mulatto child, two months old; healthy parents; was brought to office for prescription. The legs permanently drawn up; great pain whenever an attempt to forcibly extend them; had been in this con-

Digitized by Google

dition, the parents said, several weeks. No fever; nursed well; otherwise well, so far as could be seen. Examined penis; found prepuce adhered and opening small; priapism as soon as handled. Circumcised and uncovered gland after Sayre's method; child got well without further medication. 5th. Was consulted five weeks since with regard to ten-year old boy, of good parentage, who invariably wetted the bed at night. Examined penis and found firmly adhered prepuce; gave chloroform; circumcised; removed smegma. Is now entirely well, growing rapidly; has never wetted the bed since. Mother says he is quite another boy.

Now, my dear Doctor, I have written so hurriedly and imperfectly, that I fear you will not be able to make anything out of my brief notes. Some of my cases are exceedingly interesting, and results surprising to all parties concerned. I regret not being able to give a more minute detail. Again thanking you for getting me out of some ugly scrapes, and for the good done mankind, I am

Yours truly,

A. J. Reese.

Office of Drs. Cross and Easley, Little Rock Ark., April 11th, 1876.

Dr. Louis A. Sayre, New York:—

Doctor:—Your paper in 1870, and report on the subject last spring in Louisville, have attracted my attention to your theory of "reflex irritation from congenital phimosis," and I have done circumcision as advised in several instances, and always with the happiest results. I suppose the subject is still of interest to you, and even at this late day venture to send you the last of my cases.

In February last, Mrs. H. L., wife of a prominent citizen of this place, called me to see her little son, three years old. The child was of fair size and development, though very pale. He walked in a difficult, sprawling manner, and had habitually a dazed, wandering look, although when his attention was concentrated he was not wanting in intelligence. His mother said that he had suffered from trouble in the genital parts since he was six months old, that she had called their attention to it, but that none of her medical attendants had advised an operation. She described his condition as one of almost constant suffering. His rest at night was much disturbed, and he would often scream and start from his sleep. In urinating he appeared to suffer great pain, the water often coming guttatim. At such times the child would writhe and move about half bent, the tears frequently starting from his eyes. The prepuce was found elongated and very sensitive. The contraction corresponding to the meatus was so tight that an ordinary pocket probe would barely pass. The nature of the trouble was explained and consent to an operation immediately obtained. The prepuce was retrenched, the mucous membrane snipped, and found at all points closely attached to the glans. So firm were the adhesions that the membrane was literally dissected back. Hardened lumps of secretion were found almost encircling the corona. But little inconvenience resulted from the measure and the child began at once to improve. He is now vastly better in every respect, indeed, is one of the brightest and most promising little fellows of my acquaintance. I send this, not because of any connection of mine with the little procedure, but that I may say how largely we are indebted to you for putting us right on this interesting and important subject.

Very truly yours,

E. T. Easley.

When apprised that I was writing to you, my friend, Dr. Dibrell, gave me the following memoranda, and the case is so graphic an illustration of the trouble in question that I think I need not apologize for sending it.

In November, 1875, was called by Mr. O. to see his child, eighteen months old. The little patient presented a wasted, cachectic appearance, with its legs and feet œdematous. The child had great difficulty in voiding its urine, and invariably cried when doing so. He could not walk, nor could he stand, unsupported, upon his feet; his legs, to use the expression of the family, "would fly from under him." These symptoms were progressively growing worse. An examination of the genitals showed an extremely elongated prepuce with congenital phimosis. Through the opening in the prepuce a No. 1 flexible catheter could with much difficulty be passed. This condition was to me a satisfactory solution of the whole difficulty, and I at once circumcised the child. Its restoration to health was very rapid, and without the aid of any therapeutic measures whatever.

Digitized by Google

I have recently written to Dr. Easley to know the present condition of the patient, but up to the present time have received no reply.

I could add many more cases from various physicians, similar to those here quoted; but it would only be corroborative evidence and add undue length to my report, and I will therefore only quote one more letter, just received from a total stranger, since I finished writing my paper, as it is instructive in showing that when the operation is performed, it should always be done properly. I therefore add it to my report :—

FROSTBURG, MD., Aug. 29th, 1887.

DR. L. A. SAYRE :—

Dear Sir:—I have just read in the *Medical Record* that you are to read a paper before the International Medical Congress, next Monday, on the deleterious results, in children, of a narrow prepuce and preputial adhesion, which brings to mind a case I treated about fifteen months ago, a brief account of which I will now give you. In the early part of June, 1886, I was at Kingwood, W. Va., to see a case of talipes upon which I had operated a year previously, and was asked to see a boy eight years old, living in the country, three miles from Kingwood. The boy had always been unable to walk without support; rubbed the knees together until the skin was thickened and quite horny; the adductor muscles were hard and rigid, so that when the child was placed upon his back upon the table the knees could not be separated more than four inches; patient was well developed; no atrophy of any of the muscles.

He had been treated for "weak knees" by an intelligent physician, who had him wear some kind of a splint for several months.

As soon as I saw the case I was reminded of a case I saw in your Clinic at Bellevue, in the fall of 1885, it being a case sent to you from Cumberland, by Dr. "Tom" Healey.

I therefore examined its prepuce, and found it very much elongated and contracted, upon which I diagnosed it as a case of phimosis, producing reflex symptoms, and recommended circumcision, to which its parents readily consented. The operation was done without any anæsthetic with no one to assist me but the father. One week later I heard from the boy, through his father, stating that he was very much improved in his locomotion, being able to walk to the gate, about one hundred yards from the house, without assistance. However, about two weeks later on he wrote me saying the prepuce was contracting again, and that he had been trying to keep it open with a *husking knife*, but could not dilate it sufficiently, and that the boy was not walking so well as before. This result, as you will know, was because I had not removed enough of the prepuce, but the improvement immediately following the operation and then, too, the relapse simultaneous with the secondary contraction, convinced me of the correctness of my diagnosis. I therefore telegraphed them to meet me at Tunnelton, about one hundred miles west of this place, it being their nearest point on the B. and O., when I recircumcised him, whereupon he began to improve, and now walks quite well.

I write this to you because I am indebted to you for my ability to appreciate and properly diagnose and treat the case. Very respectfully,

C. C. JACOBS, M. D.

Class of '86, Bellevue Hospital Medical College.

APPENDIX.

Since presenting this paper to the Congress, I have seen a most valuable article by Dr. G. L. Magruder, of Washington, D. C., describing twenty-five cases of various nervous disturbances which he had entirely relieved by circumcision or dilatation, without any medication whatever. In this paper he also gives a summary of the nervous supply of the parts, and reviews, in so thorough a manner, a variety of the abnormal phenomena which accompany various peripheral nervous irritations, that I have thought it worthy to be added as an appendix :—

"The nervous supply to the genital organs is derived from the pudic nerve of the cerebro-spinal system and the pelvic plexus of the sympathetic system. The pudic nerve is a large branch of the sacral plexus, and gives off in its course the inferior hemorrhoidal

Digitized by Google

nerve which supplies the external sphincter, integument around the anus, and terminates in the perineal and the dorsal nerve of the penis. The perineal nerve is distributed by the cutaneous and muscular branches to the integument in front of the anus, the sphincter ani, the scrotum, and under part of the penis, the levator ani, transversus perinei, accelerator urinæ, erector penis, and compressor urethræ. The dorsal nerve of the penis accompanies the dorsal artery of the penis, and is distributed to the glans, prepuce, the upper surface and sides of the organ, and sends off branches to communicate with the sympathetic.

"In the female, the pudic nerve terminates in the clitoris, labia majora, and perineum. The other branches of the sacral plexus are the muscular, the superior gluteal, and the great and small sciatic. These supply the integument of the perineum and the back part of the thigh and the whole of the leg, the pyriformis, obturator internus, the two gemelli, the quadratus femoris, the glutei, the tensor vaginæ femoris and the adductor magnus muscles. Branches from this plexus supply the hip joint, perforating the capsule, and also the knee joint. Connection with the sympathetic nerve is had immediately through the large cavernous nerve, which, after joining with the dorsal branch of the pudic nerve passes forward to supply the corpus cavernosum and spongiosum. This larger cavernous nerve is derived from the inferior hypogastric or pelvic plexus. This plexus distributes branches to all the pelvic viscera, viz., rectum, bladder, and the vagina in the female. Filaments pass to the vas deferens, vesiculæ seminales and prostate gland in the male. The connection with the rest of the cerebro-spinal and sympathetic systems is intimate.

"Reflex spasm and paralysis from diseases of the digestive canal, the ovaries, uterus and urinary organs have been frequently observed. Why cannot the same phenomena arise from genital irritations?

. . . . . .

"Reflex paralyses are due, according to Romberg, Stanley, and Graves, to a suspension of the sensory influence of the fibres of the sympathetic system, and are motor spinal paralyses. Brown-Séquard attributes the origin to chronic irritation of the genito-urinary organs with secondary contraction of the vessels of the cord and atrophy of the corresponding parts. Levisson experimented by compressing the uterus, kidneys, intestine, or bladder of rabbits, and found reflex excitability abolished and a paralysis of the posterior limbs lasting until the irritation was removed. The paralysis was considered due to excessive irritation of the sensory fibres, thereby causing an arrest of the function of the motor-nerve centres.

"Feinberg has observed in animals, after a coat of varnish, tremor, hyperæsthesia, partial anæsthesia, increased reflex action, spasm and paralysis. Examination disclosed a dilatation of the cutaneous vessels, of the capillaries of the lung and the ramifications of the vena porta, hyperæmia of the meninges and a dusky redness of the cervical cord. If the animals survive a certain length of time, proliferation of the neuroglia occurs with atrophy of the nerve tubes from compression. Rosenthal, quoting these observations, says: "Thus, the irritation of the cutaneous nerves produces a reflex paralysis of the centres of vascular innervation in the cord." Now, if this follows from irritation of the cutaneous nerves, can we not expect as much from irritation of the nerves about the head of the penis? The same author mentions the fact that several cases had been referred to him by Prof. Dittel, which, upon exploration of the bladder had given negative results, while a careful examination of motion and sensation showed a diminution of the various forms of sensibility in the legs; in the trunk there was abnormal excitability of the nerve trunks or of the genital organs. He cites the case of a girl, aged twenty-three years, who was relieved of a paresis of three weeks' standing, by the removal of a needle deeply imbedded in the vagina.

"Leyden reported three cases of paraplegia following diseases of the bladder, which

Digitized by Google

commenced by symptoms of motor and sensory irritation. He found diffuse softening of the cord in two cases. He thinks that a sacro-lumbar neuritis may be propagated to the cord in diseases of the bladder.

. . . . . .

"Charcot, speaking of urinary paraplegia, says: 'The very number of the cases in which we see paraplegia appear in the course of disease of the urinary passages is, of itself, enough to show that the phenomenon is no chance coincidence.'

"From the foregoing I think that we are justified in the conclusion that phimosis and adherent prepuce give rise to varied troubles of more or less gravity, manifesting themselves either in the muscular, osseous or nervous systems; and that the removal of these abnormal conditions of the penis frequently affords marked relief, and at times perfect and permanent cure."

DISCUSSION.

Dr. De Forest Willard, of Philadelphia, remarked:—The former enthusiastic advocacy of circumcision by Dr. Sayre has led many rash and unthinking physicians to advise this operation in cases where it is entirely unnecessary, and has also caused them to overlook serious central lesions in cases where an adherent prepuce has been but an accidental coexisting condition.

In this, as in every other complaint, too much attention cannot be given to diagnosis and to the proper selection of cases. It is highly important to discriminate first between instances of simple adhesion and those in which actual contraction exists, since the former only require the slight operation of stripping of the glans, while for the relief of the latter, dilatation, incision or circumcision may be needed.

Stripping can be accomplished in nearly all cases of adhesion under one or two years old. It is performed by manipulating the penis for a moment until slight rigidity occurs, when gentle backward pressure soon reveals an orifice, pinhole at first, but soon enlarging until the meatus is revealed. Immediately around this opening, or at some point on the glans, adhesions will usually be encountered, but they can be broken up by the thumbs of the operator as he peels off the prepuce from the glans. Rarely a probe is required. As soon as the sulcus behind the corona is fully reached, the smegma is thoroughly cleaned away and a dressing of cosmoline upon borated cotton is applied. This should be speedily adjusted, as delay sometimes produces a condition of paraphimosis, for the relief of which dilatation or incision occasionally becomes necessary. Usually, however, two or three probes, or the rounded end of a couple of hair pins, slipped beneath the foreskin serve to permit the easy replacement of the hood. The dressing need not be disturbed until the slight inflammatory symptoms have passed, after which daily retraction and washing should be practiced throughout life.

I look upon subsequent cleanliness of this region as the greatest safeguard, not only against reflex irritations, but also against masturbation. Retained filth and smegma are far more likely to call a boy's attention to his penis by their unrecognized irritative effects than washing can possibly do.

In the Philadelphia *Medical Times*, June, 1883, I urged the non-mutilatory operation of stripping in all cases where it was capable of giving an easily-moving prepuce, but insisted that no tight hood should be left, and that all reaccumulations should be prevented.

The observation of many hundred cases since that time has confirmed the opinions then expressed, and I now very rarely circumcise a young child. A recent writer states that he seldom performs the operation after eight years of age. My practice

Digitized by Google

is exactly the contrary; young children can be relieved by the simpler methods, but when a child has reached eight or ten, and has never been able to expose the glans, contraction is almost certain to be present, and circumcision must be performed. In adults there is rarely any escape when the prepuce is tight. To take advantage of the helplessness of young children is akin to the action of the "normal ovariotomist," who persuades feeble woman to undergo an operation the analogue of which he would not dare to propose to a man.

My rule is to secure a *freely-moving prepuce* in *all* cases where severe nervous symptoms arise, when there is muscular incoördination or paralysis, partial or complete, and in all cases where evidences of malnutrition are present. If the simple methods described will not accomplish this result, free dilatation, free incision, or, more frequently, circumcision, becomes essential. I am an advocate of discriminate circumcision, but not of indiscriminate. In properly selected cases it is of the greatest value, and yields most brilliant results. In all necessary conditions, as above indicated, I do not hesitate to perform it, but it is a comfort to know that in very many cases it is wholly unnecessary, and that the same results can be obtained by the more simple plan.

Circumcision is in itself mutilatory, but, like amputation, may become a necessity.

I do not believe that the removal of the prepuce is hygienic. The object of the fold is to protect the delicate skin of the glans during childhood, and its recedence is gradual as puberty is neared. If too early exposed, the excitation is increased, masturbation and excessive venery are encouraged, and the child is subject to contracted meatus and other evils of condensation of tissues. He has also to endure the mortification of boyish taunts. Furthermore, cleanliness of prepuce and of mind are far more potent averters of syphilis than is circumcision.

It is idle to class this operation among Mosaic sanitary laws. It was ordered long before the time of Moses, not upon hygienic, but upon religious grounds, as a distinctive mark. Its adoption by other nations was undoubtedly due to the fact that their superstitious minds easily accepted the theory that by thus mutilating themselves, the acknowledged blessings showered in past times by the Almighty upon this "peculiar people" might be secured to themselves, since this was the only outward and visible sign of difference. Such barbaric sacrifices are not infrequent, and those who practice this rite are certainly not noted either for their morality or cleanliness.

As to the existence of reflex symptoms from a contracted prepuce, there is no question. Anatomically, the explanation is easy, and similar effects are seen in nervous irritations in other parts of the body. Gastric, uterine and dental disturbances are common, neuromata, foreign bodies, coxalgia, empyema, fissure of the anus, all act at times to produce a peculiar train of phenomena. Why, then, should not an irritant acting upon nerves confessedly the most excitable in the body, give rise to most grave results, affecting permanent nutrition as well as temporary nerve currents? That these results do not always follow compression of the glans and the retention of smegma, is no argument against the existence of such causes. Depressed fractures of the skull are not always followed by serious results, and many nerve injuries are produced without the occurrence of tetanus, yet this does not prove them to be harmless.

Adherent prepuce will be found in babies so constantly that it may be looked upon as almost normal or as a continuation of the intra-uterine condition. Hence, it must be remembered that serious brain and spinal disease should not be overlooked

Digitized by Google

as a factor in the production of muscular weaknesses, since the tight prepuce may have little or nothing to do with such result.

On the other hand, when no central lesion exists, the simple stripping of the glans will convert many a feeble, puny, illy nourished, wakeful, irritable boy into a healthful and happy child. Too little attention is ordinarily paid to this sexual cause in its early symptoms, as just indicated, but prompt action at this stage will often avert the later and more serious consequences; in fact, I believe that reflex paresis is always preceded by the symptoms of nervous irritability, and can often be prevented by timely action.

If the profession will watch more carefully they will find many cases of apparently obscure malnutrition that can thus be easily explained, and it is certainly a comfort to feel that only a small proportion of the younger ones will require circumcision.

To summarize: 1. In infants, strip the glans in all cases where urinary, nervous or muscular symptoms arise. A freely-moving prepuce is more healthful than a naked glans. Cleanliness is the best safeguard against disease, and is to be insisted upon all through life.

2. Incise or dilate all cases of moderate contraction.

3. Circumcise boys over ten years of age, and adults when the glans cannot be exposed, and all other cases where reflex symptoms of genital origin arise, and in whom the foregoing measures fail to give a freely-sliding foreskin.

Dr. I. N. Love, of St. Louis, said:—The tenor of the remarks during this discussion has been in the direction of conservatism. It has been my judgment and my practice for many years in these reflex irritations to pursue the radical course of circumcision. I believe thoroughly in the Mosaic law, not only from a moral but also from a sanitary standpoint. All genital irritation should be thoroughly removed. It is all very well to instruct the mother or the nurse to keep the parts within the prepuce clean, but they cannot or they will not do it. Complete and proper removal of the covering to the glans takes away all the cause of disturbance.

Dr. Sayre takes a more pronounced position on this subject than the majority of those who have discussed his paper. An improper performance of a surgical procedure is no argument against the operation, but rather against the operator.

For the reasons I have given, I am in favor of the radical application of the Mosaic rite of circumcision.

Remarks by the President:—That a considerable number of ailments or morbid conditions result from a narrow prepuce or preputial adhesions, either directly or through reflex action, is now generally admitted, and no one has done more to disseminate a knowledge of this fact than the author of the paper to which we have listened. Sometimes the causative relation of a narrow prepuce and preputial adhesions to the symptoms is very apparent, as in the following case, which was recently treated by me: F. P. L., aged twenty-three months, had been more or less fretful during micturition since the age of three months. During the last year he had been restless at night, and had apparently been in pain during the half-hour and sometimes even two hours preceding urination; at such times he often applied his hand to the penis or drew upon the prepuce; sometimes he pressed the hand of the nurse or mother to the penis. Various medicines had been given without benefit; examination of the urine gave a negative result, and there was no external evidence of inflammation in the parts affected. The child was firmly held by the mother and nurse, the foreskin was drawn forward by the thumb and finger, and the point of the

Digitized by Google

small two-bladed forceps, commonly used for the ear, was introduced into the pin-hole orifice of the prepuce. By gradually separating the blades the prepuce was stretched. On the second day the operation was completed, the prepuce was retracted beyond the glans, and the masses of smegma, white and firm, were removed. They had produced inflammatory redness and thickening of the preputial mucous membrane which surrounded them. By daily uncovering the glans, smearing it with vaseline and returning the prepuce, the dysuria soon ceased and the child has remained well. It is probable that onanism as well as the dysuria was prevented by this simple treatment.

In many instances the causative relation of the preputial disease to the symptoms which it produces is not so apparent as in the above case, but after correct treatment of the prepuce they disappear. There is one result of phimosis which I did not observe that either Prof. Sayre or those who contributed the material for his paper noticed. The expulsive efforts accompanying urination sometimes cause prolapsus recti, and frequently cause inguinal hernia. In a lecture before the Harveian Society (*British Med. Journ.*, Feb. 28th, 1880), Edmund Owen, Surgeon to St. Mary's Hospital and to the Hospital for Sick Children, says: "Perhaps the commonest cause of hernia in childhood is a small preputial or urethral orifice, and next to that I would put the smegma-hiding or adherent prepuce." Arthur Kempe (*London Lancet*, July 27th, 1878), Senior House Surgeon to the Children's Hospital, says "Phimosis is a common occurrence, and numerous ill effects can undoubtedly be attributed to it;" and he alludes to the observations of Mr. Bryant, as published in his book on the "Surgical Diseases of Children." "In fifty consecutive cases of congenital phimosis, thirty-one had hernia, five had double inguinal hernia, and many had umbilical hernia besides. In no one was the hernia congenital, its earliest occurrence being at three weeks. Circumcision was performed in these cases, and all were much benefited."

Dr. Sayre, in concluding the discussion, said that he was very glad to find that Dr. Willard and others who had formerly disagreed with his views—and some of whom had censured him rather severely—had now come to the conclusion that he was correct, and agreed with him that any constriction of the glans penis was a source of irritation to the nervous system, which should be relieved either by stretching or incision. In all cases where the relief can be accomplished by stretching, of course incision is entirely unnecessary, but there are some cases where not only incision is necessary to relieve the constriction, but where absolute circumcision is a necessity, to remove a portion of the redundant and contracted prepuce. But even in these extreme cases the operation should be so performed as to leave sufficient prepuce to cover the glans, but should be so arranged as to glide freely backward and forward over the glans without any constriction.

As to complete ablation of the parts under all circumstances, as recommended by Dr. Love, I must enter my strongest protest.

The cases of hernia and prolapsus of the rectum referred to by Dr. Smith have been so fully described in my previous papers that I have not referred to them here. That hernia and prolapsus of the rectum should be produced by the constant straining at micturition through a constricted prepuce, and that both of these infirmities should be immediately relieved by removing the constriction, can be easily understood, and I certainly feel very happy that I have drawn the attention of the profession so strongly to this important subject; and to now find that my views are so universally endorsed by the profession as being correct!

Digitized by Google

THE MILK SUPPLY OF CITIES.

L'APPROVISIONNEMENT DU LAIT DANS LES VILLES.

DIE MILCHVERSORGUNG VON STÄDTEN.

BY DR. CYRUS EDSON,
Of New York.

From a sanitary standpoint the milk supply of cities is second only in importance to the water supply. The most vulnerable portion of the community to the attacks of disease are the children.

To protect these is the health officer's first duty. Children are, of necessity, subjected to influences in large cities that tend to depress and lower their vitality. It is, therefore, of the highest importance that they should receive plenty of nourishing food.

Milk is the chief food of children. No article of food is so liable to be adulterated or charged with noxious matter. The peculiar physical properties of milk make it easy for the unscrupulous to tamper with it for his own selfish ends and it readily conceals within its opaque body disease-producing material with which it may be accidentally charged.

Probably no class of men see more plainly the evils arising from ignorance than physicians. When joined to avarice its power to injure is greatly enhanced. Nowhere do we see this combination working more successfully to spread evil than among the people who "manufacture" milk in and about great cities.

Milk may be unwholesome by reason of adulteration or by reason of contamination with noxious matter.

We will first consider the adulteration question, and then take up the wider and more important subject of the contamination of milk.

By the adulteration of milk is meant the addition of any substance or the removal of any of its constituents.

The principal adulterant of milk is water, and the next most common sophistication is the removal of cream. Many other substances have been used to adulterate milk, and we find in most text-books long lists of adulterants, including calves' brains, rape seed, starch, gum and other material the use of which is doubtful.

Chalk, salt, carbonate of soda, nitrate of soda and flour have been found by inspectors in New York milk.

An ingenious method in use is to dilute with water to which is added flour, salt and sugar, and then to enrich the color which, of course, is rendered bluish and whitish, with turmeric and annotto.

Probably the most dangerous adulterants of milk are the so-called preservatives, such as boracic acid, salicylic acid, benzoic acid and antiseptic naphthols.

These adulterants are becoming very common. In England, I am informed, their use is either tolerated or winked at by the authorities, and that scarcely a quart of milk is sold in London or Liverpool without having been dosed with salicylic or boracic acid. It is true that the amount of these preservatives necessary to effect preservative results is small, from two to eight grains to the gallon, nevertheless I believe that they are extremely harmful to the delicate digestive organs of young children, and to their kidneys, through which they are eliminated.

Cream is not often adulterated, but now and then attempts are made to adulterate it.

In 1882 a firm of dairymen tried to make an emulsion of beef and lard oils to imitate cream. It worked well until the bogus cream was shipped one cold night, when the extreme cold caused it to separate and the oil to solidify into cakes and layers of lard and tallow.

Digitized by Google

I have recently been informed that an artificial cream is being shipped to New York, made by adding egg albumen to milk.

My information is so reliable that I have no doubt but that I shall verify it. I have not yet had time to do so.

DETECTION OF THE ADULTERANTS.

In order to detect adulteration of milk by the addition of water and the removal of cream, it becomes of great importance to determine what the limits are between which the constituents of average milk vary. The percentage of fat, for example, varies according to the age, breed, time before calving, the quality of the food and the condition of the animal. By a careful compilation of the results of several thousand analyses it has been decided by the legislature of the State of New York that pure milk contains at least three per cent. of fat, not more than 88 per cent. of water and 12 per cent. of total solids, including fat.

Detection of Water.—This may be detected by chemical analysis or by the lactometer. As the former means is reliable only in the hands of a practical chemist, I will describe the latter.

The lactometer is simply a hydrometer whose 0° equals a specific gravity of water, viz., 1000, and whose 100° equals a specific gravity of 1.029, the space between the 0 and the 100 being divided into 100 parts. The 100° is supposed to indicate the specific gravity (at a temperature of 60° Fahr.) of pure milk.

It was found to be an absolute fact that milk from healthy cows never fell below 1.029 at 60° Fahr. The thousands of samples of milk taken from cows, not only in the United States but abroad, confirm this fact.

Let us suppose, then, that we have a sample of milk which has a specific gravity, at a temperature of 60° Fahr., of 90° upon the lactometer scale. As 0° equals specific gravity of water and 100° that of pure milk, a specific gravity of 90° would indicate that ten per cent. of water had been added to the milk.

Now, as a matter of fact, the average milk has a specific gravity of about 109° on the lactometer. The 100° mark was taken as the standard, because a few samples of milk were found that had a specific gravity of 102 on the lactometer, and by putting the standard at 100°—1.029 hydrometer—the authorities felt that no injustice could be done to any farmer or dealer.

The lactometer has been more abused than any instrument I know of, and the reasons for the distrust with which it has been regarded are, I think, due to—

1. The fact that formerly a large number of lactometers were on the market whose 100° indicated a specific gravity of 1.030, 1.032, etc., and others that were badly made.

2. To the fact that the temperature is often overlooked. Any one can understand what a great difference this produces in the results.

3. To the fact that if a sample of milk is taken warm from the cow, placed in a bottle, the bottle corked up, and the milk cooled to 60° F., the specific gravity of the milk may fall below 1.029, because a large quantity of air has become entangled in the milk, and this lowers the specific gravity. A very good illustration of this fact is shown in an experiment made by the chemist of our department. The skimmed milk from a De Laval Milk Separator was tested.

Owing to the rapid revolution of the machine, 4000 per minute, the milk was charged with air. As it came from the machine it had a specific gravity of 15° at 60°. In one-half hour it had a specific gravity of 105° at 60°, and after five hours had a specific gravity of 125° at 60° F.

You cannot dip the lactometer into any white fluid, and say, because the specific gravity is less than 100, that this is watered milk; or because the specific gravity is greater than 100 that this is pure milk. All that is claimed for the lactometer is that if water alone has been added to milk it will surely show that fact.

Digitized by Google

The opponents of the lactometer always say, "Would it not be possible to skim the milk and thus increase its specific gravity, and then add water until quite a quantity had been added, without the lactometer indicating anything abnormal?" Of course the specific gravity would not indicate anything, except that the milk was pure, but the appearance of the milk under such conditions would be a sure indication of the fraud. As I draw these lactometers from the cream, from the milk, from the skimmed milk, and from the watered and skimmed milk, I think it does not require an expert to detect the difference.

There are several other interesting instruments that have been devised for the detection of adulterated milk. I will only show you one other, as I want to call your attention to other important matters relating to our milk supply. This instrument is Prof. Feser's Lactoscope.

It supplies us with a simple optical test for determining the per cent. of fat in a sample of milk.

From the quantity of milk employed to render water opaque this can be readily determined. The instrument consists of a hollow glass cylinder doubly graduated, one scale giving the amount of water added to make four cc. of milk transparent, the other showing the per cent. of fat present.

A description of the various means used by chemists to detect the preservatives added to milk would take more time than can be devoted to it here. They would not be practical in the hands of busy practitioners, and so I pass them over.

In testing a sample, four cc. are transferred into the instrument by means of the graduated pipette. Water is gradually added, and the mixture thoroughly shaken until all of the black lines on the cylindrical body of milk glass can be read. The level at which the mixture stands on the percentage of fat scale shows that percentage present.

The instrument affords a ready and easy means for determining the relative richness of different specimens of human milk. In fact I have used it myself for this purpose to aid in the selection of a wet nurse.

The examination of milk by the microscope is of great importance and should never be omitted. Pure milk from a healthy animal has the appearance shown in the photograph that I show among you. The fat globules vary in size very little. It has been noted that normal food produces milk in which the globules are more uniform in size than in that produced by cows fed on unhealthy food. I show you also photo-micrographs of skimmed milk and of cream.

The subject of infected milk now presents itself.

That milk will absorb the germs of disease with great readiness is admitted by all who have at all studied the etiology of contagious disease. In a paper on this subject, read before the International Medical Congress of 1881, Mr. Ernest Hart, of London, sums up as follows: The number of epidemics of typhoid fever, recorded in the abstract as due to milk, is 50; scarlatina, 15; of diphtheria, 7. The total number of cases occurring during epidemics traced to the use of infected milk may be reckoned in round numbers as 3500 of typhoid fever, 800 of scarlet fever and 500 of diphtheria. When it is remembered that within the past ten years all these recorded and authentic epidemics occurred, and that before that time we were practically ignorant of the fact that milk is a carrier of infection, we realize the importance of taking the most stringent measures to prevent contamination of our milk supply.

Not only is milk liable to contamination by disease germs, but it is also liable to a sort of auto-infection. I refer to the development of ptomaines in milk subjected to certain influences. We are all more or less acquainted with Professor Vaughan's tyrotoxicon. I have seen a number of cases of poisoning produced by milk that could only be accounted for on the hypothesis of the presence of tyrotoxicon or some other ptomaine.

One is interesting on account of the number of persons poisoned; I will give it:—

On the 12th of May of this year I was directed by the Sanitary Superintendent of

Digitized by Google

New York to investigate several cases of poisoning at 1988 and 1990 Second Avenue, of that city, reported by Dr. J. A. Powelson. In the two aforesaid houses twenty-one persons were found suffering from symptoms of irritant poisoning. The symptoms presented were vomiting, colic, diarrhœa, vertigo, headache and great prostration. All had drunk milk purchased from one milkman, who obtained it of a reputable dairyman in Dutchess County, New York. All were taken ill in from one to four hours after drinking the milk, and those who vomited early were least affected.

By making an investigation among the customers of the milkman, fourteen other cases of poisoning were found, exactly like the Second Avenue cases.

As several gallons of the poisonous milk were obtained, a most thorough analysis was made by E. W. Martin, the chemist to the New York Board of Health. All the poisonous metals were tested for, and many of the vegetable poisons, with negative results. Professor Vaughan's method of extracting tyrotoxicon was tried carefully, and a few grains of a crystalline substance were obtained, which appeared like tyrotoxicon, but on feeding it to rabbits no effect was produced. In short, chemical analysis developed no results whatever.

The odor of this milk was peculiar and offensive, being of a sickly, sweet nature. So characteristic was this odor that milk found in several families that were affected could be readily recognized by it.

None of the persons poisoned by this milk died, though two came near doing so. These two had slight gastro-enteritis before taking the milk.

A painstaking and thorough investigation was made at the dairy from whence the milk came, and of the farms that supplied the dairy, and it was found that hoof rot was prevalent on them.

No evidence could be obtained that milk from animals affected with that disease had been shipped to New York, but the farmers had a deep-rooted belief that milk from such cows was not deleterious. A sample of milk from an animal with the disease was submitted to the chemist and myself, and it was found to have the same peculiar odor that was noticed in the case of the poisonous milk, and a small amount fed to rabbits made them violently ill. Attempts were made by one at vomiting, and both showed great apathy for some time after.

Vaughan believes that the ptomaines are more liable to be developed in milk from diseased animals, and the cases just described would seem to confirm this theory.

Notwithstanding assertions made to the contrary, I believe that the poisonous milk ptomaines are very difficult to isolate by any means known to chemistry.

We are too often compelled to make our diagnosis by exclusion. All circumstances, however, point to the fact that the ptomaines are developed during the decomposition of milk, and, consequently, anything that favors decomposition necessarily favors the production of ptomaines.

Want of cleanliness in handling milk, filthy barns, unclean udders, dirty cans and bottles, are all sources of danger. The crust that forms around the necks of bottles and cans, unless removed, rapidly decomposes, and thus charges the contents of the vessel with the germs of putrefaction.

I believe, with Professor Vaughan, that the most important advantage secured to breast-fed children arises from the lessened danger of infection of milk with germs which may produce poisonous ptomaines.

The following rules are given by Vaughan for the prevention of the development of tyrotoxicon in milk.

RULES FOR THE PREVENTION OF THE DEVELOPMENT OF TYROTOXICON IN MILK.

1. The cows should be healthy, and the milk of any animal which seems indisposed should not be mixed with that from the perfectly healthy animals.

Digitized by Google

2. Cows must not be fed upon swill, or the refuse of breweries or glucose factories, or any other fermented food.

3. Cows must not be allowed to drink stagnant water; but must have free access to pure, fresh water.

4. Cows must not be heated or worried before being milked.

5. The pasture must be free from noxious weeds, and the barn and yard must be kept clean.

6. The udders should be washed, if at all dirty, before the milking.

7. The milk must be at once thoroughly cooled. This is best done by placing the milk can in a tank of cold spring water or ice water, the water being the same depth as the milk in the can. It would be well if the water in the tank could be kept flowing; indeed, this will be necessary unless ice water is used. The tank should be thoroughly cleaned every day, to prevent bad odors. The can should remain uncovered during cooling, and the milk should be gently stirred. The temperature should be reduced to 60° F. within an hour. The can should remain in the cold water until ready for delivery.

8. In summer, when ready for delivery, the top should be placed on the can and a cloth, wet in cold water, should be spread over the can, or refrigerator cans may be used. At no season should the milk be frozen; but no buyer should receive milk which has a temperature higher than 65° F.

9. After the milk has been received by the consumer, it should be kept in a perfectly clean place, free from dust, at a temperature not exceeding 60° F. Milk should not be allowed to stand uncovered, even for a short time, in sleeping or living rooms. In many of the better houses in the country and villages, and occasionally in the cities, the drain from the refrigerator leads into a cesspool or kitchen drain. This is highly dangerous; there should be no connection between the refrigerator and any receptacle of filth.

10. The only vessels in which milk should be kept are tin, glass or porcelain. After using the vessel, it should be scalded, and then, if possible, exposed to the air.

Adulteration with water is a very common source of contamination, for the adulterator is not at all fastidious as to the quality of water he puts in his milk. I have frequently had in my possession toads and hair worms found in New York milk by the inspectors. As water is frequently the carrier of the germs of disease, it follows that water contaminated by such germs, if added to milk, will also contaminate it and make it a carrier of the same diseases.

We all know how pure water is in the average country well. I have inspected a great number of such wells, and I do not believe that one in a hundred furnishes water fit to drink.

It is safe to say that in 20 per cent. of farms in New York State the cows are watered from wells situated in the barnyard itself, contaminated by its drainings.

This leads us, naturally, from the consideration of milk contaminated by noxious matters outside of the cow to the consideration of that contaminated by factors arising in the condition of the animal herself.

Milch cows should be fed on wholesome food and given plenty of pure water to drink.

Distillery slops are highly injurious as a food for cows. In order to use them as feed, the cows must at first be closely confined and all other food withheld, as the animals will not eat them unless compelled by starvation.

After having been forced to eat the stuff they take to it like drunkards to their grog, and if permitted will gorge themselves with it. Cows thus fed are never given any water to drink, as it is considered by swill feeders that sufficient water is present in the swill to supply their need in this respect.

They are never given fresh air or exercise, both of which are indispensable to the well being and health of the animal.

Digitized by Google

For months they stand yoked between uprights, their noses over the swill trough, breathing the emanations from their accumulated filth, in stables that often have only six or seven feet headway.

I have seen cows in such stables, in stalls that were only 2 feet and 29 inches wide. In one case only 226 cubic feet of air space was allowed each of the 30 animals stabled. (The New York Board of Health insists that at least 300 cubic feet of space shall be allowed one infant.)

I give these facts because they go with the feeding of distillery swill. I have personally seen food of this kind fed, and I have been the means used by the Board of Health of New York to effect the punishment of persons feeding it. Yet I have never seen it fed except under the conditions I have described.

As distillery slops contain a large amount of free acid, the direct effect of feeding them is to charge the systems of the animals so fed with acid. It produces an acid diathesis. The result of this is that the secretions naturally alkaline are acid.

Even the milk, which should be neutral or faintly acid, is markedly acid.

Another result of this acid condition is the tendency of the tissues of the cow, especially the skin, to ulcerate. Large ulcers often form on the flanks and shoulders where the skin comes in contact with the floor on lying down. The tail often ulcerates, usually within six or eight inches of the buttocks, and drops off. Milk from animals fed on distillery swill coagulates in tough lumps. I have seen a complete cast of the vessel which held such milk formed by the tenacious curd. This cast could be handled without breaking.

The following is a copy of an autopsy made by Dr. A. H. P. Leuf, of Brooklyn, in the case of a child of four months, who died from the effects of swill milk:—

"I made an autopsy on the body of Stanley F. Heyden, of 281 Bergen St., August 1st, 1882. I found the stomach exceedingly soft and delicate, and filled with coagulated milk, forming quite a firm lump, over three inches in diameter. The stomach was also reddened. The intestines were very pale and entirely bloodless, and contained a pale, slimy material, characteristic of inflammation.

"The membrane of the intestines was also closely studded with little whitish specks, which were enlarged glands. All the other organs of the body were normal except the marked paleness. In my opinion death resulted from exhaustion (collapse), due to gastro-entero-colitis, augmented by the presence in the stomach of the firm clot of coagulated milk which was too firm for the child to vomit up or pass down into the gut, and therefore acted as a foreign body and irritant.

[Signed.] "A. H. P. Leuf, M. D."

The Brooklyn health authorities investigated the source of the milk that was given the child, and found that it came from swill-fed animals.

It was deficient in fat, while the curd was tough and abundant.

In regard to brewery grains, or brewers' grains, as they are generally called, the case is different.

Nevertheless, it is highly wrong to feed them exclusively or even for the most part. This is shown by the fact that cows fed on them become what is termed "grain sick." The animal loses its appetite and is apt to have diarrhœa.

I have no hesitation in saying that a little grains judiciously fed with other food will do no harm

The odor of swill or grains is very perceptible in milk from animals fed on either. The proper way to make the "smell test" is to shake the milkman's forty-quart can, or have some one do it for you, and then removing the cover quickly smell the inside of it (the cover).

The odor is always present. It is always safer not to use milk having the odor of grains, at least not to use it for infant's food.

In addition to the peculiar and unhealthy curd in milk from cows fed on swill and

Digitized by Google

brewers' grains, it is certain that such milk will rapidly sour and spoil, even when subjected to the most favorable conditions for keeping it. The great condensed milk factories in New York State, and I believe elsewhere, compel the farmers who supply them with milk to sign a contract to feed no brewers' grains, starch feed, distillery swill or even ensilage.

The reason for this is that milk from animals thus fed *cannot* be preserved even after being condensed, charged with sugar, and put up in hermetically sealed cans!

Bad treatment and accidental feeding of certain plants sometimes affects milk in a remarkable manner. Milk will suddenly sour only a few hours after being produced, or by some peculiar decomposition of its casein it will turn blue or it will become stringy, running in strings when poured from vessel to vessel.

Sandy milk is the result of excess of lime salts derived from food, though I believe some observers think that it is due to a disease that causes the lime in the tissues of the animal to concentrate itself in the milk.

Old milkers frequently give milk which has a bitter taste. This milk very rapidly decomposes, with the development of a very disagreeable odor.

A very curious epidemic resembling typhoid fever broke out on Washington Heights a few years ago. It was found, on investigation, that the disease was confined to the customers of a certain milkman. An examination of the cows owned by this man was made, and one was found suffering from an abscess of the udder. At the time the examination was made this cow was being milked into the common milk pail. Although the investigation was very thoroughly conducted, no other cause could be found, and when the cow with the abscess had been quarantined the sickness speedily stopped.

Milk from animals afflicted with tubercular disease has been proved to have the property of transmitting to man that disease, unfortunately common among cattle. Highly-bred animals are particularly affected by this scourge.

One cause of this is that too much attention is devoted by breeders to producing wedge-shaped animals.

It is considered desirable to breed animals having great breadth of hips and a capacious belly that narrows down toward a thin chest. In this way the lungs and chest capacity are sacrificed. The tendency is in narrow-chested animals, as in flat-chested men, to develop lung disease and tuberculosis.

Interbreeding is another curse that spreads the disease. I have known the daughter, granddaughter and great granddaughter to be crossed by the father.

Tuberculosis kills annually more people than any other disease.

Of 709 autopsies performed on the bodies of children under ten years of age 33 per cent. were found to have died from tuberculosis (*Medical Record*, New York, June 19th, 1886, p. 713). An analysis of these cases showed that the disease was twice as frequent in the abdominal cavity as in the brain. I believe that tuberculosis is rarely an hereditary disease, and base this opinion on the fact that it is almost never found in fœtal life and is rarely seen in children under a month old. Now, if the disease is more often acquired than inherited we must look for some fertile source that affects the younger portion of the community rather than adults.

Milk from tubercular cows has infected pigs to whom it was fed. It is more than reasonable to believe that it often infects children. I feel confident that tuberculosis will, before long, be included among diseases communicable from animals to man.

During the spring of 1886 an epidemic of scarlet fever occurred in a certain district near London.

Investigation showed that the families in which the fever appeared used the milk from one dairy. An investigation failed to discover that the milk had been exposed to scarlet fever contagion. The cows were carefully examined, and were found to be suffering from a peculiar affection known as blistered or sore teats. Though rather a common disease, it was unknown to have had any connection with scarlatina.

Digitized by Google

A painstaking investigation by Drs. James Cameron and E. Klein showed pretty conclusively that the disorder was transmissible, and that it was the cause of the scarlatina epidemic.

Bovine scarlatina has been described, but the description is widely different from the disease discovered in the case I have just quoted. Klein discovered in these cases a bacillus which he believed to be the cause of the disease and the true scarlatinal parasite.

Besides the teat eruption the cows had fever, quickened respiration, dry, hacking cough, sore throat, discharges from the eyes and ears, alteration in the milk secretion and some visceral lesions.

We have seen that milk may be made a source of danger to us by the dishonest, careless, or the ignorant.

By adulteration the consumer is robbed of his proper nourishment. Infected milk may act as a carrier and disseminator of disease.

The danger may be twofold: by adulteration the health of the consumer may be reduced and his system made fertile, so to speak, for the growth and development of the germs of disease that may contaminate the milk.

What, then, is the remedy to be applied by the community for the cure of the evil? The first duty of every government is to protect the well-being and health of its citizens.

Inspectors of milk should be provided, and every city should have at least one such inspector to every hundred thousand of population.

Each inspector should be provided with a lactometer, with which he should carefully test the milk offered for sale by dealers. When doubt exists in his mind as to the quality of the milk, he should supplement that test by chemical analysis.

The laws against milk adulteration in New York city are excellent, and other cities would do well to copy them.

But the control of milk adulteration disposes of but a small part of the dangers from milk. It is infected milk that is most to be feared, and against it the State provides almost no safeguards.

In order to cope with these dangers a corps of veterinary inspectors should be provided.

It should be the duty of these men to examine from time to time every herd of cows in the State. They should have the power to summarily destroy all animals suffering from tuberculosis, the State to reimburse the owner, and to effectually quarantine those liable to cause contagious disease.

But the State has another important duty to perform in addition to enacting laws and compelling obedience to them; it should educate the farmer how to properly care for his stock, and point out to him the dangers to himself and fellow men of filth and lack of hygienic surroundings.

DISCUSSION.

Dr. Leeds desired to bear testimony to the valuable work done by Dr. Edson in extending the scope and thoroughness of the work of inspection in relation to the milk supply of cities. The present principal work of inspectors, which is in detecting the percentage of adulteration *by water* merely, is valuable, but the addition of water *is* the least dangerous *adulteration. It is* a commercial fraud. But, in carrying the work of the inspector into the dairy and the stall, a much more important service is done, so far as health is concerned. To do this work thoroughly, a corps of young men, with medical training, and specially educated for their duties by familiarity not only with chemical methods, but also with microscopy and bacteriology, is required. With their aid, the present spread of infectious diseases from cattle to men can be successfully combated, and the labors of men like Dr. Edson should be assisted by such trained corps of workers and liberal appropriations by the State for their maintenance.

Digitized by Google

THE USE OF COW'S MILK IN THE ARTIFICIAL FEEDING OF INFANTS.

L'USAGE DU LAIT DE VACHE DANS LA NUTRITION ARTIFICIELLE DES ENFANTS.

DER GEBRAUCH DER KUHMILCH BEI DER KÜNSTLICHEN ERNÄHRUNG DER KINDER.

BY VICTOR C. VAUGHAN, M.D., PH.D.,

Director of the Hygienic Laboratory in the University of Michigan.

That cow's milk must form the basis of the food used with most children who are for any reason denied the mother's breast, all will admit. I shall not stop to speak of the comparative value of the milk of other animals in infant feeding; nor shall I discuss the adulterations to which the milk of the cow is subjected. I shall give my time and attention to the following points:—

1. The preservation of milk from putrefactive changes, which are liable under certain conditions to occur in it, and which render it wholly unfit for use.

2. The manipulations to which milk may be subjected in order to render it more nearly like the secretion of the breast of woman, and more suitable for use as food for infants.

3. The question as to the desirability of continuing milk as a food in certain diseased conditions of the child.

That milk and certain of its products are liable to changes whereby they are rendered not only unfit for food, but positively poisonous, is a fact which has been long known. Cases of cheese poisoning are by no means rare in Germany and in this country; while the serious effects which follow the eating of ice cream, frozen custards, etc., multiply as these luxuries become more extensively used. Some three years ago, the writer succeeded in isolating the active principle from poisonous cheese, and gave to it the name tyrotoxicon.* Later, he found the same poison in milk and in ice cream. The poison has been detected now by about a dozen different investigations, and in cheese, ice cream, custard, milk, cream puffs and oysters.† Chemically, this poison is diazobenzol, and it can be prepared artificially by the action of nitrous acid on aniline. If some normal milk be inoculated with poisonous milk, cheese or cream, and the whole be kept for a few hours at the temperature of the body, or for a few days at the ordinary in-door temperature, the whole becomes poisonous.

The action of this poison on the lower animals has been studied, and it has been found to produce symptoms which agree closely with those of cholera infantum. The administration of from 10 to 25 milligrams to a cat causes, after a time, which varies from a few minutes to two hours or longer, nausea, retching, vomiting and purging. The stomach becomes very irritable, and attempts to take food, and in some instances even a sip of water starts the vomiting again. The first stools contain fecal matter, but the subsequent ones are rice-water-like in appearance, alkaline in reaction, and devoid of fecal odor. The pulse becomes rapid and feeble, and marked exhaustion soon follows. The daily repetition of this small dose prolongs these symptoms, and after a few days the animal dies of exhaustion.

With a larger dose, from 25 to 50 milligrams, these symptoms are intensified, and in addition the breathing becomes shallow and labored. With still larger amounts, from 75 to 100 milligrams, there is violent retching, rapid and feeble pulse, but neither vomiting nor purging, and death within from one-half to two or three hours.

Post-mortem examinations show the mucous membrane of the stomach and small intestines usually blanched, soggy, and covered with a thick, ropy secretion. In some instances the mucous membrane of these parts appears quite normal. When the smaller

* *Zeitschrift für physiologische Chemie*, B. x, H. ii.

† See article read before the Section on Pathology of this Congress.

Digitized by Google

doses, referred to above, are used, the pale appearance of the mucous membrane of the stomach and intestines is the only deviation from the normal observed. Where the larger amounts are employed, the lungs are found markedly congested. This probably arises in part from the violent retching, and in part from the feeble action of the heart.

That these symptoms and post-mortem appearances agree very closely with those observed in the violent choleriform diarrhœa of children all will, I think, admit, and that the symptoms of this disease resemble those produced by some powerful poison must be evident to all who have been engaged in its treatment. The suddenness and violence of the attack, the rapid exhaustion of the little patient, the inefficiency of medicines, and the speedy dissolution, all indicate the action of some potent poison. Now, we know that a substance capable of producing these effects is generated in milk under certain conditions of putrefaction.

The blanched, soft condition of the mucous membrane of the stomach and intestine found in cats, after poisoning with tyrotoxicon, corresponds exactly with the state of the same organs in children after death from cholera infantum. The congestion of the lungs noticed in the lower animals is also present often in the severe forms of the disease, and in very rare instances we see children die in a few hours after violent retching and with pulmonary congestion, but without either vomiting or purging, just as the animals died after very large doses of the poison were administered.

The above stated facts constitute the evidence upon which we form the opinion that tyrotoxicon is frequently concerned in the causation of cholera infantum, the violence of the attack varying with the amount of the poison present.

With these facts before us, we can easily understand why this disease is most prevalent among the poor classes in the large cities. To them fresh, wholesome milk is almost unknown. They buy, generally, of the small dealer, who has kept the milk, often in filthy cans, in a foul atmosphere and at a high temperature. In the homes of many of the poor these conditions, so favorable to putrefactive changes, are also present, and often in an intensified form. Even among the well-to-do, insufficient attention has been given to the milk upon which the children depend for the whole or the greater part of their food. The same is also true in country places. Cows stand in filthy barns and are furnished only stagnant water to drink. Then there are the tens of thousands of children who must draw their sustenance from nursing bottles, the cleansing of which, in many cases, is very inadequately attended to.

I think that one of the greatest advantages secured by nursing from the breast consists of lessened liability of the milk becoming infected with germs which produce poisonous ptomaines.

The exact germ which produces tyrotoxicon is not known, but it is either the butyric acid, or some closely allied ferment. It is a true putrefactive germ, and grows best when it is denied a free supply of air. The conditions most favorable for its development are the exclusion of air and a temperature approaching that of the human body.

I have elsewhere* presented some rules concerning the care necessary to prevent milk undergoing these putrefactive changes, and will ask that I be permitted to repeat them here. They are as follows:—

1. The cows should be healthy, and the milk of any animal which seems indisposed should not be mixed with that from perfectly healthy animals.

2. Cows must not be fed upon swill, or the refuse of breweries, or glucose factories, or upon any other fermented food.

3. Cows must not be allowed to drink stagnant water, but must have access to pure, fresh water.

4. Cows must not be heated or worried before being milked.

---

* *Medical News*, June 18th, 1887.

Digitized by Google

5. The pasture must be free from noxious weeds, and the barn and yard must be kept clean.

6. The udders should be washed, then wiped dry before each milking.

7. The milk must be at once thoroughly cooled. This is best done in the summer by placing the milk can in a tank of cold spring water, or ice water, the water being of the same depth as the milk in the can. It would be well if the water in the tank could be kept flowing; indeed, this will be necessary, unless ice water is used. The tank should be thoroughly cleaned every day to prevent bad odor. The can should remain uncovered during the cooling, and the milk should be gently stirred. The temperature should be reduced to 60° F. within an hour. The can should remain in the cold water until ready for delivery.

8. In summer, when ready for delivery, the top should be placed on the can and a cloth wet with cold water should be spread over the can, or refrigerator cans may be used. At no season should the milk be frozen, but no buyer should receive milk which has a temperature higher than 65° F.

9. After the milk has been received by the consumer, it should be kept in a perfectly clean place, free from dust, and at a temperature not exceeding 60° F. Milk should not be allowed to stand uncovered, even for a short time, in the living or sleeping rooms. In many of the better houses in the country and villages, and occasionally in the cities, the drain from the refrigerator leads into a cesspool or kitchen drain. This is highly dangerous; there should be no connection whatever between the refrigerator and any receptacle of filth.

10. The only vessels in which milk should be kept are tin, glass or porcelain. After using the vessel, it should be scalded, and then, if possible, exposed to the air.

When these rules are put into operation, milk can be preserved free from putrefactive changes for a reasonable length of time, and it will remain fresh and palatable. When such care is not exercised, the milk may become, as we have seen, highly poisonous within a few hours after it is drawn from the cow. When we take into consideration the fact that many children must feed exclusively or largely upon this milk, we certainly cannot regard the time and expense which the above rules demand as of great moment.

But when milk is to be transported a great distance or kept for any length of time, we will find the same necessity for drying it and the preservation of its solids, that there is for the curing of meat or the canning of fruit. This evaporated milk may be used as it is in the "condensed milk" of trade, or it may be used in the preparation of a milk-food, which we will discuss under our second question, and for which we are now ready.

Can the milk of the cow be rendered more nearly like that of woman and more fit for food for infants than it is by simple dilution with water and sweetening with milk-sugar? Woman's milk differs from that of the cow in the following particulars: (1) In the proportion between the casein and the other albuminous constituents, and in the nature of the casein. (2) In the amount and probably in the nature of the milk-sugar. There are other minor differences, but those mentioned are probably the only ones of importance to us in the present discussion. Physiological chemists agree that there is less casein and more albumen in woman's milk than in that of the cow. König* gives the following figures, as the averages of a great many analyses, concerning the nitrogenous constituents of the two kinds of milk:—

	Total Nitrogenous Constituent.	Casein.	Albumen.
Woman's milk,	2.36 per cent.	0.59 per cent.	1.23 per cent.
Cow's milk,	3.41 "	2.88 "	0.53 "

* Nahrungs- und Genussmittel, Zweite Auflage, S. 252.

Digitized by Google

Indeed, Radenhaussen* claims that woman's milk does not contain any casein at all, but that its nitrogenous constituents consist wholly of albumens and peptones. Kirchner† has shown that the albuminose or galactin of Bouchardat and Quevenne, the lactoprotein of Millon and Commaille, and the whey protein of Hammersten, are all identical and are peptones. The same investigator concludes that the difference in the digestibility of woman's and cow's milk is due to the large amount of peptone in the former. Moreover, as was first shown by Biedert, the gastric juice coagulates cow's milk in large, hard clots, while by its action upon woman's milk it produces a flocculent precipitate. These facts can leave no doubt that there are differences between the albuminous constituents of woman's and those of cow's milk, and it is altogether reasonable to suppose that these differences influence the digestive organs. In many children, digestion is so good that cow's milk does not tax the stomach. For these children, artificial digestion is not only not necessary, but not desirable. But I cannot see why the casein of cow's milk should not be partially digested for those children in which unaltered milk produces unpleasant digestive disturbances. I say "partially digested," and that it should not be completely digested, I am certain. It is unscientific to feed any one for any length of time upon completely digested food; especially is this true of children. To relieve the action of the gastric juice altogether is to diminish its secretion. The bones, muscles and brain are weakened by inactivity, and the stomach is no exception to this rule. It must have something to do or it will soon be unable to do anything. There may be, and doubtless are, exceptional cases, in which the temporary administration of peptones exclusively is desirable. But these are exceptional cases, and the administration of the completely digested food should be only temporary. Certainly these cases do not include healthy children.

In the preparation of a milk food, the milk should be partially digested by the action of pancreatin, then boiled to destroy the ferment and prevent its further action, then evaporated in vacuo. To the mild solids thus obtained, dextrinized flour, obtained by roasting, should be added. In this way, the extra carbohydrate, above that present in the cow's milk, will be obtained, and this carbohydrate will, in some important respects at least, resemble the sugar of woman's milk. According to Biedert, the sugar from cow's milk ferments more readily than that from woman's milk, and certainly glucose ferments more readily than milk sugar obtained from either of these sources.

Such a food as this prepared in a dairy district might be transported any distance and kept for any reasonable length of time without decomposition. It would be sufficiently nutritious in itself, and the addition of milk to it would be both unnecessary and undesirable. It would not contain any vegetable matter difficult of digestion, and it would contain a sufficient amount of inorganic salts, in which many of the artificial foods now so largely used are greatly deficient. Especially is this true of the salts of lime.

Upon my third topic, I will be very brief. The question is asked: Is it desirable to continue the use of milk in cholera infantum and kindred diseases? The following simple experiments may aid us in answering this question. If several four ounce bottles be filled, one with good milk, a second with the same milk to which a few drops of another milk in which tyrotoxicon has been generated are added, a third with a meat peptonoid preparation inoculated with a few drops of the poisonous milk, a fourth with beef tea or rice water inoculated with the poisonous milk, and if these bottles be placed in an air chamber and kept at the temperature of the body for three hours, no tyrotoxicon will be found in the pure milk or in the meat preparation, while it will be found in the milk which was inoculated with the poisonous ferment. Why the germ

‡ *Zeitschrift für physiologische Chemie,* B. V., S. 13, u. 256.

† Beiträge zur Kenntniss der Kuhmilch und ihrer Bestandtheile.

Digitized by Google

does not grow as rapidly in the meat preparations as in the milk is not a matter to be discussed here. The fact is all that we need, in giving a practical answer to the question which we have asked. It shows that this poison is not generated as rapidly in the meat preparations as it is in the milk.

A few of the best clinical teachers have recommended that the use of milk be prohibited in the treatment of cholera infantum. They ascertained the advantage thus secured by experience. Now, I think that we have the true explanation of the value of this treatment. To give the best of milk in cholera infantum will be a means of supplying the germ which is present in the alimentary canal with material from which it will elaborate the poison. But I have elsewhere discussed at some length the nature and treatment of cholera infantum, and it would be outof place to discuss that subject in full here.

I have only one more thought to offer. It may be embodied in the following question: Why do the diarrhœal diseases of infancy prevail most extensively in filthy localities? Because the ferment which leads to putrefactive changes in milk abounds in filthy places. It has its home in the fermenting filth of street gutters; it is present in decaying pavements, and it is carried by every breath of air that comes in contact with decaying, souring, vegetable matter. Then, although the food of the child may be above suspicion, so long as its surroundings are filthy, so long as it inhales the germs of putrefaction, poisons may be generated in its body.

I have stated that the ferment which produces tyrotoxicon is either the bacillus butyricus or some closely allied germ. We know that the butyric acid ferment does grow and thrive often in the human stomach.

Then to give our practical conclusions, we may say: (1) Cow's milk used as a food for infants must be taken from healthy, properly fed animals. (2) It must be preserved from putrefactive changes while being transported and stored for the child's use, and (3) After having been taken into the child's stomach it must be preserved from putrefactive changes, and this cannot be done so long as the child is overfed and breathes an atmosphere laden with the germs of putrefaction.

DISCUSSION.

THE PRESIDENT asked the writer of the paper whether he had noticed if the animals experimented on by the tyrotoxicon, and presenting symptoms similar to those of cholera infantum, had elevation of temperature. In this disease the temperature is always elevated, and in the severe form of it, accompanied by frequent stools and vomiting and rapid prostration, the temperature rises to 105° or 106°.

Prof. VAUGHAN replied that he intended to give attention to this point, but had not thus far.

THE PRESIDENT also spoke in reference to the anatomical characters observed. Although if death occurs after a brief attack of cholera infantum, as twelve or twenty-four hours, the gastro-intestinal surface is pallid, yet if a longer time has elapsed, as three or four days, it is usually injected. He could not avoid the conviction that the disease is inflammatory in its nature, although a different opinion had been expressed by good observers.

Dr. VAUGHAN replied that he had not observed whether those animals experimented on by the tyrotoxicon had elevation of temperature, but thought it not improbable that animals poisoned by it, if they lived sufficiently long, would present the symptoms and lesions of gastro-intestinal inflammation.

Digitized by Google

SOME OBSERVATIONS ON HEADACHES IN CHILDREN, AND THEIR RELATION TO MENTAL TRAINING.

QUELQUES OBSERVATIONS SUR LES MAUX DE TÊTE DES ENFANTS ET LEUR RAPPORT À L'ÉDUCATION MENTALE.

EINIGE BEOBACHTUNGEN ÜBER KOPFSCHMERZEN BEI KINDERN, UND IHRE BEZIEHUNG ZUR GEISTIGEN AUSBILDUNG.

BY WILLIAM HENRY DAY, M.D.,

Member of the Royal College of Physicians of London; Physician to the Samaritan Hospital for Women and Children.

It is an honor to be invited to contribute a paper to this great International Medical Congress. When I reflect on the readiness with which our transatlantic brethren responded to a similar appeal from the mother country in 1881, I feel that there is a mutual dependence, close and intimate, which augurs well for the advancement of the work in which we, as members of a great profession, are severally engaged. Distance no longer divides us. The wide sea is no barrier to our intercourse. A ceaseless activity, a laudable competition everywhere prevails to promote the ends of science, and the distant parts of this busy, struggling world are brought closer and closer to compare the labors of fellow workers at home and abroad.

In all I have to say, I shall hope for your lenient criticism, which ever lightens the weight of responsibility. No man undertakes a light task, in the present day of active and unceasing work, when he attempts to elucidate, or throw a further gleam of light upon, any topic in scientific or practical medicine; still, even the smallest fact is important, and is often the starting point of some great achievement in science. Every thoughtful investigator contributes to the general stock of knowledge, in however small a degree, and gives a fresh impetus to research. Great truths sometimes have their origin in great errors.

The great work of national education in this country, which dates from the passing of Mr. Foster's Act in 1870, has had the effect of calling widespread attention to the excessive demands made by schools and teachers upon children. In nearly all European countries, among the poor and rich alike, abundant proof has been adduced that over-pressure is present in all. The rate of progress is stimulated by a universal rivalry which no protest can check.

In every branch of the public service, and in every profession the competition of the entrance examinations is becoming more and more severe. This exhausts the nervous system during the most active period of physical growth, when it is so important not to overtax it. Mere cramming for a particular examination can never favor intellectual progress; it does not cultivate the reasoning and reflective powers; it is simply getting together a certain number of figures and facts which are not long remembered, and serve no useful purpose beyond the hour. To say nothing of the immediate disastrous effect of this system in encouraging excessive brain activity in delicate and underfed children, it will, in the course of time, favor a great increase in nervous diseases among the community at large. The evil seed has been sown in their youth, and when they reach maturity, and a robust constitution is indispensable for the duties of life, they will probably fail in endurance, for the push and pressure thrown upon them in early years will prevent any but the strongest discharging responsible work as it should be discharged, and will thus entail national loss.

I cannot but think that the excessive strain which teachers often produce is as much caused by their want of a sound training for the work they attempt as by their use of too ambitious a curriculum. Teachers, as a body, do not understand the physiological

Digitized by Google

and psychological natures of children; and until they do, there will always be the twofold danger of attempting too much, or of attempting it in the wrong way and by the wrong means. But I will not now enlarge upon this topic.

The physician recognizes in this severe ordeal grave danger to the brain and nervous system of growing children, by which the mind is narrowed, the character weakened, and disease invited. Headache is one of the first and most persistent consequences of this baneful practice.

I shall not speak of those headaches which are the mere accompaniment of acute and febrile diseases, or due to some pathological state of the brain itself, but of headache incidental to childhood and to the period of active growth and change, commencing about the second dentition (particularly from 12 to 15 in girls), when school work begins to be keenly felt. This headache constitutes a disease in itself, a real illness; it is sometimes so persistent that the child is often obliged to give up his studies in consequence. But in any variety of headache we must not lose sight of the temperament and predisposition to nerve disturbance. In some children, as in some adults, there is always going on an excessive expenditure of nervous energy; in their pleasures and in their amusements, in their studies, or pursuits of any kind, there is the same wear and tear, because they are unable to control themselves, and have not the power of self-management when extra strain is put upon them. The balance is upset, and they cannot readjust it. But if we take a less susceptible class of children, another class whose physical and mental organization is stronger, who are sturdy and not emotional, they are less moved by any of the vicissitudes of life, they pursue their studies regularly and quietly, and when they begin to feel tired they can calmly lay them aside before they are too exhausted or have passed the limit when they cannot check themselves. Rest soon reinforces these children, and they return to their studies with their strength restored. They may go through the whole curriculum of study, or even pass through life, and never experience a headache at all. The brain is more strongly developed, and the vital organs are also in a sound state, and air and active exercise are far more congenial to these subjects than is intellectual work. Not that they wish to escape the latter, but the mind never dwells on it when not engaged in it, and they take their school work fairly in turn with their out-door amusements, so that the balance is harmoniously maintained.

HEADACHES

In the young are of greater significance than in the adult, and should receive the utmost care in investigating their nature and origin. They may be slight or severe, superficial or deep-seated; they may occupy the forehead, the vertex, the temples, or the occiput. The pain may be dull or sharp, gradual or sudden, continued or paroxysmal. These headaches may last a few hours or continue for months; they may destroy the happiness of a lifetime, and in old age depart; they may be associated with disordered sensation or disturbance of vision. Nearly all seem to owe their essential origin to disturbance in the nervous system before the vascular area is involved. Mental and emotional excitement can originate active congestion in the brain as readily as it is induced by cardiac hypertrophy or a general plethora. Passive congestion is favored by any exhausting cause.

Dr. Ferrier, in speaking of brain and mind, says, "that the degree of development of the frontal lobes is characteristic of the mental powers, the richly convoluted brains belonging to the highest intellects, and the poorest to the inferior intellects." It also appears from his statements that the slighter development of the frontal lobe convolutions is more especially marked in the female brain, which resembles, in this respect, the fœtal brain in its later stages of development. There are male brains which approach the female type, and female brains which approach the male type. As a rule, the convolutions are better developed in all the lobes when the frontal convolutions are

Digitized by Google

especially complex.* When we come to consider the actual size of the brain in the two sexes, we find that the average weight of the adult male brain is five ounces greater than the average weight of the female brain;† but if the size of the body be taken into consideration, the disproportion may not be great. After all, the actual size of the brain does not account for the difference of intellectual power between men and women. It is not, as Ferrier observes, a mere matter of brute weight or quantity, though a brain below a certain standard of weight is not compatible with normal intelligence.‡ According to the investigations of Sir Crichton Browne, the gray matter of the female brain is shallower than that of the male, and the supply of blood is proportionally less. These differences he dates from embryonic life, and comes to the conclusion that they constitute a "fundamental sexual distinction."

CAUSES OF HEADACHES IN CHILDREN.

The activity of the brain cannot be maintained without blood. It must be well nourished. A certain quantity of blood is indispensable to the performance of its healthy functions. If it be deficient in arterial supply, then its vitality is lowered, the mind is impaired, and the thoughts are confused. Not less important is the quality of the blood. If it be impure and deficient in nutritive qualities, it invites nervous change. Slight mental effort or violent muscular exertion will induce this, if the blood is not purified. The lungs, the liver, the bowels, must be in a condition to purify the blood. These facts are sometimes overlooked in searching out the cause of headaches.

I have, in a paper on "Headaches in Children,"§ referred to excess of blood in the brain (*cerebral hyperæmia*) and to deficiency of blood (*cerebral anæmia*). These two factors are most necessary to keep in view, for the cerebral circulation is greatly influenced by the relationship that exists between the nerves and blood vessels. They comprise the two great types of headache among school children, and among children generally.

The brain fails early in some children while in others it gathers strength as years advance. In childhood, when growth is actively proceeding, the constitution is so impressible that there is a remarkable tendency to nervous trouble; rapid development of organs disturbs the excito-motory system, and at this period of life there is great liability to disease. This predisposition being once established, almost any influence may excite nerve disorder.

Heredity greatly predisposes to nervous headaches in children, just as it does to gout and cancer in later life. It is handed down from parent to parent, sparing those of the offspring who may be vigorous, but striking down the susceptible and weak. The expression is familiar, "I take after my father," or, "I take after my mother." It seems probable that in certain structures of the tissues there is an especial liability to take on certain diseases.

The habits and education of some children are just of that kind to invite nerve disorder. Change these circumstances, and it no longer exists.

The class of sufferers, then, belong to those of a nervous temperament, who are easily moved to excitement or depression; they are restless, active, giving all their mental force or energy when required. These children are liable to anæmic headache and brain disturbance, not to congestion of organs and plethora so readily as those of the sanguine temperament. Maternal health during pregnancy has been recently shown by Dr. Langdon Down to have a great influence in the production of idiocy in the child.

---

* "The Functions of the Brain," 1886, p. 467.

† "Carpenter's Physiology," 1876, p. 736.

‡ *Op. cit.*, p. 467.

§ Read before the Harveian Society, Feb. 2d, 1882.

Digitized by Google

Sickness, uterine hemorrhage, emotional disturbance and grave febrile disorders lead, in the offspring, to epilepsy and chorea. In congenital idiocy, heredity is a great factor, neurotic disease being fearfully transmissible. Prolific child-bearing tends to produce feeble-mindedness in the children; violent shocks of all kinds, distressing news and severe emotional disturbance to pregnant women have weakened the mental powers of their offspring. Dr. Down's inquires lead to the conclusion "that the mental condition of the embryo took its impression from the mental condition of the father at the time of conception." The "over-education of women, when only the emotional side of their nature is cultivated, leads to the production of feeble-minded children."*

Careful inquiry will now and then elicit a history of syphilis in the parents, or some form of nervous disease in them, or of convulsions in infancy, or of such inherited tendency before birth. There may be, and there often is, a combination of causes to invite nerve disturbance, as feeble health, fatigue, mental shocks, and the want of food. No explanation has yet been forthcoming to account for the predisposition to some diseases, but we do know, where it is latent, how readily they are invited; what a trifle will disturb the balance and originate changes of function and alteration in structure. What do we know regarding the obscure diseases of the nervous system in children, such as tetany, or chorea, or what of certainty about epilepsy? The phenomena vary in each disorder. That they are due to some derangement of the nervous system, or to some irritation acting on excitable nervous structure, cannot be gainsayed; but what more do we know? I have known children who suffer from periodical headaches to exhibit symptoms not unlike an approaching attack of epilepsy—perhaps epileptic in their nature, but falling short of the culminating point.

OVER-PRESSURE.

I will endeavor to look upon this question in the most impartial manner, and to arrive at no hasty conclusion. The agitation concerning "over-pressure," in all parts of the world where the old system has given place to the new, would not have arisen without cause. It is not limited to the United Kingdom; it comes from nearly every country in Europe. We must admit that enthusiasts are prone to exaggerate when they witness a few cases of break-down, and fail to remember the thousands of children who pass through their school life happier and better for the regular hours and discipline that is maintained. Still, the outcry is real; and if it draws attention to the evil where it exists, improvements will inevitably follow, and cases of over-strain will become less frequent.

It will be conceded that no system of education, however carefully conducted, will meet the requirements of all children. The State cannot legislate for individuals. The child of sanguine temperament and robust constitution is happy with his educational training, and he can throw off any feeling of strain the moment he gets into the playground, and forget all about it; but the child of nervous temperament is too anxious and miserable at the idea of failure. He is defeated by his own anxiety. This applies more to girls, whose leisure hours are very differently passed. They are far less active than the opposite sex, who throw their whole thoughts into cricket or other amusements, which girls cannot always do. Girls are by nature more nervous and impressionable; the mind is more easily over-strained, and they brood over their tasks long after they are set aside, which is so much wasteful expenditure of nerve force.

Mr. Romanes, in an interesting article on the "Mental Differences between Men and Women,"† has collected evidence from High School girls who are studying with a view of competing for first classes at Cambridge. Many work habitually ten or eleven hours

---

* "Lett. Lect. B. M. I.," 1887, Jan. 22d, pp. 149, 150, 151.

† *The Nineteenth Century*, May, 1887.

Digitized by Google

a day, and near the time of examination from fifteen to eighteen hours. The power of endurance in these girls is proved beyond doubt, but such over-pressure is often the prelude to an early break-down.

The experience of every-day life assures us that but few men attain to eminence, or are capable of sustained effort in any calling, if they are of delicate constitution. The sound mind in the sound body is indispensable. Broadly speaking, those who have had a happy and healthy childhood are the most successful competitors in the race of life.

The intellectual type of boy, endowed with a strong brain, is not easily overworked. He possesses the instinct of knowing how to take care of himself, while the intellectual boy with a feeble brain and over-zeal to accomplish his work soon fags and degenerates in power. He is not well balanced in mind or body, and lacks the capacity to meet the demands made upon him without feeling too acutely the worry and strain. But those who are dogged and indolently inclined, who are indifferent, and without ambition for success in life, never put forth their efforts. This type is probably the exception in the female sex; moreover, being physically weaker than boys, they are mentally more liable to break down. It has been truly said, that many persons spend more hours in thinking over the work they have to do than it would take them to do it twice over. Deficiency in energy of will is often physical at bottom.* Nowhere has a more correct statement been made than that the mental force of girls tends, in a large number of cases, to dissolve into emotion.†

It has been proved, however, both in this country and in America, that young girls are more acquisitive than boys of the same age, and there is stronger power of perception, but as adolescence is reached, and the brain is more fully developed, boys show greater originality of mind and power of amassing knowledge.‡

Headaches, I am convinced, have become of more frequent occurrence among poor children, and deaths from meningitis are more on the increase, since the Education Act came into operation. I have met with cases of meningitis in school children in whom headache was neglected, and the initial and curable stage allowed to pass by. If this mischief be invited by over-pressure, we may justly and indignantly protest against a system which exacts harder lessons than the child can well accomplish, and against the misguided effort to maintain his attention.

Where, for economy's sake, very large numbers of children have to be taught together, it is impossible, even with the best system, to prevent the weaker and more delicate children from suffering. Still, we have overwhelming testimony from medical men and teachers, that infinite mischief to the mind, to the eyesight, and the general health of the scholars is frequently caused by the increasing requirements of the Educational Department. The Act of 1870, which only provided for Elementary Education, consisted of reading, writing, arithmetic, and needle-work; but since then other subjects have been added, namely, geography, elementary science, history, Euclid, algebra, mensuration, chemistry, physics, botany, physiology, Latin, French, etc. Although these subjects are optional, and only a few are allowed to be taken up at one time, the spirit of emulation that is encouraged too often tries the strength of the children beyond endurance, and makes too great a demand upon the tender intellect. Children who are poorly fed and poorly clothed, or who are delicate, cannot be expected to accomplish much varied intellectual work without an early break-down, especially when they come, as many of them do, from wretched homes, where a family of five or six frequently live in one room.

The facts obtained by Dr. Hertel from fourteen schools in Copenhagen show that out of a total of 3141 boys, one-third of the number is made up of sickly ones, and

* "Over-work, from the Teacher's Point of View," by Mrs. S. Bryant, D. Sc., 1885, p. 6.
† *Idem*, p. 8. ‡ *Op. cit.*, *19th Century*.

Digitized by Google

that most of these children enter on school life with impaired health. That this is a very grave state of things must be admitted, such children being incapable of much intellectual exertion, at a period when all their force and vitality are needed for physical development and when outdoor life is so essential. At about the age of twelve, when the children pass from the mixed classes into the modern division, the percentage of sickly children rises in the first modern class to 38.8, and in the second classical forms to 41.9. Dr. Hertel remarks that the increased sickliness in both instances is owing to the development into manhood, commencing about the thirteenth year. There is a fall in the sick percentage in the higher forms.

From nine to twelve, according to Dr. Kotelman, the increase in height is 5.71 inches, the increase in weight 19 pounds; from thirteen to sixteen the increase in height is 9.37 inches, the increase in weight 44 pounds; from seventeen to twenty the increase in height is 2.43 inches, the increase in weight 23 pounds. These investigations prove that when physical development is most active, at thirteen, or thereabouts, and growth is rapidly proceeding in bone and muscle, if the mind is forced to such an extent, the evil consequences may be permanent.

Reviewing all the facts which he has collected, Dr. Hertel arrives at the conclusion that 20 per cent. of the children are sickly on entering the Danish schools; after two years they advance to 30 per cent., and shortly before puberty they suddenly rise to 40 per cent. This shows what an extraordinary influence school training, with the change of habits, has upon the health of young persons during the period of life when growth is making its most supreme efforts. The outdoor life which is so natural and congenial to boys, is exchanged for a sedentary and thoughtful occupation. Such a contrast cannot take place without an influence on their health and spirits.*

VENTILATION

Is a matter requiring careful consideration. I am sure that defective ventilation of school-rooms is a most fertile cause of headache. If the air of these rooms, or any inhabited apartment, is impure, in consequence of too many persons being crowded together, so that the products of respiration or uncleanliness from any cause contaminate the atmosphere they breathe, it is injurious to the health, and the nervous system is sure to suffer. We can form some idea of this from the close atmosphere of theatres and ball-rooms, and the headache induced by them. Although the air of any room where a large number of persons assemble cannot possibly be as pure as the outer air, mechanical contrivances can be adopted to render the atmosphere of any apartment moderately pure. In many of the best arranged dwelling houses of London no sensible difference is experienced on entering them. There is no disagreeable odor; but, enter them when they are brilliantly lighted and the space is crowded, and even with the best appliances a closeness is felt if the carbonic acid in it reaches seven or eight per 1000 volumes. If it reaches nine or ten per 1000 volumes it is "close and gusty."† It seems to me headache is as often the result of a polluted atmosphere as of the lessons imposed on the pupils.

It is indispensable that wherever large numbers of children are assembled together there should be a constant supply of fresh air, in order that the atmosphere may be kept pure and sweet. The rooms should be frequently disinfected to destroy the organic emanations. Our board schools, being of modern structure, are well adapted to insure this supply of purer air, and every care has been taken, from a sanitary point of view, that the health of the children should not suffer, but the proper use of the sanitary means must rest with the teacher. Persons who live habitually in an impure

* "Over-pressure in High Schools in Denmark," 1886, pp. 23–33.

† "Parke's Manual of Hygiene," 1873, p. 134.

Digitized by Google

atmosphere breathe noxious gases and vapors, without a knowledge of their existence, their olfactory organs having lost their normal sensibility. It is only when persons coming from the open air enter impure dwellings that they are fully aware of the state of things.

NERVOUS HEADACHES.

A weak cerebral circulation, whether arising from general debility or from heart weakness, invites nervous headache in those predisposed to it. If a child or an adult is wanting in constitutional strength and vigor, nerve disturbance will ensue when he is overtaxed by intellectual effort or by any exhausting illness. To unravel the relationship existing between the mind and the body would demand a philosophical inquiry, into which I will not venture. The brain is a complicated organ, and some of its diseases are not explained by a minute examination of its structure after death.

The mental functions are in some degree dependent upon material organization, and are closely related to it. Intimate as is the alliance, dissection of the nervous substance does not clear up the difficulty; still, we are able to compare how the one acts and reacts upon the other, and of the connection that exists between them.

All cases of nervous headache in school children cannot be traced to mental strain. If the brain be normal, and the general health good, no danger will result from carrying out the educational code; but in weakly and sensitive children the foundation of serious illness may be laid by enforcing a degree of intellectual exertion which a healthy child could bear with impunity.

Headache is one of the first symptoms that arises. As soon as it declares itself the whole aspect of the child is altered, and he is engrossed with his own sufferings. Melancholy takes the place of cheerfulness. He is kept at work, because his ailment is not noticed by his teachers or by his parents. With this state of things, the child soon becomes anæmic, loses his appetite, and the headache, which at first was *occasional*, becomes habitual. Dr. Treichler states that one-third of the pupils at Darmstadt, Paris, and Nuremberg suffer from it.* He considers the cause to be over-intellectual exertion entailing work at night, and taking up a variety of subjects.†

The anæmia produced in these cases sets up disturbance in the ganglion cells of the cortex of the cerebrum, the quality and quantity of the blood become changed, and the brain loses energy and activity. The mind is consequently enfeebled, and the mental faculties are clouded. When this is once brought about, children become emotional and excitable; hence the cerebral vessels become too suddenly filled with blood to bear it with impunity, and this dilatation of the vessels (local hyperæmia), through vasomotor disturbance, is another common cause of this form of headache; the vessels of the brain are partially paralyzed, and the ganglion cells too exhausted to grasp new ideas, as Dr. Treichler observes.

Of 7478 boys and girls examined by Professor Bystroff, in the St. Petersburg schools, during five years, ending in the spring of 1886, he found headache in 868, or in 11.6 per cent. The percentage increased with the ages of the children and with the number of hours occupied in study; thus, it occurred in only five per cent. in children eight years of age, while from fourteen to eighteen it attacked from 28 to 40 per cent. The author attributes these obstinate headaches in school children to excessive mental labor.‡

Similar experience is recorded by Dr. Hertel. He found headaches increase as the oldest classes were reached. In the lower mixed classes it was 5 per cent. In the two highest classes it rose to 38 per cent. This class is the most exposed to headache and bleeding at the nose—proofs, as he justly states, that mental work favors

---

* *Intellectual Times*, April, 1880. † *Ibid.*

‡ "On Headaches in School Children," *Brit. Med. Journ.*, May, 1886.

Digitized by Google

congestion of the brain, and I would add constipation and the troubles resulting from a sluggishness of the bodily functions. I am frequently meeting with boys and girls in whom headaches, nose bleedings, and muscular twitchings have followed studious application to books. The same holds good with respect to the frequency of nervousness and anæmia.†

FRONTAL HEADACHES.

The most frequent form of headache from intellectual work and overstrain is pain across the forehead, generally the whole forehead, and especially along the superciliary ridges. There is a dull, oppressive weight; it is felt on waking and increases during the day. *Any reading or intellectual exertion* involving close attention, reasoning or reflection, *increases the pain*, and the child will often be seen leaning his head forward on his hands, oppressed with suffering. *The forehead, which at first is cold like the extremities, becomes hot after an interval*, the temples throb, and the child cannot bear the light. There is tension and fullness in the cerebral vessels. The appetite is generally poor, or is absent altogether, frequently the tongue is furred and the bowels are torpid. The face at first is pale and drawn, then it may become flushed, and vomiting, which brings relief, may then ensue. I have known such children become very torpid and listless from this form of congestive headache, and avoid their companions. They sleep and dream heavily, often of their tasks, while in other cases they become quite changed in character, and are nervous, restless, and irritable. Here the brain is rather *irritable* than congested. *Such children are sometimes furious in temper if any opposition is offered to their wishes*, or they are low-spirited and will not utter a word unless spoken to. If a girl, she may be so emotional as to burst into tears on the slightest provocation. Sometimes any intellectual effort, however easy and however much sought after by the pupil, cannot be accomplished without the recurrence of headache; even reading aloud to another person, and the small attention given to it by the pupil will bring on the pain. The study of mathematics, or anything involving much strain, will make the suffering all the greater.

There is sometimes a disproportion, as I have hinted, between the cerebral development of the child and the intelligence he possesses; just as the muscular system suffers pain and weariness when it is imperfectly nourished.

If the headache is moderate, the child, either of his own accord or at the desire of his teachers, continues his work, but it is a trouble to him, for he is taken up with his own suffering, and the mind is so unhinged that he soon forgets what he has learned. There is no disguise with one who is really suffering from a genuine congestive headache; he can generally be recognized; his whole manner bespeaks pain, and he cannot change his thoughts, the least movement increases the pain, and he desires to be alone.

NEURALGIC HEADACHE (ONE-SIDED HEADACHE)

Is not of frequent occurrence among children as a consequence of school work. It is rarely traceable to this cause, yet it is sometimes met with. Whenever it is complained of the teeth should be carefully examined, as a carious tooth will often be found to cause the trouble, though in anæmic and exhausted children the pain may continue a long time after the removal of the tooth. People of all ages may have carious teeth, and yet only suffer headache or facial neuralgia when the health is reduced. Such sufferers belong more especially to the neurosal temperament, the nervous system becoming lowered after intellectual exertion. The most striking cases in my experience have been in delicate constitutions, and among half-starved children, who live in wretched homes and who are deprived of fresh air and wholesome food. After long and painful illnesses I have known it very severe. The pain is always paroxysmal, coming and

† *Op. cit.*, p. 36.

Digitized by Google

going when least expected. It is almost invariably unilateral, affecting one or other of the branches of the fifth nerve on one side only. There are certain painful and tender spots in the course of the affected nerve, and sometimes hyperæsthesia. When severe, the face betokens terrible anguish and despair. The patient cannot bear the noise of other children and becomes fearfully irritable under it. In the intervals of pain he is cheerful and energetic. He seldom vomits. In a case of this kind there may be congestion of the conjunctivæ, lachrymation, throbbing of the temporal arteries, frequent pulse, and sometimes, though rarely, so much vomiting that the child is thought to be bilious. In 1881 I saw a little girl, eleven years of age, who had been a victim of it from four years of age. It followed an attack of acute tonsillitis and enlarged cervical glands. In another case it was a sequel of whooping-cough and diphtheria. During the second dentition, when the teeth are slow in appearing, neuralgic headache involving the branches of the fifth nerve, as they ramify over the brow and temple, is sometimes observed in boys and girls. I have known attacks of facial neuralgia frequent in young, anæmic and excitable girls, who exhaust themselves by great mental application and insufficient rest and relaxation. They throw off the acuteness of the seizure by resting for a few days, but the pain returns when school work is resumed.

IRRITABLE BRAIN.

An occasional effect of school work is to produce an irritation of the brain, in other words, an excessive and morbid sensibility, which may be followed by congestion and meningitis. I have elsewhere referred to simple cerebral irritation as a functional disorder in very young children, independently of any association with congestion or inflammatory exudation. I have spoken of an "irritable diathesis" in these cases, and, although the pathology is obscure, I believe that anæmia of the brain from deficient circulation is the primary change.*

Children so affected are of nervous and anxious temperament; they are delicate in constitution, sometimes rickety, or reduced by diarrhœa, or by the eruptive fevers. Delayed dentition, the presence of intestinal worms, a slight blow on the head, or exposure to the sun or cold may invite it. But whatever the cause, the nervous system is weak and unstable. The most typical cases to which I have drawn attention are in young children from two to five years of age, before they have gone into the school room, when one or other of the causes to which I have alluded would be in operation. As a consequence of school work, I have met with it in boys and girls. In one case a boy eleven years of age suffered from this state of brain. He complained of headache after school examinations, but it never seemed severe. He was rather strange in his manner, and, as I have observed in similar cases, could not concentrate his attention. It was accompanied with fainting fits, and he could not obtain any sound rest at night, dreaming and tossing about. His temperature would sometimes run up to 102° in the evening, and fall to the normal point in the morning. Recovery followed complete rest for six weeks and good nursing.

In these cases we must not wait till the patient complains of headache. It is important to recognize the first symptoms of cerebral irritation. There may be nothing definite, but we know the boy or girl is not well by some alteration in his or her disposition; the absence of self-control, petulance, anger, restlessness at night, and weariness over lessons should be enough to direct our attention to a group of symptoms which, if unheeded, will end in a break-down.

In girls, more especially from thirteen to fifteen years of age, a tendency to hysterical disturbance sometimes accompanies this state of brain, often invited by close and continuous study when important physiological changes are taking place in their constitu-

* "On Irritable Brain in Children," Med. Soc., London, 1886.

Digitized by Google

tion. They are first exhausted, then they become irritable, and finally hysterical. Being by nature softer, weaker, and more sensitive than boys, the forced efforts to acquire knowledge in many subjects cannot be borne with impunity. Even great energy of character and determination of will are sure to lead to failure when the health is delicate. They require rest and relaxation at this period of their lives to enable them to throw off the languor and depression induced by laborious study and close application.

The vision is constantly affected as a result of over-strain, and myopia is common among children in elementary schools. Dr. Widmark examined the condition of the eyes of young persons attending some of the schools in Stockholm. He examined 704 males and 742 females. He found that from six to eight years of age there was no myopia; at nine, though the girls were still nearly free from it, 14 per cent. of the boys were distinctly myopic. This difference arises from the boys having to work harder in the preparatory school before admission to a secondary school. After this age myopia goes on increasing in both sexes until, at sixteen, when girls usually leave school, the percentage is 33. The percentage among the boys is 39. Among the advanced pupils myopia was more common and more marked among the girls. Dr. Widmark attributes the latter circumstance to delicacy of sex, but mainly to inferior physical education, and needlework and music in place of outdoor games.* Excess of work for the eye seems to be more detrimental to girls than to boys.

A Government Commission recently made an inquiry concerning the eyesight of the children in the classical department of the largest schools in Denmark. They found that the percentage of short-sightedness increased with the number of the class; thus, among boys in the first class it was 14.7 per cent., increasing gradually upward till it reached 45.4 in the sixth class.† Sir Crichton Browne proved, in 1885, that as many as 25 per cent. of the children in our elementary schools were short-sighted, and Mr. Brudenell Carter's experience is in the same direction. Myopia, hypermetropia and astigmatism are the chief disorders.

If this large percentage of boys suffer from myopia, there must be a considerable number of children who are hypermetropic, which is more likely to produce headache. One very startling case of this kind came under my notice some years ago. A delicate boy, twelve years of age, during a long and painful illness, which proved fatal four years afterward, was in the habit of reading a great deal in the recumbent posture. He became hypermetropic, and, according to an oculist, had the sight of a man at seventy. He also suffered from terrible paroxysms of headache, of the one-sided or genuine neuralgic type. Every ophthalmic surgeon is acquainted with a variety of headache (ocular headache) which arises from excessive strain on the eyes for the accomplishment of minute work of any kind or reading small print.

From the foregoing observations it will follow, as a matter of course, that these headaches cannot be successfully overcome till the cause is removed. Once invited, they take a hold which may last more or less for life. Look at the question as we may, the system of education and the habits of life have changed like past manners, customs fashions and pleasures. The nervous system is in a state of greater tension and trembles more in the balance than formerly. First with respect to *education* and *over-pressure* where it exists. It is evident that no system can be adopted unmixed with evil of some kind. The most carefully-devised plan for instruction has its drawbacks and its shortcomings. Though existing regulations may be sound, there is room for improvement. It is proved beyond the possibility of doubt that strong and well-fed children, who live in comfortable homes, can accomplish the standard subjects without any risk of a break-down, while those children reared in squalid homes and badly fed are likely to

* *The Lancet*, February 28th, 1887, p, 456.

† *Op. cit.*, p. 147.

Digitized by Google

fail in the elementary subjects taught in our schools, and headache is about the first symptom they complain of. These children would succumb under almost any system of education. Their constitution will not bear the training prescribed for the generality. Pressed beyond their strength, their growth is stunted, they lose their appetite, become irritable, and lack interest in everything.

Life passed in large towns and cities, with the noise, the bustle, and the impure atmosphere, is terribly exhausting to the poor school child, who has not the same advantages as the country child who breathes purer air and lives on more wholesome food. Constitutional weakness is not so quickly invited. The country child pursues his work in a steady, quiet, plodding way, and is not so easily driven, hurried or terrified by over-zealous teachers. Headaches among these children are rarely met with as a consequence of education. But the case is quite different with delicate children who are educated in our large towns and cities. Headache is the first symptom to attract our attention in these cases. It has a very injurious effect upon the health and spirits of the child. He loses color and becomes anæmic; pathological changes lead to a disturbance in the nutrition of the brain, which, being badly nourished, is quickly fatigued. Headache and irritability of the brain are followed, sooner or later, by a passive dilatation of the cerebral blood vessels. Now, it is in children who have suffered from an irritable brain that grave danger is to be apprehended if undue pressure is put upon them as they grow older. Sir Crichton Browne, in this country, Hertel (Denmark), Treichler (Bad Leuk Bern), Kjelberg (Sweden), are unanimous in opinion that the school system of their respective countries is injurious to the growing powers of such boys and girls, by enfeebling the mind, and thus destroying the force of character in after-life.

Professor Kjelberg, of Upsala, is of opinion that the true powers of mind and force of character are wanting among the young men of the rising generation. Dr. Hertel's observations show that headache and nose-bleeding are common among boys in the Danish schools, the school work tending to produce *congestion of the brain;* they do not obtain sufficient sleep, some even sitting up late, till midnight, and working "over eleven hours a day,"* so that they have neither the time nor the strength and inclination for outdoor exercises to maintain the general health. To overwork a young brain is to enfeeble it.

All strain falls heavily upon delicate constitutions. If the child who has suffered from an irritable brain cannot obtain sufficient sleep, and has become over-anxious in his work, *congestion* of the brain, leading to meningitis, is an occasional result. I have, within the last four years, seen four children who died from meningitis, traceable, I think, in the most conclusive manner, to school work and night lessons at home.

No course of instruction in a school can be framed to meet the capacity of every child, but I think a medical board should be established, which would help us in this direction, and decide whether the child is fitted to go through the code as laid down by the educational department. Sir Crichton Browne, upward of three years ago, advocated the weighing, measuring, and medical examination of every child in every elementary school, and the recommendation was an excellent one. If we only take a casual glance at some children, we see at once that it would be preposterous to view all of them, at a given age, as equal in mental capacity and physical strength. But this is what the school board does. It lays down, almost of necessity, hard-and-fast lines. It presupposes that the same amount of intellectual work is obtainable from a child that is delicate and weak as from one that is vigorous and strong. As Sir Spencer Wells puts it: "The vice of the system is that it is indiscriminate."† It seems to

* "Over-pressure in High Schools in Denmark," by S. Hertel, 1885.

† "Inaugural Address, delivered before the Congress, at York, 1886."

Digitized by Google

me, therefore, that Sir Crichton Browne's suggestion for a regular medical examination should be adopted, in order that it may be ascertained whether a particular child is capable of going through a part or the whole curriculum of school work. If this was adopted, sickly children, and those with a history of irritable brain, would be weeded out, or, at any rate, the same course of study and application would not be expected from them.

A child of ordinary health and intelligence, who goes to school regularly and gives moderate attention to his studies, is scarcely likely to suffer from headaches in consequence, unless he is predisposed to them; but there are many children who attend our board schools whose physical and mental powers fail under the attempt; they become excited with the forced effort they employ to keep pace with their rivals, and so mischief, which might have slumbered or passed away, is awakened, and failure results. Home lessons, in addition to regular attendance at school, is surely unsuitable for delicate and very young children. I could quote several instances in which grave mischief has followed the over-exertion of a child's mind.

If intellectual training in the young be carried too far, and at the expense of physical health, deterioration must result. Competitive examinations are now carried to a point far beyond the capabilities of many boys and girls, and it has become of the greatest importance not to lend too much encouragement in this direction. Yet year by year the forcing system goes on in every country in Europe, and the supervision which ought to be exerted is not provided.

We must understand the laws of nature if we are to be and to keep in harmony with them. As Mr. Herbert Spencer pointed out long ago, and as all experienced and thoughtful teachers believe, those who have to do with the rearing and training of children should be thoroughly acquainted with their physiological and psychological natures, with what promotes and what injuriously affects the development of these. In our actual studies and methods of education this is too often neglected, and artificial conditions are created which act injuriously on the organism. The child gradually drifts into ill health, nutrition is impaired, and growth, instead of being uniform and progressive, is checked, and the brain loses its energy.

In the *treatment* of *recurring headaches* in school children many points require attention. In the first place, education must be abandoned for a time and the child's thoughts be directed into another channel, so that the brain may lie fallow or be otherwise and more gently exercised. The mischief is brought on by over-strain, which first induces irritation of the brain, followed by congestion and disturbance of the cerebral circulation. I frequently see children whose parents, in ignorance of the circumstances which have led to this trouble, will not consent to their studies being temporarily relinquished, and the consequence is they gradually drift into bad health; the bowels become costive, the appetite is capricious or destroyed, and headache and confusion of the brain become chronic. The pain, in these cases, is almost invariably frontal—a dull, heavy, continuous pain across the centre of the forehead or immediately over the eyebrows. It is an oppressive pain. This character of headache is invariably aggravated by iron and quinine. Bromides should be given three times a day in full doses to lessen excitability and quiet the brain; if there be nausea, let them be administered in effervescence with carbonate of soda and ammonia; keep the bowels gently open. At the onset, it is sometimes advisable to give a full saline purgative, so as to drain the portal circulation and indirectly relieve the head. Above all, bathe the head with a large sponge well soaked in tepid or cold water. Let it be done frequently during the day. I have known this to give more relief than any other remedy that could be suggested. It lessens the congestion of the over-filled vessels, promotes their contraction, equalizes the cerebral circulation, and exerts a sedative action on the nervous centres.

Digitized by Google

TREATMENT OF IRRITABLE BRAIN.

As soon as the patient shows signs of cerebral irritation, all school work should be abandoned. Slight headache, disturbed sleep and easily-induced fatigue are warnings. If promptly attended to, they will, in most cases, vanish; but if they are unheeded, and the child continues to exert his brain, the circulation will become more disturbed, the headache will increase in severity and the temperature run up three or four degrees, from mental excitement and exhaustion. Rest of mind and body is to be adopted, as in all forms of headache traceable to school work. Bromide of potassium and hydrate of chloral will be required if there be restlessness and any febrile disturbance. These remedies will tend to lessen congestion and arterial tension, should these be present, and will calm the excited brain. Cold applications to the head will often invite sleep and tranquillize the patient when chloral and bromide fail. When the pain subsides, and there is no vomiting, milk, beef tea, and other simple kinds of nourishment should be given. It must not be forgotten that this irritation, if not carefully watched, may pass into severe congestion and meningitis.

TREATMENT OF NEURALGIC HEADACHE.

Here, also, rest and relaxation are of primary importance. If the pain is traceable to over-exertion of the mind, lessons must be discontinued and the general health attended to. When no cause can be ascertained, it is often hereditary. If it should prove to be of malarial origin, then quinine and arsenic are the remedies to employ; and if there be anæmia—a very frequent cause—then iron is the most reliable drug. It is an excellent plan in these cases to give the child one or two teaspoonfuls of cod-liver oil after breakfast, before he goes to school, this being one of the best means we possess for strengthening the nervous system. It increases the red corpuscles in the blood and improves digestion. A small quantity of alcohol, in some shape or form, with the two chief meals of the day, may be necessary. If there are decayed teeth they must be attended to. Severe supra-orbital neuralgia has yielded to the lancing of the second molar tooth.*

NECESSITY FOR SLEEP IN SCHOOL CHILDREN.

The importance of sleep cannot be over-estimated in children whose brains are much taxed by school work. Complete suspension of mental activity and rest of the muscular system are needed for at least nine hours out of the twenty-four, children of sensitive temperament requiring a large allowance of sleep by reason of a more rapid expenditure of nervous energy. If home lessons are required of such children after a day's work at school, they are sure to prove injurious. Children of bilious and phlegmatic temperament may not need much sleep, because they are habitually torpid, and the "wear and tear" is not so great with them. But there are children in whom the disposition to somnolency exists, and their health would seriously suffer if they did not obtain it. Whenever sleep is profound and children cannot be easily aroused, it is an evidence that they have not had enough. Sleep, to be healthy and restorative, should be quiet and dreamless, the functions of the mind being completely suspended.

The study of health and disease ever offers something new. Our bodily organs are exposed to constant change; they are prostrated by pain, stimulated by pressure, subdued by fear; they sympathize with our mental state, and in order to realize the connection we should carefully consider every little fact which has an import and a meaning; not that we may speculate on what *may* happen in the future, but that we may know what *is* happening in the present. As Milton tells us—

> Not to know at large of things remote
> From use, obscure and subtle, but to know
> That which before us lies in daily life,
> Is the prime wisdom. . . . . —*Paradise Lost.*

---

* *Brit. Med. Jour.*, May 5th, 1882.

Digitized by Google

DISCUSSION.

Dr. Wm. S. Dennett, of New York, said:—*Mr. President, Gentlemen and Ladies*—I thank you for the privilege of taking part in the discussion of this interesting and suggestive paper, the reading of which afforded me great pleasure, inasmuch as it dealt not only with the detail of the subject from a purely medical point of view, but because the author has been led to consider one of the many relations that exist between medical and educational science.

Here we are brought face to face with the fact that there is no well-recognized medical specialty so narrow in its scope that it does not afford room for the broadest investigation. The organ to which the specialist gives his attention may not be more than an inch in diameter, but if he is himself worthy to claim a place in the ranks of the liberal profession, he is likely to find himself at any time shoulder to shoulder with other men from different fields, each doing his part in some great work that raises him from the position of a business man to that of a philanthropist.

There is a great deal of routine and drudgery in medical art, and the art is what we all must needs practice. But there is nothing narrowing or uninteresting in medicine as a science, and most of us are glad occasionally to leave the art behind us and interest ourselves to some purpose in broad questions of a political or a social nature.

In this particular question of the influence which the medical profession ought to have in the direction of the affairs of the public schools, and the methods in accordance with which educational institutions should be conducted, we are all interested.

My brothers in ophthalmology will bear me out in the assertion, that a very important part of the work that has been done by them during the fifty years since physiological optics has become a science, has had to do with questions that concern the medical profession as guardians of the race, and as being, in some measure, responsible for the welfare of the coming generation; and that one of the most widely-recognized fields of useful action has been the investigation of the influence of school life on the visual apparatus of the individual and the race. It is with no little satisfaction that we are reminded, in listening to Dr. Day's concise and emphatic paper, that representative men from almost every branch of medical science are interesting themselves in the same, and there is, perhaps, no field among the problems of the day where concerted action is more needed or where it promises more gratifying results, than in the subject before us.

The condition of the schools of the civilized world tells us that our influence has been too little felt in the past. But the action of the colleges that are now beginning to add to their professorships those whose sole duties are to look out for the physical well-being and physical education of their students, tells us that we shall have abundant opportunity to make for ourselves a better record in the future.

The history of Greece and Rome, and the faces and figures that their artists have left, give undoubted proof that there was a time when physical well-being was the all-important factor; while, to-day, we seem to have forgotten that there is such a thing as a body to be cared for. We must neglect no opportunity to interest ourselves in such matters until ventilation, exercise and recreation are as much under the supervision of competent authority as grammar, arithmetic and geography.

Dr. Day, in his careful enumeration of the causes of headache, mentioned those caused by refractive changes in the eye, and he has given some interesting statistics in regard to the frequency of such changes. No one familiar with the literature of the subject will think for a moment that the matter has been over-stated. We all know that myopia, hypermetropia and astigmatism are frequent causes of headache,

Digitized by Google

and that permanent cure is often effected when this cause has been discovered and corrected.

But headache is much more frequently associated with these troubles in adults than in children. Indeed, one of the unfortunate things about myopia in school children is, that it commences and increases in such a way that the victim has had no warning until after the damage is irreparable; and it is this fact that has led the oculist so often to ask that periodic examinations of students be required.

We all recognize the type of headache to which Dr. Day has referred as associated with refractive troubles, as well as the occipito-cervical feelings of discomfort which are associated with muscular insufficiency. The medical profession might congratulate themselves if every case of incipient myopia were attended with pain, for then the child would have a tangible warning of the occurrence of this serious ailment. But the fact is, that though headache is an occasional symptom of myopia, it should be borne in mind by every one who is interested in the nervous diseases of children, the large majority of such cases have no emphatic warning of this kind, and often no warning of any kind. The principal symptom is a failure of vision for distant objects. And, as the speaker has mentioned recently, in a short article in the *New York Medical Record* of July 9th, all our children are allowed to grow up without even an approximate idea of what the healthy eye should see.

Such oversight is very unfortunate, and not only the ophthalmologists, but the neurologists and all others interested in school hygiene, should hasten to remedy this oversight, among the many others which exist in the matters of school management.

And this is not said in the spirit of one who complains of the past or of the present, but of one who hopes for the future; not of one who wishes to ignore the fact that the schools, as they exist to-day, are such that we point to them with pride, but of one who wishes to press forward earnestly in the good work that has already been done, so that no one can say that we, as heirs of all the ages, failed to appreciate the trust that we have received from our fathers, and to do for our generation as much as they have done for us. Progress in one age necessitates reform in the next, and there is no doubt that the very efficiency of the educational methods that we have received has made it incumbent on us to protect our children from the evil effect of over-exertion and stimulation, and many of us will be called upon to help to decide how it shall be done. We must not be too revolutionary in our methods. There are those who think that because intemperance has at some time done injury in our midst, and is still doing injury, the world should be governed by rules that would make of it a large asylum for inebriates. There have been dyspeptics who, in the fancied interests of reform, have tried to make their fellow-men do homage to their own weak stomachs by eating only Graham bread and abstaining from a more varied and stimulating diet.

And some of us have heard lately that grammar should be abolished from the public schools because its intricacies have proved too much for some of the delicate intellects that have been brought to bear upon it. The writer of the paper to which I have the honor to respond has justly said that no hard and fast lines can be drawn to fit all classes of scholars, and he has described two classes of nervous and mental organizations which cannot be subjected to the same treatment, either in or out of the public schools. But let us who have, or who hope to have, influence in matters of educational reform, not make the mistake of sacrificing the strong to the weak in every particular. Let us teach more rather than teach less, but let us, as far as is practicable, arrange that the fast may run while the slow may walk, and that neither shall be sacrificed overmuch to the other. And let us all hope ere long to see the day

Digitized by Google

when the physician shall share with the metaphysician the responsibility of educating our young; when he shall have his place in the corps of public instructors, and when physical exercise as well as mental shall be a part of the daily curriculum in all institutions of learning. Let me ask, in closing, that those of my hearers who shall be in future called upon to assist in framing the laws which shall govern our school boards, shall bear in mind the necessity of tending as much as possible to some arrangement which will allow the bright and strong to take more while the feeble take less; while each is able to receive his proportionate share of improvement. We must remember, too, that the science of education is as much a science as the science of medicine, and that while there are certain matters on which we as physicians can speak with authority, it becomes us to think carefully before insisting on any wholesale changes which do not seem good to our co-laborers in the pedagogic field, who are to-day as active and progressive in their departments as we are in ours, and who are cognizant of the fact that to certain types of mental organization ambition is as injurious as laziness, and that only in the most exceptional cases is child or adult to be allowed to do his utmost. Whether he is training for a boat race or studying for a prize, he can ill afford to work himself to the limit of his endurance. Such effort is always injurious; the risk is sometimes necessary in the active business man; never to the scholar.

Dr. J. G. Wiltshire, Bath, Me., remarked—I have listened to the paper with pleasure and profit. I arise to offer my thanks for his admirable presentation of the subject and to respectfully submit a few remarks which may serve to bring out more prominently a truth alluded to by Dr. Day. He states that the observations of ophthalmologists, in their investigations of the vision of students in a given country, have revealed the fact that myopia was found to exist in so many instances as to convey the idea that hypermetropia preëxists much less frequently in children than myopia; when, in fact, the converse obtains—the myopia occurring secondarily as a consequence of efforts of accommodation, and congestion of the eyeball when the students bow their heads toward the desks in their studies. The myopia steadily increases in percentage and extent as the amount of close work increases. Dr. Risley, of Philadelphia, found that myopia, commencing in the primary classes with a *low* percentage, steadily increases as the pupils pass to the highest grade in our public schools (see Sœlberg Wells, p. 631). Besides, Dr. Day admits that in the cases cited by him the myopia increases in degree and percentage as the student advances in his student life. Doubtless, in many of his cases there was no preëxisting myopia, the eye being emmetropic or hypermetropic, but afterward becoming myopic in a way that suggests the idea that this error of refraction is produced by the efforts of the muscles of accommodation and convergence while in the act of study.

How is this change in the shape of the eyeball brought about? We will first suppose that we have emmetropic or hypermetropic eyes to deal with in children who are occupying low desks or high seats. In order that they may indulge a natural indolence they lean over their books, thus bringing the object of vision so near them that the rays of light no longer enter the eye in a slightly divergent direction, but in a highly divergent direction, so as to cause the eye to summon an extra effort of accommodation in order to focus them upon the yellow spot or centre of vision, and thus avoid producing circles of dispersion on the retina, as they would otherwise do. The combined efforts of accommodation and convergence will insure acute vision for awhile, but shortly headaches and other evidences of asthenopia announce themselves; at this juncture the extrinsic muscles of the eye will begin to compress the ball, and the stooping attitude of the child favoring congestion, all

Digitized by Google

conspire to weaken and lengthen the tunics of the ball in an antero-posterior diameter, thus producing myopia.

I can well see how astigmatism, hypermetropia and myopia can produce headaches in those who violate the requirements of normal accommodation and convergence. To correct head symptoms due to errors of refraction and accommodation, we must remove the cause by selecting for our patients proper glasses. For hypermetropia give a convex spherical glass of proper strength; for myopia select a concave spherical glass of the proper strength; for astigmatism order a proper cylindrical glass with its axis in the correct meridian.

Dr. DENNETT, New York, replied that the statistics relative to myopia and other refractive troubles had been so long and so fully collected and tabulated that there was little or no uncertainty in the meaning of the deductions which could be made from them. It was easy enough for one to put his hand on the records of something like a quarter of a million examinations.

Dr. Dennett believed that hypermetropia did precede myopia, and that no one thing could to-day be spoken of as the principal cause of that trouble, but that there were some reasons for thinking it to be one of the penalties that we pay for a civilized and thoughtful ancestry.

He remembered to have seen some statistics collected among the Italian peasantry, where reading and writing were uncommon, but where there was undoubted descent from an old civilization, and where the percentage of refractive troubles was nearly as great as existed among the student classes in Germany. He referred to an article written about ten years ago by Dr. Hunt, of Boston, relative to the connection of this trouble with embryonic changes, in which the development of brain encroached on the growth of the tissues which go to make up the framework of the eye (sclera) and other parts of the body.

REMARKS BY THE PRESIDENT.—The interesting and instructive paper to which we have listened is from the pen of one who has given much attention to the headaches of childhood. His long experience and many observations relating to this subject give weight and prominence to the views which he expresses. This paper, from one who is justly regarded as an authority, will probably lead to a better understanding of the common ailment of which it treats, so that remedies can be more satisfactorily and effectually applied. Another equally prominent London physician, who holds an official position in this Section, Dr. Charles West, has also made a careful study of headaches in children. In his Lumleian Lectures he says, "In infancy and childhood . . pain referred to any part signifies, almost without exception, that disease of some sort or other is going on there, or near at hand." Dr. West continues, "There are two classes of cases in which it is of especial importance to bear in mind this caution: The one, those cases in which pain is referred to the head; the other, those in which it is situate in one of the lower limbs. In the former case the pain is almost invariably symptomatic of organic disease of the brain; in the latter, with almost equal certainty, of hip-joint disease." . . . He adds, "I shall not think I have said too much if I do but impress on the minds of any of my younger brethren . . . the excessive rarity of pain in childhood, except as a sign of local disease." As regards the headaches of children in New York City, where my observations have been made, I think that these remarks of this distinguished and able contributor to pediatric literature should be modified. I accept the statement as entirely true, that persistent headache in a child, attended by fever and not influenced by treatment, especially by quinine, is a symptom of local cerebral disease,

Digitized by Google

usually meningitis. We have probably all had unpleasant experience with cases like the following: A child of perhaps six or eight years, of parentage not decidedly unhealthy, and with no marked tendency to tuberculosis, without history of injury of the head, or of otorrhœa, loses appetite, becomes fretful, is quiet, but is able to walk about, has a temperature of 101° or 102°, complains of a dull frontal headache, without vomiting and without any appreciable change in the appearance and action of the eyes. We hesitate in diagnosis between a fever arising from malarial influence and meningitis. We prescribe, tentatively, alternate doses of one of the bromides and quinine, but the dull headache persists. The treatment does not exert any appreciable controlling influence upon the disease, and in a few days—four or five—inequality of the pupils and other symptoms indicate unmistakably a meningitis. The *tache cerebrale* of Trousseau gives but little diagnostic aid in such cases, for it can often be produced in febrile states not due to disease of the nervous centre. Cases like the above, occurring in my own practice and in the practice of others, a considerable number of which I have had the opportunity to examine, have long since convinced me that headache in a child, if attended by fever, and without any appreciable local cause elsewhere than within the cranium, if it persist beyond the fourth or fifth day, uninfluenced by the bromides and quinine, almost without exception, indicates intra-cranial disease; and it is important that an early diagnosis be made, that the friends may have sufficient forewarning, and the proper treatment be early employed.

But headache in children, persisting for weeks, or even months, without fever, is a common ailment in New York, especially in school children and in girls; I have observed it most frequently in children between the ages of six and twelve years. The child with this form of headache loses its customary vivacity, but still pursues its studies in the school; it sleeps as usual at night, or is rather restless; its appetite may be moderately good or impaired, and there is no marked deviation from their normal state in other functions. The headache not infrequently increases during the school hours, and many children return from school with complaint of an increase in the cephalalgia, which soon, by the rest at home, diminishes, but does not cease.

Most children with this form of headache are tolerably well nourished, and apparently lose but little flesh and strength, except as impairment of the strength may be indicated by languor. One having had a ruddy complexion loses it, and the countenance has more pallor than in health.

This form of headache is in a certain proportion of cases accompanied by neuralgic pain in other parts, especially in the left side of the chest, or in the sternal or epigastric region. Wherever the pain is located, whether it be, as is usual, a frontal cephalalgia, or be seated in the trunk, pressure over or near the roots of the nerves which supply the affected part increases the pain. Thus, pressure upon the nucha, close to the occiput, or upon the lower part of the occiput, increases the frontal headache. If thoracic pain is present pressure upon the spine at a certain point increases it. I have observed cases in which pressure a little below the occiput, upon the cervical vertebræ, caused pain in the region of the eyes and nose, and lower still, at some point in the thorax. The site of the pain varying according as the pressure was made higher or lower along the spine.

This form of cephalalgia is apparently a marked exception to the rule stated by Dr. West, that persistent headache in children has a local cause. Perhaps in some of these cases there may be a local cause in the condition of the eye, errors of refraction, by which the pain is increased, but I think that those who have studied this form of headache will admit that its chief and common cause is a depressed or perhaps over-

Digitized by Google

sensitive state of the nervous system, and an anæmic condition of the blood. It is common among the children of the tenement-house population in the cities, and is, I presume, rare in the rural districts, where a better state of the general health prevails.

The prognosis is favorable, but the headache is usually protracted, continuing till a better state of the general health is obtained. However, I have known patients to recover quickly when taken from school and sent to the country, in the summer months. If removed from the severe discipline of the schools, allowed a more generous diet, with abundant recreation in the open air, many recover without the aid of medicine, but vegetable and ferruginous tonics assist in promoting recovery. The elixir of calisaya bark with iron, or some similar tonic, should be prescribed. A child brought from a tenement house to the children's class at Bellevue rapidly recovered under the use of the following prescription :—

℞. Potass et ferri tart, ʒj
Tinct. cinchon. comp., ℥iv.
Dose, one teaspoonful, in water, 3 or 4 times daily.

The frequency of this form of headache in city practice, and the fact that systematic writers do not seem to me to give it the attention which it deserves, induce me to make these remarks.

THE PATHOLOGICAL ANATOMY OF LARYNGEAL DIPHTHERIA AS RELATED TO INTUBATION.

L'ANATOMIE PATHOLOGIQUE DE LA DIPHTÉRIE LARYNGÉE PAR RAPPORT À L'INTUBATION.

DIE PATHOLOGISCHE ANATOMIE DER KEHLKOPFDIPHTHERIE MIT HINSICHT AUF DIE INTUBATION.

BY WILLIAM PERRY NORTHRUP, M. D.,

Pathologist, New York Foundling Asylum.

In preparation for the discussion of the methods of relieving the urgent symptoms of laryngeal diphtheria, it is not amiss to first of all contemplate the nature of the lesion.

In this paper I shall undertake no review of the history of the disease, shall not try to disprove the dual theory, shall propose no new theory. What I shall do, is, lay before you some of the facts observed in autopsies on children dying of diphtheria in whom there has been found diphtheria of the larynx. Let it be remembered that I do not say whether all or any of them have been croupy or have been intubated. I mean simply to see what facts force themselves upon our minds in studying these records, and there is but one constant factor, viz., the cases all had pus and fibrin exudate in the larynx. Cases of diphtheria in which the pseudo-membrane was not found in the larynx after death have been excluded.

The name diphtheria has been used because in each case, sooner or later, ash-gray pseudo-membrane has been observed in the pharynx, and there has been parenchymatous nephritis associated with it, and also a tendency to heart failure.

The autopsy records of the New York Foundling Asylum show 116 cases, in which there was found after death pseudo-membrane, pus and fibrin exudate upon the mucous

Digitized by Google

membrane of the larynx. Of this number 68 began with symptoms showing that the larynx was invaded before or at the same time that pseudo-membrane was seen in the pharynx. In other words, there was first of all croup.

Of the 116 cases 38 showed pseudo-membrane in the pharynx, and the child subsequently became croupy. In the remaining cases the order of precedence was indeterminate.

Ninety cases of so-called "membranous croup" have been analyzed. Of the 90 cases under consideration nine showed pseudo-membrane from the tip of the nose to the finest bronchi; six from the nose to the bifurcation of the trachea; 17 from the pharynx to the main bronchi. In 17 cases pseudo-membrane was found in the larynx and trachea; in three, in the pharynx and larynx; in one, in the larynx only.

With but one exception, the pseudo-membrane was continuous to the extent of its distribution. In this exceptional case there was well-marked pseudo-membrane from the pharynx to the middle of the trachea. From this point to the bronchi of the fourth division there was no exudate to be seen, and yet beyond this point there were well-formed casts of the finest bronchi.

Such was the distribution of the membrane in 90 cases.

The cases are collected from no epidemic and are distributed over the records of five years. I purposely separate them from 26 cases to be later considered by themselves.

Of the 90 cases 50 were females. The average age was two years, seven months. The number of children dying under one year was 13; between one and two years, 12; between two and three years, 10; between three and four years, 36; between four and five years, 11; between five and six years, one; between six and seven years, one. Others were indeterminate. The greatest mortality, then, was between three and four years, where we find 36 of the 90 cases. Above and below this age the number falls abruptly to ten and eleven. In 54 cases the average number of days from the beginning of croupy symptoms till death was three and four-fifths days. In two cases croup was the first symptom, and the children were dead in 20 hours.

Emphysema.—The interstitial variety has been observed in eight cases; the vesicular in nine.

Pneumonia.—This complication attained to the number of 56 in 90 cases. Twenty-nine had pneumonia enough to be sufficient cause of death. In twenty-seven cases bronchial diphtheria was the prominent cause of death. Eighty-four cases showed extensive invasion of the respiratory tract, 22 followed measles, eight followed scarlatina.

In the spring of 1887 there occurred an epidemic of measles, which rapidly added 26 cases to the records. Since it presented peculiarities these cases will be discussed by themselves. Of the 26 cases all had measles and pneumonia. Eight had scarlet fever with measles and pneumonia. Of the 26 cases 15 had, besides measles and pneumonia and diphtheria, well-developed nephritis. The complications show the severity of the epidemic.

In previous autopsies on children dying after having worn the O'Dwyer tube from three to seven days, there was found merely rubbing away of the superficial epithelium. The loss of substance did not extend below the epithelium layer.

When we come to observe these twenty-six cases occurring in rapid succession during an epidemic of measles, in which several children had, beside measles, pneumonia, scarlet fever, diphtheria and nephritis, we may not be surprised that the tube was harmful to the tissues.

Each time the child swallowed, the lower end of the tube was thrown against the anterior wall of the trachea. In case the tissues are well nourished they may suffer but little.

When, as in the above cases, the system is overwhelmed with complications and the

Digitized by Google

vitality of the tissues is lowered, it is not surprising that this foreign body causes injury to the parts. So it does. The mucous membrane is rubbed away, the submucous tissues are rubbed away, the cartilages are laid bare, and the ulcer thus formed grows wider. This occurred in some cases where the tube had been worn no more than three or four days.

It is well to emphasize this point. Up to this epidemic, ulcers of the trachea extending deeper than the mucous membrane had not been observed. During this fatal epidemic of measles, which rapidly added 26 cases to the records, five severe ulcers were found where the lower end of the laryngeal tube had rubbed against the anterior tracheal wall. I have never seen any ulcers of the larynx caused by the head of the tube. Slight superficial destruction of epithelium has been found, but nothing to amount to a complication.

We come now to speak of pneumonia, which occurred in 56 of the 90 sporadic cases and in 26 of the 26 cases of the epidemic. In nearly all these cases pneumonic consolidation has been sufficient in extent to leave no doubt as to the nature of the complication.

When there has been doubt, the post-mortem diagnosis has been made on the microscopic findings. Infiltration of the capillary bronchi accompanied with beginning parenchymatous changes is here denominated broncho-pneumonia. In studying so important a complication of laryngeal diphtheria as this, it is important that all should agree upon the names employed. It has been quite common to call the condition of many such lungs collapse. This test has been recommended to differentiate collapse from hepatization. Pass a pipe into the bronchi and blow gently through it. If the lung is in collapse it will easily and fully inflate under gentle pressure. If it does not inflate, it is pneumonic. This test is crude in the extreme. Collapse exists mostly associated with the pneumonic process.

I have never failed to find in atelectic areas abundant evidence of inflammatory process, both in the capillary bronchi and air passages and in the alveoli. In a lung removed from the thorax, it is easy to force air through the obstructing plug, and fill out the remaining capacity of the partially collapsed alveoli. In such lungs it is often possible to restore the rose color by inflation, but it is deeper red than normal, and scattered through it are impermeable lobules of hepatization.

82 of the 116 cases had pneumonia, either evinced by well-marked and unmistakable signs of consolidation or revealed by the microscope.

This does not seem so extraordinary, either, when we reflect in how many cases pseudo-membrane advanced along the air tubes to the capillary bronchi.

The prevalence of pneumonia as a complication certainly need not be urged as proof that it is *schluck pneumonie.* A diphtheritic inflammation is creeping down the bronchi. By preference it selects dependent portions of the lung. That may argue that the products of inflammation drain into these portions and excite inflammation of similar nature there. Whatever the method, the inflammation advances to the finest bronchi by continuity of tissue. Theoretically, it may cease to advance and remain "capillary bronchitis," but, practically, this is not met with.

The microscope reveals beginning parenchymatous changes and commonly scattered hepatized lobules.

SCHLUCK PNEUMONIE.

Ten years ago much was said about foreign body pneumonia. Particles of food, secretions of the mouth, might enter the lungs and cause pneumonia. Since that time this term has been used mostly in connection with surgical injuries, where hemorrhage into the trachea has taken place, or where foreign bodies of whatever kind have passed beyond the large bronchi and have become lodged.

When the O'Dwyer tube came into use, and it was said the patient had difficulty

Digitized by Google

in swallowing milk because it occasioned cough, then immediately arose the shout that schluck pneumonie was the cause of death. *A priori* it would seem reasonable.

Again, examining a cadaver lying on its back, it will be seen that from the main branches of each lung two large branches put off, one to the lower posterior portion of the upper lobe and one to the lower lobe running nearly parallel with its posterior margin. From this large and long bronchus numerous smaller bronchi put off to the posterior margin and dependent portion. From their relations to the trachea and main bronchi, it seems these two bronchi are well adapted to catch the drain of the trachea and main bronchi, and through the smaller dependent branches to conduct to the posterior margin of the lower lobes and lower posterior portion of the upper lobes.

This *a priori* reasoning is fortified by the pathological selection. It is in the lower lobes, posterior portion, that we find the first pneumonia, and very commonly there is with it consolidation of the lower portion of the upper lobe posteriorly.

It would seem, then, that we might *establish the fact*, if there were pneumonia from the passage of milk into the dependent branches. Especial attention has been devoted to this point, and thus far no evidences have been found of the presence of foreign material.

Furthermore, the experiment has been tried of inducing schluck pneumonie by feeding the child on milk and other fluids, having finely divided carbon in suspension. If this insoluble powder, having a strong contrasting color, were taken into the lungs in elective regions, it would be possible to find it later.

In these experimental cases the powdered carbon (bone black) was given while the child was able to swallow fairly well, so as to make the test satisfactory, and for the same reason it was discontinued when the child became enfeebled and was about to die.

I may say that I have never found any evidence that milk or bone black had passed into the lungs of a child wearing a laryngeal tube. The smallest particle of milk bathing the under surface of a swollen and insufficient epiglottis would excite violent cough. It does so in laryngeal tuberculosis in the adult.

Tread as lightly as we may, it is obvious that we are striving to avoid the terms "Membranous Croup" and "Croupous Laryngitis." To me it is a conviction that these records from which I summarize show that the kind of croup met with at the New York Foundling Asylum, requiring mechanical interference, is none other than diphtheria invading the respiratory tract. The two most familiar classes of cases are these: First. A child having measles coughs croupy. The next day patches are seen on the pharynx. One day later the dyspnœa has advanced so far as to require intubation.

On autopsy—(I append an autopsy, because nearly every measles case, complicated with laryngeal diphtheria, develops pneumonia and dies). On autopsy, the larynx, trachea, and bronchi contain tenacious, continuous pus and fibrous exudate and the lungs show pneumonic consolidation. Examination of the kidneys reveals parenchymatous nephritis. Urine drawn from the bladder contains hyaline and epithelial casts and kidney epithelium.

The second case is a child, running about the ward in the evening, showing no symptoms till the attention of the nurse is attracted by a hoarse cough. During the night dyspnœa develops rapidly, and in early morning a tube is required and inserted. The urgent symptoms are relieved, but in thirty-six hours the child succumbs to the rapid invasion and overwhelming poison. There is no physical sign of pneumonic consolidation. On autopsy, we find membranous croup indeed.

What are the findings in this rapidly fatal case? The pharynx shows nothing or only a few grayish points on the tonsils. The epiglottis is swollen, and poorly adapts itself to its work of guarding the entrance to the respiratory tract. The concavity of the epiglottis is covered with pus and fibrin exudate, continuous and tenacious; so is the inside of the larynx; the same is true of the trachea, the same of the bronchial

Digitized by Google

tubes, and even from the finest bronchi of portions of the lungs can be drawn out casts of the capillary tubes. There may also be, even in this short course, scattered small masses of pneumonic consolidation. The kidneys are not usually so early affected.

For these reasons it is deemed advisable to use the term laryngeal diphtheria to apply to this lesion, as expressing more accurately the nature of the disease, and to reserve the term membranous croup for the croupous larnyngitis of medical history.

TO SUMMARIZE:

1. The cases here studied are 116 in number; 90 of which were sporadic, 26 epidemic. They all occurred in the New York Foundling Asylum, an institution which has the constant care of 1800 children from the ages of a few weeks to five and seven years.

2. In the sporadic cases the laryngeal tube of Dr. O'Dwyer caused no ulceration worthy of consideration.

3. In the epidemic cases, numbering 26, there were five serious ulcers.

4. The causes of death have been mostly extension of pseudo-membrane to the bronchi, and pneumonia.

5. I have been able to find *no* evidence that milk or other foreign material has found its way into the lungs. The pneumonia is broncho-pneumonia and not schluck pneumonie.

DISCUSSION.

REMARKS BY THE PRESIDENT.—Membranous croup has always been regarded as a most important disease, on account of the extreme suffering which it causes, and its usual fatal termination. It has in recent years become much more prevalent than formerly, through the spread of diphtheria, for wherever diphtheria prevails membranous croup is common as one of its local manifestations. In New York city, where diphtheria has been established since 1858, 4982 died of croup during the six years ending with 1886, or more than two, in the average, each day. During the same period 8925 died of diphtheria without croup. Therefore, in this city about one-third of those who die of diphtheria have croup, for it is admitted by all New York physicians that there is scarcely a case of membranous croup within the city limits except in connection with diphtheria. In Brooklyn, during the seven years ending with 1886, 2506 children died of croup, or one each day, and 5011 died of diphtheria without croup, so that, as in New York, one-third of those who die of diphtheria die of croup. The increasing prevalence of croup as diphtheria extends and becomes established, is equally shown by the statistics of other cities.

We will now have the opportunity, which all of us will appreciate, of learning the anatomical characters of croup from one whose opportunities for studying its pathology are not equaled in the Western hemisphere.

Digitized by Google

LE TUBAGE DU LARYNX CONTRE L'ASPHYXIE DU CROUP ET LES RÉTRÉCISSEMENTS DU LARYNX.

INTUBATION OF THE LARYNX IN THE ASPHYXIA OF CROUP AND LARYNGEAL STENOSES.

DIE INTUBATION DES KEHLKOPFES GEGEN DIE ASPHYXIE DER CROUP UND LARYNX-STENOSEN.

PAR LE DR. E. BOUCHUT,

De l'Hôpital des Enfants Malades à Paris.

En présence des résultats si défavorables donnés par la trachéotomie dans l'asphyxie du croup ; de la difficulté de cette opération pour un grand nombre de médecins peu expérimentés, des accidents qui accompagnent parfois cette opération, des nombreux cas de morts qui ont lieu sur la table opératoire dans nos hôpitaux et en ville, j'ai pensé qu'on pouvait essayer de donner de l'air aux malades par les voies supérieures du larynx sans recourir à l'opération sanglante de la trachéotomie.

Il suffisait pour cela de placer dans le larynx par la bouche à l'aide d'une sonde conductrice, une canule dilatatrice d'un diamètre approprié qui permette le passage de l'air et le rejet des fausses membranes et des mucosités bronchiques. Cela est très facile. De cette façon on supprime toute opération sanglante et les dangers qui s'y rattachent.

J'ai fait ces essais en 1858, sur plusieurs enfants avec une réussite très satisfaisante. J'ai eu trois guérisons sur dix opérés et je vais communiquer au Congrès le dernier cas qui s'est terminé d'une façon très heureuse.

Hélène X. agée de 18 mois est reçue à l'hôpital des enfants malades et entre dans le service de M. Bouchut, le 12 Janvier, au No. 1 de Sainte-Cathérine.

Elle est malade depuis quatre jours environ, le visage est maigre ; elle a une grande difficulté pour respirer, et la respiration est accompagnée de cornage.

A chaque inspiration les muscles inspirateurs sont fortement contractés. Les narines se dilatent et le diaphragme est refoulé en haut de façon à présenter cette forme de dyspnée qu'on appelle le *tirage*.

Après l'examen de la gorge, où l'on constate l'existence de fausses membranes, on diagnostique le croup.

Le 12, les conditions de la malade sont extrêmement graves, il y a lividité de la face, les muqueuses sont cyanosées et la diminution de la sensibilité ou anesthésie caractérisant l'asphyxie annonce une fin prochaine.

L'âge du sujet laisse les assistants indécis au sujet de savoir si l'on fera la trachéotomie. A mon arrivée j'examine la malade et décide le tubage de larynx, je fais de suite l'opération.

Opération.—Pour cela je pris un tube ou canule d'argent de 0''' 2c. de long et de 0''' 007m. de diamètre, ayant à son extrémité supérieure un mince bourrelet et un petit trou dans lequel on fait passer un gros fil de soie.

J'introduis dans le tube à titre de mandrin conducteur l'extrémité d'une sonde d'homme, pourvue elle même d'un petit relief d'arrêt, et j'ai soin de tenir dans ma main le cordon en soie de façon que le tube soit fixé au-dessous du bourrelet de la sonde par le fil qui est dans ma main avec la sonde. La malade est couchée sur le dos, la tête soulevée par deux coussins, et pendant l'opération elle reste toujours dans la même position.

Se place dans la bouche de l'enfant l'index de la main gauche, protégé par un long anneau métallique qui laisse découverte par une dernière phalange du doigt, et j'arrive ainsi à toucher la glotte. En la conduisant sur le doigt, je pousse la sonde garnie de

Digitized by Google

sa canule dans le larynx, et si elle est d'un diamètre approprié, à l'aide de son bourrelet saillant, elle reste fixée entre les cordes vocales. Je retire alors la sonde conductrice en ne tirant pas sur le fil de soie, et ce fil resté libre dans la bouche, je le fixe au cou de l'opérée. Tout cela s'effectue dans l'espace de quelques minutes.

Aussitôt après l'opération la malade porte plusieurs fois les mains au cou et à la bouche, comme pour exercer des tractions sur le fil. Le corps étranger placé dans son larynx, tout en ne lui causant aucun phénomène réflexe, la gênait évidemment.

Peu-à-peu elle devint plus tranquille et la respiration devint plus facile. Elle put boire sans suffocation, et manger un potage.

Le 14, quoique dans un état toujours assez grave la malade n'eut plus d'accès de suffocation. La dyspnée persista, mais sans les caractères inquiétants qui avaient nécessité l'opération. Elle eut plusieurs accès de toux désagréables. Le bruit de scie persiste, la face est rosée, la sensibilité normale, l'intelligence intacte.

Le 14, j'enlève le tube, en pénétrant avec l'index de la main gauche jusqu'au contact de la glotte, et en exerçant de petites tractions sur le fil. Peu après la petite malade commence à devenir inquiète, le nombre des respirations augmente, les lèvres se cyanosent.

Le tube dans lequel étaient amassées quelques fausses membranes est nettoyé et réappliqué immédiatement de cette façon on a pu éviter l'accès de suffocation qui se préparait.

Le 15, l'enfant pendant la journée d'hier a été tourmentée d'une dyspnée intense entrecoupée de rémissions longues.

Au moment de la visite la peau est froide, le pouls petit, les respirations fréquentes et encore difficiles.

Le 16, la malade respire mieux, reprend ses forces, et passe le reste de la journée dans de bonnes conditions. La diarrhée a diminué, elle a dormi la moitié de la nuit, mais le tube est sorti pendant le sommeil, et il y eut un accès de suffocation. Le tube est remis.

Le 17, peu d'instants après l'application du tube, il y eut une amélioration notable, la journée a été calme, la nuit tranquille malgré quelques secousses de toux; la face est moins colorée, les mains sont chaudes. La respiration est à peu près normale.

On Enlève le Tube.—L'amélioration continue, la journée et la nuit ont été bonnes, la respiration, accompagnée du bruit de scie, est encore assez difficile, mais moins fréquente, la malade a envie de jouer.

Les 19 et 20, même état.

Le 21, la respiration, malgré le bruit de scie, est bonne, toux légère persistante.

Les phénomènes de la maladie s'améliorèrent peu-à-peu, et la malade quitta l'hôpital le 27 Janvier, 1858, guérie complètement.

Comme on le voit dans cette observation, le tube placé dans le larynx au moment où l'enfant était asphyxiée demi anesthésique, a été bien supporté. Il s'est engoué alors, on l'a retiré à l'aide de fil de soie, et il a été remis. Au sixième jour, il a pu être enlevé définitivement. La respiration s'est rétablie par degrès et la guérison était complète au quinzième jour.

Digitized by Google

[ABSTRACT.]

TUBAGE OF THE LARYNX IN STRICTURE AND IN THE ASPHYXIA OF CROUP.

BY DR. E. BOUCHUT,
Of Paris.

In view of the unfortunate results of tracheotomy, the difficulty which is met by inexperienced physicians in performing the operation, the accidents which sometimes occur, and the number of deaths which take place on the operating table in our hospitals and in private practice, I looked elsewhere for some means of conveying air to our patients, through the larynx, without operative interference.

The end sought was gained by placing in the larynx, with the help of a guide passed into the mouth, a tube wide enough to allow the admission of air and the expulsion of false membrane and bronchial mucus. This easy procedure took the place of the bloody operation and its attendant perils.

In 1858 I performed several experiments upon infants, with very good results. I had three recoveries in ten cases. I will relate to the Congress the last of these cases which recovered :—

Helen X.; aged eighteen months; was admitted to the Children's Hospital in my service on January 12th, *au No. 1 de Sainte Catherine.* She had been sick about four days. There was emaciation, great difficulty of breathing and wheezing expiration. At each inspiration the respiratory muscles were violently contracted, the nostrils were expanded and the diaphragm was pushed up in the manner which characterizes what is called the drawing respiration (le tirage.) Examination of the throat led to the diagnosis of membranous croup.

On the 12th the symptoms were very serious. The lividity of the face, the blueness of the mucous membrane and the failure of sensibility which attends asphyxia, indicated the approach of death. The age of the patient caused my assistants to hesitate before performing tracheotomy. On my arrival I decided on tubage of the larynx by the following method :—

Operation.—I procured a silver tube, or cannula, about two cm. long and seven mm. in diameter, having at its upper end a flange and a little hole through which was passed a silk thread. I introduced in the tube, as a guide, the end of a urethral sound, on which there was a projection or shoulder, and took care to hold in my hand the silk thread in such a way as to fix the cannula against the lower side of the shoulder of the sound, which was held in the same hand with the thread. The child was placed on her back, with the head on two cushions, and this position was maintained during the operation. Introducing into the child's mouth the index finger of my left hand, protected by a long metallic ring which left uncovered only the distal phalanx, I touched the glottis. Guiding it by the finger, I pushed the sound and its cannula into the larynx, and as it was of the right diameter, with the help of its flange, the cannula was retained between the vocal cords. I then withdrew the sound without disturbing the silk thread, which I fastened to the child's neck. All occupied but a few minutes.

Immediately after the operation, the child several times raised her hand to her neck and mouth to pull away the thread. The foreign body in the larynx caused no reflex phenomena, but was apparently an annoyance. By degrees she became quiet and the respirations easier. She was able to drink and took some broth.

Jan. 14th. Although still in an alarming condition the patient had had no attacks of suffocation. The difficult breathing persisted, but without the threatening symptoms which call for an operation. She had several distressing attacks of cough.

Digitized by Google

The wheezing continued, the face was flushed, and the sensibility and intelligence were normal.

Jan. 14th. I removed the tube, introducing the index of the left hand as far as the glottis and drawing gently on the thread; very soon the patient became restless, the respirations increased in frequency and the lips became blue. The tube, which contained a mass of false membrane, was cleaned and replaced without delay, and thus was prevented an immediate attack of suffocation.

Jan. 15th. I learned that the child had been troubled the preceding day by attacks of intense dyspnœa, occurring at long intervals. At the time of my visit the skin was cold, the pulse small and the breathing labored.

Jan. 16th. The patient was breathing better, was stronger and had passed the preceding day comfortably. The dyspnœa was less and she had slept half the night. But during sleep the tube had been expelled and there was an attack of suffocation. It was replaced.

Jan. 17th. Shortly after the reapplication of the tube there was marked improvement. The day and the night had been quietly passed, except for several coughing spells. The face had a better color, the hands were warm and the breathing was nearly normal. The tube was finally removed. Improvement continued. Day and night she was comfortable. The breathing was still noisy and not entirely easy, but it was less frequent, and the child wanted to play.

Jan. 19th and 20th. The same.

Jan. 21st. The breathing, in spite of a slight wheeze, was good. Slight cough persists.

The symptoms of the disease gradually disappeared and the patient was discharged, entirely well, January 27th, 1858.

In this case we have seen that a tube placed in the larynx when the patient was asphyxiated and half unconscious was well borne. It became obstructed and was removed with the aid of the silk thread, and was replaced. On the sixth day it was finally removed. Respiration was established by degrees and recovery was complete on the fifteenth day.

INTUBATION OF THE LARYNX.

INTUBATION DU LARYNX.

INTUBATION DES LARYNX.

BY JOSEPH O'DWYER, M. D.,

Attending Physician to the New York Foundling Asylum.

In the beginning of the year 1880 I began my experiments, at the New York Foundling Asylum, with the operation now known as intubation of the larynx. As the history of the evolution of this operation is long and of no particular interest to any one except myself, I will merely describe briefly, as I show them, a few of the more important modifications that have been made in the tubes.

About the time referred to tracheotomy was looked upon with marked disfavor at the Asylum, for the reason that we could not show a single recovery to demonstrate its

Digitized by Google

usefulness. We had no argument to offer in favor of it, except euthanasia, and not a few of even the most intelligent of the laity fail to understand how a child's suffering can be relieved by cutting its throat.

The failure of tracheotomy in my hands, not only in hospital but private practice also, was, therefore, the incentive that led me to seek a substitute. The one that naturally presented itself to me, as it had to hundreds of others, was a flexible catheter passed into the larynx through the mouth or nose.

Dr. E. R. Chadbourne, then resident physician to the New York Foundling Asylum, and to whom I am much indebted for valuable assistance in the early part of this work, also used a prostatic spiral catheter inserted into the larynx, through the nose, with relief to the urgent dyspnœa in several cases. A brief trial of this method was sufficient to demonstrate that it was impracticable, but it was the use of these long tubes that suggested the short ones. The end of a catheter projecting from the mouth or nose, besides other objectionable features, gave the child an opportunity to seize and remove it whenever it succeeded in freeing its hands. Therefore, why not shorten it so as to rest within the nostril or in the pharynx, with some device to prevent it from slipping down; and, finally, why not solely in the larynx?

Here was a large cavity above for the lodgment of a tube with expanded upper extremity and narrow opening below, to prevent it from slipping through. As soon as thought of, it was almost demonstrated that a tube could be so constructed that one end would extend into the trachea while the other occupied the vestibule of the larynx, and thus permit the epiglottis to close over it during the act of swallowing. But here was encountered the first stumbling block. What was to hold it down? What would take the place of the tapes tied around the neck that served to retain the tracheal cannula? I regarded it as self-evident that a plain tube of this kind, having the form of the rima glottidis, would be immediately rejected, and, therefore, did not consider it worth a trial.

After much thought on the subject the only solution of the problem that occurred to me was to construct the tube in the form of a bivalve, the blades of which would separate when inserted, and thus give a greater transverse diameter below than between the vocal cords. I tried several modifications of this form, which always proved self-retaining and gave prompt relief to the dyspnœa, but the triangular open space between the blades allowed the swollen tissues to intrude, and also served as a lodging place for detached fragments of pseudo-membrane.

When fully convinced of the failure of this form I was obliged to fall back on the plain tubes that I considered it useless to try in the beginning. They were made of the same oval or elliptical shape and of the same length, about one inch, with a small button hole in the posterior angle near the upper extremity, by which they could be removed. These tubes were retained much better than I expected, but this, as I subsequently learned, was due to the protrusion of a small process of the mucous membrane through the opening just referred to, which had to be dispensed with, as it formed a nucleus for the accumulation of secretions. When I procured tubes without this opening I soon found that they would not be retained except in very young children, or by using a tube a size too large to be safe. I then increased the length so as to bring the lower end within about one-fourth of an inch of the bifurcation, believing that this means would prevent their expulsion, and at the same time diminish the danger of obstruction from loose or partially detached masses of pseudo-membrane. In order to determine the proper length for these tubes it was necessary to make a large number of measurements of the trachea at different ages.

While these tubes were not so frequently rejected, coughing, or even bending the neck, was sufficient to force them upward above the tip of the epiglottis, where, owing to their lightness, they would remain until pushed down by the finger. On inspecting

Digitized by Google

the pharynx after an attack of coughing they could often be seen in this position, and sometimes projecting behind the soft palate.

To the sides of the same tubes I now added a second shoulder about one-fourth of an inch from the upper extremity, which left a furrow between for the vocal cords to rest in. In order to prevent this second shoulder from offering any obstacle to the introduction of the tube it was made wedge-shaped with the thick end directed upward. This device proved satisfactory in retaining the tubes, but rendered their removal very difficult with the imperfect extracting instrument I was using at that time, and was, therefore, not used in many cases.

The difficulty was finally overcome by a very simple device, without interfering either with the insertion or removal of the tubes, and consisted in increasing the transverse diameter in the middle by leaving the metal thick at this point and tapering gradually toward either extremity. A tube constructed in this way, when projected upward by coughing, will slip back into position by pressure of the vocal bands on the sloping sides, aided by its weight, unless the thickest portion should reach the chink, when it would, in all probability, be expelled.

This retaining swell is only made thick enough to hold the tubes loosely in the larynx in order to permit of their easy expulsion in case of sudden occlusion by masses of pseudo-membrane too large to pass through. Owing partly to this and partly to the difference in the size of the larynx in different children of the same age, they will sometimes be expelled when perfectly free from obstruction and before the laryngeal stenosis has been permanently relieved.

The difficulty that I encountered in devising a means of preventing the rejection of the tubes was evidently not experienced by Bouchut, from the fact that he makes no mention of it in his reports. The explanation is easily found. Bouchut used tubes of cylindrical form and of large size, which were retained by the pressure exerted on all sides, especially the lateral pressure of the vocal cords; and it was this that furnished Trousseau with the only argument, founded on fact, that he could bring against the operation, viz., that when used in the larynx of the dog they produced ulceration, not only of the soft parts, but destruction of the cartilage as well, and the inference was that the same would take place in the child's larynx.

I have here a tube that is not even cylindrical, which has nothing to retain it except its size, and nothing more is necessary. If placed in the larynx of a child six or seven years old it would be retained indefinitely. It is short, of large caliber and was intended to serve a double purpose.

1. To facilitate the expulsion of masses of pseudo-membrane, when such is known to exist in the trachea and which cannot be expelled through the small lumen of the ordinary tubes. As yet, I have not used it for this purpose.

2. As a laryngeal dilator: I have noticed in several cases of croup that the dyspnœa was relieved for several hours, and sometimes for as long as two days, when the tube had been retained for only a very short time. A larger tube producing a greater amount of dilatation should accomplish more. So far I have not given it a satisfactory trial in this respect, but sufficient to demonstrate that it is useless in the ordinary severe forms of croup, especially in young children. But in cases of slow development, in older children, who require only a little help to struggle through, using this form of tube and leaving it in for two or three hours, with string attached, will give a better chance of recovery, even if it should be necessary to insert it often, than wearing a tube continuously for several days. Tracheotomists all recognize the importance of getting rid of the cannula at the earliest possible moment, and the same rule applies to that in the larynx.

Impressed with the importance of this, I tried to hasten the disappearance of the

Digitized by Google

stenosis by coating the tube with various astringents incorporated in gelatine, but so far without result.

Previous to the time of making the modification last described, I was using tubes with very small shoulders, that would rest on or in close proximity to the vocal cords, in order to permit some contraction of the ary-epiglottic folds above the tube, to aid the epiglottis in protecting the larynx during the act of swallowing. Theoretically, deglutition should be less difficult with the tube thus deeply placed in the larynx than with the large-headed tubes now used, that ride much higher; but, practically, I have found no difference. At the same time it is proper to state that Dr. Waxham's experience with the same tubes does not coincide with mine.

There are two serious objections to the small-headed tubes :—

1. The danger of being pushed through into the trachea in attempts at extraction.
2. The tendency of the swollen tissues to overlap and obstruct the upper opening.

With the large-headed tubes now in use the first accident is impossible unless the glottis is first lacerated by passing the extractor down beside instead of into the tube, and forcibly removing it while the blades are widely separated. I saw this occur after several unsuccessful attempts at extraction had been made. When I saw the case the tube could not be felt by the finger, and I had much difficulty in removing it. A larger size was used, which also passed so far down that the tissues closed over it and it was removed by the string, which was still attached. I did not understand the case at the time, but the explanation came soon after, during practice on the cadaver. The tube slipped into the trachea and I removed the larynx and found the cricoid cartilage completely cut through anteriorly and the glottis more than doubled in size. I was thoroughly convinced that no matter how extensively the soft parts might be lacerated the tube would still be held by the cricoid cartilage, as it was on this I relied, more than on the vocal cords, to hold the small-headed tubes that I used so long without this accident.

To diminish this danger as much as possible, I have added a regulating screw to the extractor, which prevents the blades from opening to their full extent.

About the same time that I increased the size of the heads I reduced the length of the tubes from one-fourth to one-half inch, in order to facilitate their introduction, and also gave a backward curve to the upper extremity, to carry it away from the base of the epiglottis, where the straight tubes frequently produced ulceration. I have recently been trying tubes having a still greater posterior curve, believing that they would interfere less with the functions of the epiglottis. But while I have noticed no difference in this respect, I have, so far, failed to find any ulceration or even abrasion of the mucous membrane on the anterior wall of the trachea corresponding to the lower end of the tube. With the tubes in general use ulceration at this point is sometimes found, and the manner in which it is produced I will describe later.

I suppose that without exception the first criticism passed on these tubes by physicians who see them for the first time is that the caliber is too small to admit a sufficient quantity of air to the lungs or to afford room for the passage of secretions. It will therefore be necessary to give my reasons somewhat in detail for adopting tubes of small in preference to those of large caliber, as this is contrary to the teaching of all tracheotomists.

If a large cannula is better than a small one for the trachea, it is equally so for the larynx, the same arguments applying to both.

I began with tubes of about the same caliber as those generally used by tracheotomists, but the change from the cylindrical to the oval shape, while giving a long antero-posterior diameter which conformed to the chink of the glottis, where there was ample room, did not conform to the sub-glottic division of the larynx, which, like the trachea,

Digitized by Google

is cylindrical but very much smaller than it. The difference in size between these portions of the breathing tube will be best appreciated by comparing a section from the cricoid cartilage with one from the trachea of the same subject.

I found ulceration in this portion of the larynx at points corresponding to the long diameter in every case in which a tube of such size had been retained for any length of time.

This defect could only be remedied by using cylindrical tubes which would conform in shape to the cricoid portion of the larynx or by diminishing the long diameter at the expense of the lumen of the tube. The former plan would materially increase the pressure on the vocal cords, with the consequent danger of ulceration and permanent impairment of function. I therefore adopted the latter method of shortening the antero-posterior diameter, until ulceration ceased to occur, and demonstrated, by using these tubes of diminished caliber in a large number of cases of croup, that there was ample breathing room left, and further, by using them on children several years older than the age for which they were intended.

Several times, on removing the tube at the usual time, the fifth or sixth day in cases that were progressing without dyspnœa or other bad symptoms, I have found the lumen reduced to one-half or less, by closely adherent gummy secretions.

In one child two years old, that had considerable dyspnœa, which was attributed solely to broncho-pneumonia that was present, I found the tube, when removed on the fifth day, to all appearances completely stopped up. On passing the obturator through it, what looked like a solid cast was pushed out. Had I found it in this condition on removal after death, I would have certainly attributed the fatal result to this cause.

Such cases prove conclusively that the full lumen of even these small tubes is not necessary for the free entrance and exit of air in a state of rest.

A still more convincing proof of this fact is, that I have used the tube intended for children from eight to twelve years of age in a woman forty years old who had chronic stenosis of the larynx. The only time that this patient was able to assume the recumbent posture and obtain a comfortable night's sleep, in over two months, was the first night that she had this little tube in her larynx.

I might quote many similar examples in which respiration was carried on through a small fraction of the normal lumen of the larynx or trachea and yet air enough be admitted not only to sustain life but also to be compatible with health and comfort in a state of repose. Nature has supplied us with a superabundance of breathing space in order to provide for the great increase in the consumption of oxygen during violent muscular exercise and to allow for considerable curtailment, by the various diseases to which the respiratory organs are liable, without endangering life.

There is, therefore, no necessity that a tube in order to be large enough for free respiration should even approximate in size to that of the larynx or trachea.

As far as respiration alone is concerned, no objection can be offered if a tube be many times larger than is necessary, for such exists normally. But there is another reason besides the mere entrance and exit of air, that demands serious consideration, viz., the removal of the secretions that are almost constantly present in the lower air passages in croup, owing to the frequent association of bronchial catarrh with this disease.

While the extrusion of masses of pseudo-membrane is accomplished with greater ease and with less danger of sudden occlusion through a tube of large than of small lumen, I believe the reverse is the case with the ordinary secretions, and it is on this ground alone that I claim an advantage for tubes of small over those of large caliber. Secretions, if not removed in the ordinary manner, will accumulate and be drawn downward into the alveoli, and not only interfere with the important function of the lungs, but also give rise to inflammatory changes.

If we glance for a moment at the mechanism of cough, nature's method of keeping

Digitized by Google

the air passages clear, we find that after the lungs are filled by a deep inspiration, there is a violent spasmodic expiratory effort coincidently with complete closure of the glottis, by which the imprisoned air is subjected to a marked degree of compression before it is permitted to escape. It is this compression alone which imparts to the air that friction power which is sufficient to scrape off, so to speak, tenacious secretions from the mucous membrane of the trachea and bronchi. No one will claim that there is any power to expel secretions with ordinary expiration, nor with the most forcible and rapid expiration, as long as the vocal cords retain the position occupied when their antagonizing muscles are in a state of rest—that is, the cadaveric or expiratory position. Close approximation of these organs is absolutely necessary to give full expulsive power to the cough; some degree of approximation to give any at all. This can be demonstrated by any one who will try to cough with an open glottis. A familiar and still more forcible illustration is the simple act of blowing the nose, which is almost identical with coughing. If both nostrils are clear, even closing one is not sufficient to give much power to expel secretions; both must be almost or completely occluded in the same manner as the glottis, until the air has been compressed.

With these facts before us, it is not difficult to understand the manner in which the act of coughing is modified by a tube in the larynx or trachea. There is no longer the ability to close the glottis, and the air escapes before it has undergone any considerable degree of compression, and consequently its expulsive power is impaired, and the impairment will be in proportion to the size of the tube.

Coughing, therefore, through a tube of small caliber, by retaining the air better, stimulates in some measure the normal method, which is the strongest argument that can be offered in favor of it.

It is this inability to promptly get rid of secretion after intubation or tracheotomy that I regard as the most fruitful cause of pneumonia as a complication of croup. In my experience, this complication is rare in cases that struggle through without operative interference.

Trousseau, who first recommended the use of large tracheal cannulas, gave as a reason for this that the improvement that at first succeeds the operation of tracheotomy in some cases, soon gives place to a return of the dyspnœa, and attributes this change to the inadequate size of the cannula employed, which does not provide for the permanent admission of a sufficient quantity of air. In illustration of this fact, he says: Place a quill in the mouth, and closing the nostrils, endeavor to breathe entirely through it; at first you breathe easily enough, but soon your respiration becomes laborious, and at length you are fain to throw away the quill, and with open mouth once more to fill your lungs completely.

This, I claim, is no illustration at all, for the simple reason that the difference between the size of the adult larynx and a quill is so enormous that the comparison is absurd.

If any one wishes to try this experiment, and will use a tube bearing anything near the same proportion to the lumen of the adult air passages that the smallest tracheal cannula ever used bears to those of the child, he will find no difficulty, except the feeling of oppression produced by closing the nostrils, in breathing through it for an indefinite time. To verify this statement to some extent, I will show you an adult larynx and trachea with tube in situ. This tube was worn by three different persons, from one to three weeks at a time, all of whom could breathe through it with perfect comfort. With it I will also pass a larynx and trachea from a child four years old, with tube of proper size in position.

No measurements are necessary to demonstrate that the lumen of the small tube is greater in proportion to that of the trachea which contains it, and consequently affords

Digitized by Google

more breathing room than the lumen of the large tube compared with that of the adult trachea.

The early return of the dyspnœa, to which Trousseau alludes, and on which he based his argument in favor of larger tubes, can, with greater probability, be attributed to the entrance of blood or the accumulations of secretions, or both, than to the inadequate size of the cannula employed. I have repeatedly observed marked improvement to the dyspnœa follow the removal and reinsertion of a tube that was perfectly clear, from the expulsion of retained secretions by the increased cough excited by the operation and the deeper respiration induced by the struggles of the child.

Such a result following the introduction of a larger tube would naturally be attributed to the increased breathing room thus afforded.

Another subject to which I will briefly call your attention, and one that is of interest principally in relation to intubation, is the seat of the stenosis in croup.

It had heretofore been universally regarded as confined to the chink of the glottis, but while this is the rule, there are exceptions, where the fatal obstruction is situated lower done in that narrow portion of the larynx bounded by the cricoid cartilage. As it can only be demonstrated in cases that have died without operative interference, and the opportunity of examining such is rare, I can form no estimate of the frequency with which it occurs.

I have here a specimen illustrating this, but to a much less marked degree than I have seen in other cases. One cross section is made through the cricoid cartilage about a quarter of an inch below the cords, and another a little lower down or just through the beginning of the trachea, where a small quantity of pseudo-membrane can be seen.

If any fibrinous deposit exist above it is insignificant, the obstruction being due solely to the infiltration of the mucous membrane and underlying tissues.

The opening that still exists, and which, in all probability, is greater than was present during life, although very small compared with the normal caliber of this portion of the larynx, appears to me, when compared with other cases, large enough to admit a sufficient quantity of air to sustain life. The little patient from whom this specimen was taken had been suffering from double broncho-pneumonia and naso-pharyngeal diphtheria, complicating a severe type of measles, for some days before the development of croup.

When I saw the case, a few hours before death, it appeared to be getting as much air as the crippled lungs could use, and did not show any signs of suffering in this respect. It had a good voice and noiseless breathing, and on these symptoms, especially the former, I made a diagnosis of sub-glottic stenosis.

The fatal issue was, therefore, only hastened by the laryngeal obstruction.

This specimen also shows marked stenosis of the rima; the vocal cords are seen lying very close, but from this it would be impossible to estimate the amount of breathing-room that may have existed during life, by reason of the forcible inspiratory expansion of the glottis.

In sub-glottic stenosis, on the contrary, we can estimate very closely as to the amount of breathing-room that existed during life, as this portion of the larynx is surrounded by an unyielding cartilaginous wall, and not susceptible of any other change except decrease of its caliber. Swelling of its mucous membrane, which is always more or less involved in croup, can take place only toward the centre, and the opening found after death represents very nearly that which existed during life. For the same reason it will be easily understood that obstruction at this point will offer more or less resistance to the passage of a tube which will require some force to overcome. As a matter of fact, some force is necessary, but it is slight in comparison to what we would be led to expect from examining a larynx such as I have just shown.

Digitized by Google

Intubation has still two very serious defects to be overcome. First, the difficulty of swallowing fluids while a tube remains in the larynx. Second, the difficulty of removing large masses of pseudo-membrane from the trachea through the natural channels. With the object of remedying the former, I have tried several modifications in the shape as well as the size of the heads of the tubes, the most important of which I will show you. I have found no improvement in deglutition with any of them.

The one presenting a concave upper surface would render the insertion of the extracting instrument a very easy matter, if further experience should prove that swallowing was even as good as with the ordinary tubes, that are slightly convex above. The same objection applies to this as to the funnel shape given to the aperture in the upper extremity of the early forms, namely, that while the extractor enters more readily fluids will do the same.

I believe it is only a question of time until some means will be found to overcome this difficulty, and it may be in some modification of Waxham's device of an accessory rubber epiglottis.

For the removal of pseudo-membrane from the trachea, I have had three instruments made, none of which I have yet tried, except on the cadaver. The curved portion of this instrument, when left long enough to reach to the bifurcation of the trachea, renders its insertion into the larynx impossible, except by passing the finger behind the lower extremity and bending it forward as it reaches the neighborhood of the arytenoids. It would probably be easier in the living subject, where the larynx occupies a higher position than after death. This instrument can be covered with thin rubber in the same manner as Otis' urethrometer, after which it is constructed.

I first tried to have an instrument made on the principle of the œsophageal bristle probang, using piano wire and watch-spring steel, but failed.

The second instrument is more suitable for breaking up false membrane, in order to render its expulsion by coughing less difficult.

The last device of this kind promises the most. It consists of a conical and very flexible soft silver tube—the one I have here is a little too stiff—with a rubber air chamber at either end. It is intended to be inserted with a short curve and pushed down in the same manner as an elastic catheter would be. When well down, the lower air chamber is inflated by pressing on the bulb attached to the handle, and withdrawn in this condition. The bulb attached to the handle should only contain sufficient air to fill the lower one, otherwise there is danger of rupturing it. The latter can be divided into several compartments, like a sausage, by tying it down at one or more points.

I have had a cylindrical tube made, funnel-shaped above, to pass these instruments through. This has the advantage over passing them directly through the larynx, of giving the patient a chance to get some air during the operation.

A set of tubes suitable for children of all ages under puberty is now composed of six instead of five, as formerly. I found that while the largest of the old set intended for children from eight to twelve years was well retained in girls, boys of the same age frequently rejected them. The sixth tube can, therefore, be held in reserve in case the other is expelled, or it can be used in boys from nine to twelve years inclusive.

If used after puberty, a string should be left attached and fastened round the ear as a matter of safety, as I have no knowledge, so far, in regard to the size of the larynx during the period of adolescence.

No one for a moment would suppose that a good workman, with a set of these tubes before him, would find any particular difficulty in copying them, but it is a fact, nevertheless, that the best instrument makers in New York have succeeded in reproducing them only after many failures and repeated instructions. Some gave it up after several trials, and no two now make them exactly alike.

Digitized by Google

As they are being made to a considerable extent by those who have never received any instruction whatever, I will point out the most important defects found in such tubes and the accidents liable to result from what appear to be mere trifles.

The first of these is leaving out the backward curve given to the upper extremity, or that portion of it covered by the shoulder. This can be shown by placing any straight surface in contact with the anterior portion of the tube. The object of this curve is to carry it away from the base of the epiglottis, where the straight tubes formerly used always left their mark, sometimes in the form of a perforating ulcer. With the large-headed tubes now used this curve is even more necessary, and for another reason: while the small-headed tubes swing like a pendulum from the vocal bands, the large head, by holding the tube higher in the larynx, converts it into a lever, with the posterior commissure acting as the fulcrum.

During every act of swallowing the upper short arm of the lever is pressed backward with considerable force by the base of the tongue, which pushes the epiglottis before it, and tilts the lower end against the anterior wall of the trachea.

This is the cause of the ulceration not unfrequently found in that situation.

This curve is the great stumbling block to the instrument makers, who find great difficulty in making the obturators fit accurately with it.

Another important defect is leaving the metal thin on the anterior and posterior aspects of the lower extremity, where it should be thick enough to form a blunt, rounded surface that will not injure the mucous membrane.

A tube with these two defects has all the elements necessary to produce extensive ulceration on the anterior wall of the trachea.

The metal should likewise be left thick on the anterior surface of the upper extremity, to prevent the formation of a cutting edge under the epiglottis, but as thin as possible at all other parts of that portion of the tube that occupies the larynx. Many not knowing the object of this, have regarded it as a serious defect, by occupying space that should be added to the caliber.

The tube, when attached to the introducer and ready for use, should be immovable, and this, owing to the joint in the shank of the obturator, together with the curve in the tube, is very difficult to obtain, and can only be secured by accurate fitting of the upper part of the obturator. Any motion of the tube causes it to slip off as soon as it comes in contact with the base of the tongue or other part, which is very embarrassing even to the expert operator. Other defects of minor importance I will not mention.

The introduction of a tube into the larynx of a struggling, half-asphyxiated child is not the simple operation that those who have not tried it believe it to be. An operator who possesses more than usual dexterity, combined with coolness and an easy case to begin with—for there is a great difference in this respect—will experience no particular difficulty in properly placing the tube, and he will regard it as a very simple operation.

If, on the contrary, the operator who has less dexterity should, on his first attempt, encounter a difficult case, he will come to the conclusion that it is a difficult operation to perform, which it undoubtedly is in exceptional cases. Such was that of a child two years old, with very small mouth, small pharynx and soft, almost normal epiglottis. Two physicians of considerable experience with intubation failed, after repeated attempts, to place the tube in the larynx, and I only succeeded on the third trial. The same conditions, as regards room, obtain in very young children, but I have seldom encountered any difficulty, except when the epiglottis is not implicated to any extent in the disease and it is difficult to get the finger under it. In removing a tube from a child five months old, the epiglottis doubled on itself, and I was obliged to hook it up with the extractor; with a swollen, rigid epiglottis this does not occur.

When intubation of the larynx shall have attained that degree of perfection where further improvement is impossible—which will probably be many years hence—and,

Digitized by Google

what is equally important, when a sufficient number of operators will have acquired proficiency in the use of the instruments, this operation, as well as all other surgical measures, will still remain unsatisfactory in the treatment of croup, because of its many and fatal complications, and from the very nature of the disease itself.

I was thoroughly familiar with the pathology of croup treated with and without tracheotomy before the beginning of my experience with intubation, and knowing particularly how often the pseudo-membrane invades the small bronchi, I never anticipated any wonderful results from the operation, and never developed as much enthusiasm on the subject as many others have.

Those who obtain good results in the first few cases will naturally conclude that their success will continue, and they will over-estimate the value of the operation. Those, on the contrary, who have bad results in the beginning will condemn it as useless. The truth will be found between these extremes.

Placing a tube in the larynx or trachea is at best only a temporary means of admitting air to the lungs, and simply affords time for nature and art to subdue the disease.

What we need is a remedy that possesses a curative, or at least a controlling, influence on the inflammatory process that produces the fibrinous exudation. Evidence of the strongest kind is accumulating in favor of the bichloride of mercury as having the latter quality. As far as I have been able to learn, those who have used this remedy in conjunction with intubation have obtained better results than I have without it. I will mention particularly Dr. Francis Huber, of New York, who faithfully and persistently used the bichloride from the appearance of the first symptoms of croup, and obtained 20 recoveries in 47 cases intubated—the best results yet reported in an equally large number of cases.

Four of his recoveries were infants under one year, which is also significant, as the youngest recovery that I have had was fifteen months. Dr. Huber also says, in his paper read before the New York Academy of Medicine June 2d, 1887, that he was obliged to resort to intubation in only one out of every three or four cases of croup that he was called upon to treat.

I have had many cases that I can safely say came near getting well; only a little something more was necessary to pull them through; probably this little help was the bichloride, which they did not get.

In comparing the results obtained by tracheotomy and intubation, the most important question is not whether, in a given number of cases, one will save more lives than the other, but which operation can be performed or will be permitted in the greater number.

Tracheotomy never was and never could be popular, and has never been performed in any more than a fraction of the cases of croup, because of the great repugnance to any cutting operation that exists among the poorer class of people, where this disease principally prevails.

I know, from my own experience of twenty years in practice, and from conversations with a large number of physicians on the subject, that this operation has not been resorted to in more than one-fourth of the fatal cases of croup that have occurred in New York city, and which amounts to about one thousand a year.

This is certainly not from any scarcity of surgeons competent and willing to do the operation, but from the impossibility of obtaining the consent of the parents and friends of the patient.

Even if tracheotomy were performed in only a small percentage of the cases of croup, a disease that prevails so universally in every country, city, town and hamlet, the number of operations every year must mount into the thousands, and yet why is it so difficult to collect statistics of any considerable number outside of hospital reports? It is simply because the few only who obtain good or fair results publish their cases.

Digitized by Google

The exceptions to this rule are rare, because there is nothing to be gained and much to be lost by reporting bad results.

It is not uncommon for physicians who have been in practice for a quarter of a century in the large cities, and who, in all probability, have seen hundreds of fatal cases of croup, to report one or two dozen tracheotomies, usually with a large percentage of recoveries, proving that the operation was performed only in the most favorable cases and where good nursing could be obtained.

METHOD OF OPERATING.

A set of intubation instruments consists of six tubes, each supplied with a separate obturator, an introducer, an extractor, a mouth gag and a scale. The obturator, when in position, projects a little beyond the lower extremity of the tube, and is rounded off into a probe point, to prevent pushing down pseudo-membrane and injuring the tissues of the larynx. It also serves as a means of attachment to the introducer. The numbers on the scale indicate the years for which the corresponding tubes are suitable. The smallest tube, when applied to the scale, will reach the first line, marked 1, and is suitable for children of one year and under. This tube can be used with children of two years with perfect safety, as far as slipping into the trachea is concerned, but the probability of its being coughed out would be great. The second size reaches the line marked 2, and is suitable for children between one and two years old. The third size is for children between two and four years old, and the next size for children from four to seven years old. The next size, reaching the line marked 8–12 on the scale, is for children up to twelve years of age, but not after puberty, as the sudden increase of the size of the larynx at this time would render it liable to pass through into the trachea. The largest size is intended for those children whose larynx is so large that the No. 5 tube is not retained.

The female larynx in children, as in adults, is smaller than the male, which should be taken into consideration in selecting the tube, as well as the size of the child compared with its age. For instance, in a small, delicate girl of four years, a No. 2 tube would be the proper one, while, on the other hand, in some boys of three and a half years it would be advisable to use the 5–7 size, especially if the case is a considerable distance from you; and coughing out the tube would entail some danger and a great deal of inconvenience.

When the proper tube is selected for the case, a fine thread of braided silk is passed through the small hole near its anterior angle, and left long enough to hang out of the mouth, its object being to remove the tube should it be found to have passed into the œsophagus. The obturator is then screwed tightly to the introducer and passed into the tube. It is well to push off the tube once or twice before using it, to see that everything works smoothly.

The following is the method of introducing the tube, which is done without the use of an anæsthetic. The child is held upright in the arms of the nurse, with its head on her left shoulder, to avoid interfering with the mouth gag; and the arms are secured to the sides either by being held below the elbows by the nurse or by wrapping a sheet around the body. The legs should be secured between the nurse's knees. The gag is inserted in the left angle of the mouth, well back between the teeth, and opened as widely as possible without using undue force. An assistant holds the head, thrown somewhat backward, while the operator inserts the index finger of the left hand, to elevate the epiglottis and direct the tube into the larynx.

The instruments should be worked in the median line, the operator facing the patient squarely. The handle of the introducer is held close to the patient's chest in the beginning of the operation and rapidly elevated as the tube approaches the glottis. Very little force is necessary to overcome the obstruction, and if the tube does not enter the

Digitized by Google

larynx easily, it should be withdrawn and another attempt made. It should be pushed well down into the larynx before it is detached from the obturator, and while removing the obturator it is necessary to keep the finger in contact with the head of the tube to prevent it from being also withdrawn. The string should not be removed until the dyspnœa is relieved and the operator is certain that the tube is in the larynx. In some cases the tube causes so very slight an amount of irritation that I allow it to remain for ten or fifteen minutes to excite more cough, and thus expel the accumulated secretions and overcome any collapse of the lung that may exist. When the thread is withdrawn, the finger must be kept in contact with the tube to prevent its being also withdrawn.

In removing the tube, the child is held in the same position, but the head is not thrown quite so far back. The finger which is used as a guide for the extractor is brought in contact with the head of the tube, and then pressed toward the patient's right, in order to uncover the aperture and allow the extractor to enter in a straight line. Continuous pressure with the thumb is made on the lever above the handle while the tube is being withdrawn.

Owing to the small aperture of the tube compared with the size of the larynx, the extraction of the tube is more difficult than its insertion. It is during this part of the operation that the greatest amount of injury is liable to be done to the larynx by pushing the instrument down, outside of the tube, and removing it forcibly with the blades widely open. It requires no force whatever to remove a tube from the larynx, and if any resistance is felt, it will be found that the point of the extractor is not in the tube, but caught in the surrounding tissues. To reduce this danger as much as possible, I have added a regulating screw, which prevents the blades from opening any wider than is necessary to hold the tube firmly. It can be adjusted to suit a tube of any size.

INTUBATION OF THE LARYNX: ITS ADVANTAGES AND DISADVANTAGES, WITH STATISTICS OF THE OPERATION.

INTUBATION DU LARYNX, SES AVANTAGES ET DÉSAVANTAGES, AVEC DES STATISTIQUES DE L'OPÉRATION.

INTUBATION DES LARYNX: IHRE VOR- UND NACHTHEILE, NEBST STATISTIK DER OPERATION.

BY F. E. WAXHAM, M.D.,

Professor of Diseases of Children in College of Physicians and Surgeons of Chicago; Clinical Professor of Laryngology and Rhinology in Chicago Ophthalmic College.

Nothing in the history of medicine has excited more widespread interest in America than this new operation, which promises to supersede tracheotomy. Too frequently in the past have we been obliged to sit helpless and witness death from slow strangulation, because we have not been allowed by parents to perform tracheotomy. Too frequently, indeed, physicians in this country have been so unfortunate in their experience with tracheotomy that they have even doubted the utility of the operation. How natural, under these circumstances, that they should give a substitute that promised so much their hearty approval.

The origin of the operation in France, by Bouchut, some thirty years ago, and its failure, the revival of the operation in America by Dr. O'Dwyer, and his patient labor in modifying the instruments and making them of practical value are now facts

Digitized by Google

of history. Previous to 1885 he alone was performing the operation in America. During 1885 a few others, knowing of his work, joined in the research, while now, in only a little over two years, we find operators all over the United States, and the number of cases reported reaching one thousand. This is a record beyond parallel in the history of any operation, and is sufficient evidence that it is a procedure well worthy of earnest attention.

It is far from me to denounce or disparage tracheotomy. It is a noble operation—one that has saved many a life in the past, and will continue to save many in the future.

While advocating intubation we must not forget its dangers.

One of the greatest disadvantages of the operation is the difficulty of performing it. There are comparatively few who possess the manual dexterity necessary for performing the operation quickly and well. Those who are not intimately acquainted with the anatomy of the parts, or who are not naturally dextrous, must practice patiently and faithfully upon the cadaver, which is not always convenient or possible.

Another disadvantage is the danger of injuring the soft tissues from lack of skill in performing the operation. If the tissues are torn or bruised, or the trachea perforated, it can be readily seen that the chances of recovery will be greatly lessened.

Still another disadvantage is the difficulty of extracting the tube. It is generally conceded that the removal of the tube requires more skill than its introduction. If a tube with a very small head is used, so that it sinks deeply in the larynx, and beyond the reach of the finger, great difficulty will be experienced in removing it.

Still another danger results from pulling up the membrane below the tube, thus occluding it. To guard against this danger we must use judgment in selecting the tube. To use a tube that fits tightly will endanger the patient's life, as it cannot, in such a case, be rejected by sudden and powerful efforts, and the child dies from sudden asphyxia. We may also guard against this danger by watching the patient carefully, and quickly extracting the tube if not rejected. If the membrane is not then expelled we may use the trachea forceps or resort to tracheotomy. This danger, however, is a slight one; almost invariably the tube and membrane will be expelled together. Membrane may be pushed down ahead of the tube when it is introduced. In such a case the tube should at once be removed, when, if the membrane is not rejected, the trachea forceps should be used, or tracheotomy performed, as in the former case.

Still another disadvantage is the difficulty of swallowing and the consequent danger of pneumonia from the entrance of food and fluids into the lungs. I am convinced that this danger has been greatly exaggerated by the opponents of the operation. This danger may be obviated, first, by the careful selection of the tube, and, second, by the most watchful care in feeding and management of the case. Many, in selecting a tube, choose the largest that can be introduced. This is a great mistake, as it gives rise to two dangers: first, to sudden suffocation in case there is detachment of large patches of membrane below the tube, and, second, there *will* be greater difficulty of swallowing, and, consequently, greater danger of broncho-pneumonia. Let us observe the caution not to introduce too large a tube, and then let us be imperative in our directions in regard to the use of only semi-solids. Cracked ice and ice cream may be given to quench the thirst, and stimulants given only by enema. If we observe these precautions we will not frequently meet with pneumonia from this source. These are the dangers and the disadvantages of intubation. None are so grave that they cannot be overcome.

In contrast to the disadvantages we may enumerate many advantages which the operation possesses over tracheotomy. It can be performed by the expert quickly, almost instantly. There is no loss of blood to further prostrate the patient. There is no injury to the soft tissues, and little or no pain. There is no shock from the operation. There is no danger from septicæmia or erysipelas, as from an open wound. There

Digitized by Google

is very little irritation from the tube, much less than from a tracheotomy tube. There is no open wound to heal by slow granulation. The air enters the lungs through the natural passages, is warm and moist, and there is consequently no drying of mucus in the tube, and no cleansing is required. Recovery is rapid after removal of the tube. We can do with less skilled attention than after tracheotomy. We can, therefore, save many patients where poverty precludes the skilled attention so necessary after tracheotomy.

Consent of parents is much more easily obtained than for tracheotomy, and we will save many patients where this operation would not be allowed.

In addition to all these advantages there is still another—the most important of all: We can save as large a proportion of cases as by tracheotomy, at all ages, and a much larger proportion among children less than three years.

In collecting the following statistics I have been greatly assisted by Dr. Dillon Brown, of New York, and Dr. E. F. Ingals, of Chicago. These statistics show that the operation has been performed by—

Dr. J. O'Dwyer,	Private practice	81	times, with	20	recoveries.
	Hospital "	65	" "	9	"
" Dillon Brown		100	" "	23	"
" F. E. Waxham		120	" "	31	"
" F. Huber		52	" "	24	"
" J. M. Bleyer		42	" "	11	"
" W. P. Northrup		33	" "	6	"
" E. F. Ingals		12	" "	3	"
" S. A. McWilliams		3	" "	0	"
" Wm. Cheatham		15	" "	1	"
" Geo. W. Mason		4	" "	2	"
" H. O. Bates		6	" "	3	"
" F. Henrotin		9	" "	3	"
" J. S. Mullfinger		2	" "	1	"
" Harles		2	" "	2	"
" J. B. Wheeler		1	" "	0	"
" E. D. Fergurson		1	" "	0	"
" Chas. Dennison		1	" "	1	"
" F. Tipton		1	" "	1	"
" S. H. Dunning		7	" "	5	"
" I. H. Hance		6	" "	1	"
" E. E. Montgomery		15	" "	8	"
" A. E. Hoadley		13	" "	2	"
" J. Tascher		11	" "	4	"
" O'Shea		37	" "	14	"
" A. B. Strong		34	" "	2	"
" F. C. Schaeffer		4	" "	0	"
" C. E. Denhardt		24	" "	10	"
" A. Caillé		17	" "	7	"
" Roswell Park		13	" "	4	"
" F. Van Fleet		22	" "	7	"
" D. C. Cocks		21	" "	6	"
" T. H. Meyers		21	" "	4	"
" G. H. Cocks		14	" "	4	"
" W. K. Simpson		10	" "	0	"
" J. A. Anderson		10	" "	1	"
" E. S. Cocks		10	" "	3	"
" J. J. Reid		10	" "	4	"
" Geo. McNaughton		10	" "	2	"
" J. Eichberg		6	" "	2	"
" W. H. Prescott, 6 " W. B. Bolles, 2 " H. S. Smith, 2		10	" "	2	"
	Carried forward,	875		233	

Digitized by Google

Brought forward,	875	times, with	233	recoveries.	
Dr. H. H. Mudd	6	" "	2	"	
" Carl Beck	5	" "	4	"	
" G. W. Gay	4	" "	0	"	
" J. McManus	5	" "	2	"	
" H. D. Ingraham	3	" "	0	"	
" A. S Hunter	3	" "	0	"	
" N. S. Roberts	3	" "	1	"	
" David Prince	2	" "	2	"	
" W. E. Shaw	2	" "	0	"	
" J. W. Niles	1	" "	0	"	
" J. R. Richardson	47	" "	17	"	
" E. C. Morgan	1	" "	0	"	
" F. Donaldson, Jr	1	" "	0	"	
" Langmann	1	" "	1	"	
" H. F. Ivins	1	" "	0	"	
" A. G. Case	1	" "	1	"	
" S. S. Palmer	1	" "	0	"	
" B. T. Shimwell	1	" "	0	"	
" W. L. Carr	2	" "	0	"	
" F. K. Priest	5	" "	0	"	
" J. E. Winters	6	" "	0	"	
" F. W. Merriam	3	" "	2	"	
" H. W. Berg	8	" "	2	"	
" Forscheimer	5	" "	2	"	
" John A. Robinson	1	" "	0	"	
" C. G. Jennings	4	" "	0	"	
" D. Collins	2	" "	0	"	
" M. P. Hatfield	8	" "	0	"	
	1007		269		

Total number of operators, 69.
Total number of cases, 1007.
Total number of recoveries, 269.
Percentage of recoveries, 26.71.

The ages were recorded in 661 cases, and were as follows:—

There were	31	patients under	1	year of age, with	5	recoveries, or	16.12	per cent.
"	97	"	1	"	15	"	15.46	"
"	149	"	2	"	29	"	19.46	"
"	140	"	3	"	42	"	30.00	"
"	98	"	4	"	32	"	32 65	"
"	56	"	5	"	19	"	33.92	"
"	27	"	6	"	10	"	37.03	"
"	32	"	7	"	16	"	50.00	"
"	12	"	8	"	5	"	41.66	"
"	10	"	9	"	5	"	50.00	"
"	2	"	10	"	1	"	50.00	"
"	5	"	11	"	2	"	40.00	"
"	2	"	14	"	0	"	00.00	"
Total,	661				181		27.38	

Average age, 3 years and 3 months.
There were 277 patients under 3 years of age, with 49 recoveries, or 17.68 per cent.
There were 384 patients over 3 years of age, with 142 recoveries, or 34.37 per cent.

The history of a single patient in this connection may not be uninteresting, as the history of one is that of many. On August 22d, 1887, I was called by Dr. Oscar J. Price, of Chicago, to perform intubation upon Dora B., a little girl of three years, and the following history was obtained: The little patient was brought to the Doctor's office on August 14th, with a mild attack of bronchitis, for which he prescribed, with

Digitized by Google

benefit. A few days later the child was exposed during inclement weather, and became worse. On August 21st the Doctor was called to the house and found diphtheritic membrane covering the tonsils and uvula, a croupy cough, husky voice, and considerable dyspnœa, showing that the larynx was invaded. In spite of thorough and energetic treatment the child became rapidly worse. On August 22d I was summoned in great haste and found the patient moribund. No pulse could be detected at the wrist, the hands were cold, the features livid and the child actually dying. No hope was entertained of recovery, but it was decided to give the little patient at least a chance for life. A tube was quickly introduced and the child soon revived. The respiration became easier and the pulse returned, although very rapid and feeble. As the child revived coughing was induced and considerable ropy mucus and softened membrane were ejected. Cracked ice was given to quench the thirst and semi-solids for nourishment. As the child swallowed very poorly, stimulating enemas were given. The child improved perceptibly, although the pulse continued from 130 to 150 per minute. The temperature ranged from 101° to 102°.

On the third day, as the little patient seemed so very feeble, the tube was removed, but the dyspnœa gradually returned, and within eight hours became so urgent that the tube was again introduced. Although the patient seemed very feeble, she was in a far better condition than when the operation was first performed. The child took but little nourishment, but still a sufficient amount to sustain life. Two days later the tube was again removed, after being in position five days. Respiration was now unimpaired, and the little patient made a gradual but perfect recovery. The interesting features in this case were, first, the low vitality of the child when the operation was performed, and yet the perfect recovery, and, second, notwithstanding that the child took nourishment poorly, more so than is usual in such cases, yet there was no bronchopneumonia developed, such as many attribute to the falling of food and liquids into the lungs.

Intubation of the larynx, to be successfully performed, depends, first, upon judgment in selecting the proper tube.

Second, upon the delicacy and skill with which it is performed.

Third, upon the most careful management of the case.

Fourth, upon faith in the operation.

That intubation has been performed 1000 times within two years, and that 269 lives have been saved from certain death, entitles the operation to a place in the history of medicine.

INTUBATION OF THE LARYNX.—HISTORY.

INTUBATION DU LARYNX.—HISTOIRE,

INTUBATION DES LARYNX.—GESCHICHTE.

BY DILLON BROWN, M. D.,

Attending Physician Out-door Department of Bellevue Hospital.

I. CATHETERIZATION OF THE LARYNX.

Catheterization of the air passages dates its history from the time of Hippocrates. He suggested that in cases of inflammatory cynanche, "cannulas should be carried into the throat along the jaws, so that the air can be drawn into the lungs." This practice had many followers among the ancients until abolished by Asclepiades and Paul, of Egina, who introduced bronchotomy. The remembrance of catheterization was almost

Digitized by Google

lost unt'l Desault revived the operation in 1801. Some time previous to this it was attempted by several, but its execution was found impossible on account of the extreme sensitiveness of the mucous membrane of the larynx and trachea. Chaussier in his memoirs (1780–81) proposed to use the laryngeal tube, which still bears his name, in the asphyxia of new-born children and in cases of dyspnœa from obstruction in the upper air passages. Desault suspected for some time that this extreme sensibility of the mucous membrane to the presence of a foreign body could be blunted by the habitual presence of some smooth object, and the larynx could thus be taught to tolerate a tube for any length of time. He obtained a convincing proof of this by accident. On attempting to pass an elastic tube in the œsophagus of a patient, who had received a transverse wound in the neck, for the purpose of feeding him, the tube slipped, by mistake, into the larynx. There was a sudden cough, and the patient attempted to vomit, but in a little while he was tranquil. However, every attempt to pass fluids through the tube caused a violent, convulsive cough, extreme agitation and anxiety; but in the intervals the patient was tranquil and comfortable. Suspecting the tube to be in the larynx, he brought the flame of a candle to its extremity, and it was immediately caused to vacillate by the air that escaped. Upon withdrawing the tube and putting it into the œsophagus, the patient was fed without difficulty. In the works of Desault, edited by Bichat, reference is made to six cases treated by passing long elastic tubes into the larynx, the upper end of the tube projecting either from the mouth or the nose. The first was a case of *cynanche trachealis*, in which the obstruction was so severe that bronchotomy was considered necessary and urgent. The obstruction was completely relieved by the tube, and, although the patient died the next day, there was no return of the dyspnœa. The second case was treated by a surgeon of Toulouse, and communicated by one of his pupils to Desault. A boy, thirteen years old, with *schirrhus angina* of the parts surrounding the glottis, had a noisy and very difficult respiration. A gum-elastic tube was inserted in the larynx and the upper end allowed to project from the mouth. This gave relief, but some time after the tube became obstructed and required cleaning. He repeated the operation six times, and was satisfied with the result, but was compelled at this juncture to neglect the patient, on account of illness. The next case was one of *cynanche gutturalis*, in which there was much swelling of the tongue and soft parts of the pharynx. On the fifth day all the symptoms were so intense that not only was the patient in imminent danger of suffocation, but he was not able to swallow the smallest quantity of food. An elastic tube was passed into the trachea and the dyspnœa relieved. At first it excited some cough, but this soon ceased. The next day the tube became obstructed, and was removed, cleansed and reinserted. This time its presence excited scarcely any cough. It remained in place a day and a half, when it was no longer needed. The patient recovered. At the hospital of Lyons, a soldier who had attempted suicide received food and air for fifteen days by the aid of two tubes, introduced one into the larynx and the other into the œsophagus. Giraud reports the next case, and also refers to a similar one, but has no notes of it. A man in a fit of madness cut his throat, dividing the trachea about an inch below the cricoid cartilage. The position of the head necessary to bring the edges of the wound together caused the most extreme difficulty in breathing. Therefore, Desault passed a gum-elastic tube through the nose and into the trachea. Its presence at first excited violent coughing, but this did not last long. The respiration became easy, and the edges of the wound could be brought in exact contact without exciting either pain or dyspnœa. He was doing well, but in the evening, during a new paroxysm of madness, he pulled off the dressings, tore open the wound and caused a hemorrhage which killed him.

Finaz of Seyssel, in a thesis presented to the Faculty of Medicine of Paris, in 1813, reports a case of severe laryngeal obstruction, where he urged that a gum-elastic catheter should be passed through the larynx into the trachea. His advice was not followed,

Digitized by Google

and the patient died of asphyxia. The autopsy showed a smooth, non-inflammatory tumor just below the base of the epiglottis, and confirmed his opinion that catheterization of the larynx would have saved the life of the patient. Ducasse, in 1817, and Patissier, in 1820, proposed the same method in œdema of the glottis, but their advice was not accepted. Py (1808), Mackenzie (1825) Emile Gendron (1826), Guimier (1827), Liston (1827), and many others, have carried out the same idea.

Gironard de Sancheville, in a memoir in November, 1827, advocated catheterization of the glottis in croup, and, by this means, the application of caustics to the larynx. In his early attempts he had much difficulty, but after a certain amount of practice and experience he could do the operation with ease and celerity.

There have been reported many cases in which, in attempts to feed the patient by means of a catheter, it has passed into the trachea instead of the œsophagus. Desault's first case was one of this kind, and for others see a thesis by Prus, Paris, 1817, No. III, p. 26, and articles by Thoré and Baillarger in *Annales Medico-psychologique*, t. V and VIII. Dr. John Watson reports a case of stricture of the œsophagus which it was necessary to cut, and to sustain life the patient was fed with a stomach tube. The tube frequently slipped into the larynx—in about one-fourth of the trials—and its position there was easily demonstrated. A similar case has come under my own notice, in which a large quantity of egg-nog was poured into the lungs instead of the stomach. The autopsy showed the bronchi filled with coagulated milk.

Dieffenbach (1839), at the Berlin Charity Hospital, operated upon a case of croup by making a local application of nitrate of silver to the larynx through a curved tube which was passed into the trachea. The left index finger, which acted as a guide to the larynx, was protected by a shield of metal. In the *Journal of the Soc. d. Méd. pract. d. Montpellier*, for 1844, are found three cases of catheterization of the larynx—two by Lallemand and a third by Justin Benoit. In all of them the dyspnœa was relieved, and in one of them—a case of œdema following chronic syphilitic laryngitis—the recovery was complete. In 1845, Depaul, in an important memoir on insufflation in the asphyxia of new-born, by his researches confirmed the results then obtained by Duméril and Magendiè, and demonstrated that insufflation was not difficult, that it never caused a rupture of the pulmonary vesicles, and that it would resuscitate after all other means had failed. He used a modification of Chaussier's laryngeal tube. In the same year, Scoutetten introduced into the larynx of a baby at the point of suffocation a gum-elastic catheter, and practiced insufflation. The relief to the dyspnœa was complete, but the presence of the instrument excited so much irritation—cough and alarming spasm—that he was compelled to remove it. The obstruction immediately returned, and, in spite of its inconvenience, he was forced to reinsert the catheter. However, after repeating this operation a number of times, the presence of the tube in each case causing an alarming amount of irritation, he performed tracheotomy. The child recovered.

In 1852, Lestier, in a treatise on œdema of the larynx, called attention to the use of the laryngeal sound in this disease, and praised it above bronchotomy. He mentioned its use by surgeons to investigate the degree and locality of stenosis of the air passages.

Pia devised a peculiar laryngeal cannula of wood, composed of two parts joined by a leather tube, so that the introduction of air could be regulated at will. Dr. Albert invented a copper cannula, so protected at the end as to push back mucus and prevent the tube from becoming obstructed by it. Leroy, Desgranges and James Curry have also made different modifications of the laryngeal catheter. In 1851, Didot devised a laryngeal tube, which resembled that of Chaussier externally, but was so arranged that local applications could be made through it. About this time the treatment of diseases of the air passages by catheterization and local applications excited widespread interest, and the medical journals of this period are filled with the subject. The most important

Digitized by Google

contributions were those by Horace Green, of New York, and Loiseau, of France, but others came from Reybard (1855), Erichsen (1855), Bowditch (1855), Roux (1856), Bennett (1858), Gros (1858), Dêlfrayssé (1858), Crégny (1858), Garin (1858), Gesenius (1858), Redat (1859), Clark (1859), Collin (1860), Brown-Sequard (1862), Jäsche (1867), Löwenhardt (1867), Inderfurth (1868), Bossi (1868), Depres (1869), and many others.

To Dr. Horace Green, of New York, belongs the credit of having made local applications to the larynx a well-recognized therapeutical measure. His first case, one of follicular laryngitis, was treated in 1838. In 1848, he published a memoir "On the Pathology of Croup, and its Treatment by Topical Medications;" in 1856, an article on the injection of the bronchial tubes and tubercular cavities in the lungs; in 1859, on the treatment of croup by cauterization and catheterization of the larynx; and the next year, an article on the difficulties and advantages of catheterization of the air passages in diseases of the chest. Green's work encountered at the New York Academy of Medicine and elsewhere the greatest opposition; and the discussions on it were extremely bitter, and seemed more like a persecution than any attempt at an honest discussion. The history of the S. S. Whitney case makes a very interesting chapter in the ethical history of the New York medical profession.

Loiseau, imitating Dieffenbach, Depaul, Chartres and others, presented, in 1857, to the Paris Academy of Medicine, a paper entitled, "A simple and easy method of entering the air passages in order to cauterize them or extract false membranes; to dilate the glottis; to introduce substances used in the treatment of croup, either in the form of liquid or powder; and finally, to take the place of tracheotomy." Loiseau used a long tube, one end of which projected from the mouth. His method of operating was to protect the left index finger with a metal ring, which also acted as a mouth gag, to depress the tongue, seize the epiglottis and raise it, and pass the tip of the finger between the aryteno-epiglottidean folds. Then, using this finger as a guide, it is easy to pass the tube in the larynx and make a local application through it. The committee appointed by the Academy of Medicine (consisting of Trousseau and Blanche) to consider Loiseau's paper, returned a highly favorable report, which was unanimously adopted by the Academy. "This operation," says Trousseau, "is considered a very good means of taking the place of tracheotomy, and, at all events, should be tried before practicing that operation." Gros reported a successful case, and Collin, in his memoir, has collected twelve cases, seven of which recovered. In spite of this, he concluded that catheterization of the larynx is an inferior operation to tracheotomy.

Weinlechner, in 1870, devised a long tube to be used in cases of laryngeal stenosis, either acute or chronic. He reports forty-six cases of croup treated by Loiseau's method, with seventeen recoveries, and speaks very favorably of the operation. He condemns the use of the short tube suggested by Bouchut. Prior to this catheterization of the larynx was employed for the stenosis of croup by Monro and by Portal. Von Hüttenbrenner, in 1874, discusses this operation, and in 1881 a successful case is reported by Paton, and another by Sanctuary.

In the treatment of chronic stenosis appear the names of Czermak, Busch, Semelder, Gerhardt, Trendelenburg, Stoerk, Schroetter, McSherry, Bergmann, Sceparowicz, Nawratil, Richet, Gurlt, Hack and others. In most of these cases the operator passed the dilators through the tracheal opening upward into the larynx. Schroetter proposed dilating the larynx through the mouth by using hollow, three-cornered, vulcanized bougies. This method was previously suggested by Trendelenburg, who practiced it in one case. Hack reports a case of œdema of the glottis treated by the Schroetter tubes, in which the patient, when on the way to the hospital, was seized with severe dyspnœa and successfully introduced the tube into his own larynx. Ranchfuss reported two cases in 1875, and among many other names we find Caird (1876), Labus (1876), Alexich (1878), Ribemont (1878), Polk (1880), Chiari (1884), etc.

Digitized by Google

The most important recent communications upon this subject are those of Macewen in 1880 and of Hering in 1882. Macewen's first and last case was done to prevent blood from entering the larynx when operating upon the mouth. His other cases, both successful, were for œdema of the glottis—one due to the burn of a hot potato, and the other following a chronic laryngeal affection. Monti, of Vienna, has resorted to this operation for years, and a few months ago Landgraf reported a case before the Medical Society of Charity Hospital at Berlin, in which there was stenosis of the left bronchus due to the pressure of an aneurism of the aorta. The catheterization of the bronchus was done a number of times, and gave so much relief that at the end of a month the patient was well enough to leave the hospital. Unfortunately, the cause of the stricture was not recognized, and in a few weeks he returned, having had a severe asphyxial seizure, and shortly after died.

In 1858 Bouchut published his memoir, and with this exception, every attempt to relieve laryngeal stenosis by the use of a tube passed through the mouth into the larynx was made with a catheter or long tube, one end of which rested in the trachea and the other projected either from the mouth or the nose. This method has so many almost insurmountable objections, that in spite of the frequent attempts it has made to supplant tracheotomy, it has always been an unsatisfactory operation.

II. TUBAGE OF THE GLOTTIS.

The whole history of tubage is represented by Bouchut's memoir and the report upon it by the Paris Academy of Medicine. In September, 1858, Bouchut submitted to the Paris Academy of Science a paper entitled "A New Method in the Treatment of Croup by Tubage of the Larynx." His tube was a silver, truncated, hollow cone, similar to a common thimble, from one and a half to two centimetres long. At its upper extremity were two collars, six millimetres apart, between which the vocal cords were supposed to rest, and it was pierced at the top by a hole for the passage of a string, to which it was fastened, not only to permit its withdrawal, but to prevent its slipping through the larynx into the trachea. The tube was fitted to the end of a curved sound and introduced by this means. The left index finger was used as a guide to the larynx and was protected by a metal ring, which also acted as a mouth gag. Bouchut operated in all upon seven cases of croup; five died and two recovered after having been tracheotomized. Upon these cases he sought to establish—

1. Tubage is easily accomplished by means of a cannula held in position by the inferior vocal cords, and it does not interfere with the functions of the epiglottis.

2. The larynx is tolerant of the presence of this cannula.

3. The possibility of relieving the asphyxia of croup and other laryngeal affections without tracheotomy.

4. The facility with which large pieces of pseudo-membrane can be expelled through the tube.

5. The advantages of this new procedure to the country physician far from assistance.

Trousseau, who was chairman of the committee appointed to report on his paper, condemned the operation without mercy, as both unphysiological and impractical. Möller, of Koenigsberg, in 1861, made several unsuccessful attempts to use Bouchut's tube, and with this exception the operation has never been heard from since this report of the Paris Academy of Medicine, thirty years ago, until Dr. Joseph O'Dwyer, of New York, solved the problem.

Digitized by Google

BIBLIOGRAPHY.

CATHETERIZATION OF THE LARYNX.

Allexich (G.). Croup laringeo guarito col tubage della laringe. *Gazz. Med. ital.*, pror. renete, Padora, 1878, XXI, 57–60. 1 pl.

Bergmann. Ein Fall von Laryngitis hypoglottica hypertrophica behandelt mit Tubage. *St. Petersb. Med. Wochensch.*, 1883, VIII, 145–147.

Bossi. Ueber die Katheterisation der Luftröhre zur Wiederbelebung scheintodter Neugeborner. Sitzungsb. d. Ver. d. Aerzte in Steiermark. Graz, 1867–68, V, 46–53. Also: *Wiener Med. Presse*, 1868, IX, 869–871.

Brown-Séquard. Possibilité d'introduire un tube dans le larynx sans produire de douleur ou une réaction quelconque. Compte. rend., *Acad. d. Sc.*, Paris, 1882, XCV, 553–555. Also: *France Méd.*, Paris, 1882, II, 461.

Chiari (O.). Tubage des Kehlkopfes. 8°. Berlin, 1885. E. Grosser. 20 p.

Chiari (O.). Tubage des Kehlkopfes. *Deutsche Med. Ztg.*, Berlin, 1884, II, 547, 559, 571.

Collin (P. V.). Du cathétérisme laryngien. 4°. Paris, 1860. *Paris Thèses*, 1860, No. 7.

[Case.] Observation d'angine diphthéritique et de croup chez un enfant; cathétérisme laryngien; guérison; quelques remarques sur la valeur et les indications thérapeutiques du cathétérisme laryngien. *Bull. gén. de thérap., etc.*, Paris, 1860, LVIII, 175.

Créquy. Observation de croup; tubage de la glotte; saignée; analyse du sang; tracheotomie; anasarque; èpauchement pleuritique. *Union méd.*, Paris, 1858, XII, 525.

Delfrayssé. Tubage du larynx fait avec succès dans un case d'asphyxie chez un nouveau-né. *Gaz. d. hôp.*, Paris, 1858, XXXI, 543.

" Deux nouveaux cas de croup guéris par le cathétérisme et la cautérisation du larynx (méthode de M. Loiseau). *Gaz. d. hôp.*, Paris, 1859, XXXII, 105.

Garin (J.). Le tubage du larynx et la tracheotomie, comme methode de traitement du croup. *Gaz. méd. de Lyon*, 1858, X, 469, 501.

Gesenius. Der Katheterismus des Larynx und sein Verhältniss zur Tracheotomie; nach den neueren Verhandlungen besprochen. *Schmidt's Jahrb.*, Leipzig, 1859, CII, 235–244.

Glück (J.). Another mode of passing catheters into the trachea. *Am. Med. Monthly*, New York, 1855, IV, 184.

Green (H.). Croup; its treatment by cauterization and catherization of the larynx. *Am. Med. Monthly*, New York, 1859, XI, 81–95. Also: Reprint.

" On the difficulties and advantages of catheterism of the air passages in diseases of the chest. *Am. Med. Month.*, N. Y., 1860, XIII, 81–97. Also: Reprint.

Gros (L.). Observation de croup traité avec succés par le cathétérisme du larynx. *Bull. gén. d. thérap., etc.*, Paris, 1858, LV, 219–222. Also: *Union Méd.*, Paris, 1858, XII, 434.

Inderfurth (H.). Ueber Katheterisation der Luftröhre bei Neugeborenen. 8°. Berlin, 1868.

Jäsche (G.). Ein erfolgreicher Katheterismus der Luftröhre. *St. Petersb. Med. Zeitsch.*, 1867, XII, 39.

Küster. *Deutsche Med. Zeitung*, Berlin, 1886, VII, 876.

Landgraf. Ueber Katheterismus der grossen Luftwege. *Berliner Klinische Wochenschr.*, 1887, XXIV, 85.

Löwenhardt (S. E.). Ueber einen Apparat zur Katheterisation der Luftröhre und Luftinhalation bei asphyktisch Neugeborenen. *Monatsch. f. Geburtsk. u. Frauenkr.*, Berlin, 1867, XXX, 265–292.

Macewen (W.). Croup; tubage du larynx; guérison; oedème de la glotte; introduction des tubes trachéaux par la bouche en place de la trachéotomie. *Paris Méd.*, 1880, 2. S., V, 585.

Möller. Ueber Bouchut's Tubage beim Croup. Amtl. Ber. ü. d. Versamml. deutsch. Naturf. u. Aerzte, 1860. Königsberg, 1861, XXXV, 167–173.

Orth (K.). Zwei Fälle von Kehlkopfstenose. *Deutsche Med. Wochenschrift*, 1886, No. 29.

Paton (J. W.). Case of croup treated by passing catheters into the trachea by the mouth. *Brit. M. J.*, Lond., 1881, I, 803. Also: *Am. Med. Bi-Weekly*, N. Y., 1881, XIII, 13. Also: *Brit. M. J.*, 1881, I, 1001; 1881, II, 690.

Pollard (Bilton). Three cases illustrating the difficulties in establishing natural respiration after

Digitized by Google

tracheotomy, and their treatment by tracheal catheterization. Clinical Soc. of London. *London Lancet* (Am. Rpt.), June, 1887, p. 454.

Prus. *Thèses Paris*, 1817, No. III, p. 26.

Rauchfuss. Ueber zwei Fälle von Katheterismus des Larynx. *St. Petersburger Med. Zeitsch.*, 1875-76, n. F., v, 64.

Reese (D. M.). Dr. Horace Green and catheterization of the bronchi. *Am. Med. Gaz.*, N. Y., 1860, XI, 241–249.

Sanctuary (T.). Tracheal catheterism in Croup. *Brit. Med. Jour.*, London, 1881, t. 1001.

Sprague (S. L.). Catheterization of the lung. [*From:* Bos. Soc. Med. Observation.] *Boston M. & S. J.*, 1855, LIII, 210–212.

Tubage (Le) du larynx et la trachéotomie. *Gaz. d. hôp.*, Paris, 1859, XXXII, 33.

Von Hüttenbrenner (And.). Ueber den Katheterismus des Larynx bei der croupösen oder diphtheritischen Erkrankung desselben. *Jahrb. f. Kinderk.*, Leipzig, 1874–75, VIII, 89–97.

INTUBATION OF THE LARYNX.

Pacific Med. & Surg. Jour., San Francisco, 1886, XXIX, 65; 1887, XXX, 129; 1887, XXX, 395.

Med. Record, New York, 1885, XXVII, 206; 1886, XXIX, 214, 410, 474, 641; 1886, XXX, 280, 487, 645, 665, 683; 1887, XXXI, 26, 70, 73, 108, 109, 161, 192, 324, 416, 608, 646, 677, 685, 686, 687, 705, 707; 1887, XXXII, 99, 137.

Paris Méd., 1887, XII, 97, 17; 1886, XI, 541.

Memorabilien, Heilbronn, 1886, N. F., VI, 518,

Medical Review, Pittsburg, 1887, I, 114.

American Pract. & News, Louisville, 1886, III, 321; 1886, II, 377.

Berliner Klinische Wochenschrift, Aug. 8, 1887.

Journ. American Med. Assoc., Chicago, 1887, VIII, 9, 199, 342; 1886, VII, 35, 76; 556, 1887, VIII, 291, 135; 1886, VI, 186, 147; July 2, 1887; 1886, VI, 426; 1887, VIII, 337, 358, 701.

Archives Pediatrics, Phila., 1886, III, 215; 1885, II, 657; 1887, IV, 154.

Medical Journal, New York, 1887, XLV, 238; Nov. 28, 1885; 1885, XLII, 145; July 23, 1887, p. 95; 1887, XLV, 273, 624; 1886, XLIV, 322, 373; 1886, XLIII, 384.

Medical Journ. & Examiner, Chicago, 1885, L, 475; 1885, LI, 511; 1886, LIII, 132; 1886, LII, 351, 353, 193; 1885, LI, 401; 1886, LII, 214; 1886, LIII, 370; 1887, LIV, 300, 407.

Medical Age, Detroit, 1886, IV, 313; 1887, V, 15.

Mississippi Valley Med. Monthly, Memphis, 1886, VI, 345.

Cincinnati Lancet Clinic, 1887, LVII, 54, 97, 208; 1887, N. S. XVIII, 321; 1886, N. S. XVII, 606.

Med. & Surg. Rep., Philadelphia, 1886, LV, 362, 393, 586; 1887, LVI, 621, 525.

Western Med. Reporter, July, 1887, p. 153.

Albany Medical Annals, 1887, VIII, 41.

Medical Times, Chicago, 1887, XIX, 53.

American Lancet, Detroit, 1887, N. S. XI, 92; 1886, N. S. X, 401.

Med. Presse, New York, 1887, IV, 17; Dec. 1886, p. 40; 1887, III, 259.

American Jour. Med. Science, Philadelphia, 1886, N. S. No. 184, p. 409.

Medical Register, Philadelphia, 1887, I, 174, 42, 122, 146.

Wiener Med. Wochenschrift, 1887, XXXVII, 2, 35, 58.

Medical Times, New York, 1886, XIV, 55; 1887, XV, 53.

Wiener Med. Presse, No. 12, 1887.

Indian Medical Gazette, Calcutta, 1886, XXI, 334.

La Independencia Médica, Barcelona, 1887, XXII, 365.

Southern Medical Record, Atlanta, 1887, XVII, 325.

Allgemeine Med. Central-Zeitung, May 26, 1886.

Le Progrès Méd., June 18, 1887, p. 499.

Gaillard's Med. Journal, New York, 1887, XLV, 101.

Buffalo Med. & Surg. Journal, 1885, XXV, 553; 1886, XXVI, 226, 116.

Kansas City Med. Index, 1887, VIII, 148.

Virginia Med. Monthly, Richmond, 1886, XIII, 648.

Concours Médicale, Feb. 26, 1887.

Digitized by Google

Rev. Gen. d. Clin. et d. Therapeut., 1887, I, 216.
Hahnemannian Monthly, Philadelphia, June, 1886.
Medical Herald, Louisville, 1887, VIII, 583.
Gazz. Med. d. Roma, Aug. 1, 1887.
L'Union Médicale, Paris, 1887, XL, 1049.
Maryland Med. Journ., Baltimore, 1886, XVI, 147, 67, 168, 391.
Boston Med. & Surg. Journ., 1887, CXVI, 363; 1886, CXV, 598.
Journal Laryngology & Rhinology, London, 1887, I, 1.
La Riforma Medica, Napoli, 1887, No. 159, 160, 161 and 162.
Therapeutic Gazette, Philadelphia, 1886.
Medical Monthly, New York, 1886, I, 9.
Pacific Record of Med. & Pharm., San Francisco, 1886, I, 68.
Lancet, London, Dec. 19, 1885, p. 1159.
Giornale Internazionale d. scienz. med., 1886, VIII, 996.
Weekly Medical Review, St. Louis, 1886, XIV, 171; 1887, XV, 57, 306.
Eastern Med. Journ., Worcester, 1887, VII, 30.
Provincial Med. Journ., 1887, VI, 225.
Deutsche Med. Zeitung, No. 38, 1887.
South California Pract., 1886, I, 344.
Medical News, Philadelphia, 1887, L, 341.
St. Louis Courier Med., 1886, XVI, 470.
Obstetric Gaz., Cincinnati, 1887, X, 124.
St. Louis Med. & Surg. Journ., 1887, LII, 115.
Canadian Pract., Toronto, 1887, XII, 1.
Arch. Méd. Belges, Bruxelles, 1887, XXXI, 183.
Reference Handbook of the Medical Sciences. Ed. by A. H. Buck, M. D. Vol. IV, p. 419.

DISCUSSION.

Dr. PITNER (Jacksonville, Ill.) questioned the correctness of the closing statement of Dr. Waxham, that the 249 cases of recovery following intubation were so many lives saved by the operation. This assumes that all cases of stenosis from croup or diphtheria are, without operative measures, necessarily fatal, and he had observed some of these cases recover under treatment without operation. He did not, however, question the utility and great importance of this operation, which appeared to him to be preferable to tracheotomy in a large majority of the cases where an operation is indicated.

Dr. C. G. JENNINGS (Detroit, Mich.):—I hesitate to speak of my small number of intubations, and I should not do so were it not for the fact that my experience with tracheotomy and intubation has been directly opposite to that of the gentleman who has just read his paper. Too much cannot be said in praise of the efforts of Dr. O'Dwyer and Dr. Waxham. They have given to the profession and to the people a method in the treatment of croup which will save hundreds of cases.

My own experience up to the present time includes 36 cases of tracheotomy, with 17 recoveries, and 12 cases of intubation, with no recoveries. Dr. Waxham has given us the statistics of about 1000 cases of intubation, showing about 26 per cent. of recoveries. If compared with the general statistics of tracheotomy, this, I think, is a better record than the latter operation can give. It is to be noted, however, in these statistics that certain operators in both operations have records far above the average, and I believe the successful tracheotomists have a larger proportion of recoveries than the physicians performing intubation who rank as the most successful. This would indicate that tracheotomy performed by the best operators will give better results than intubation performed by operators of equal dexterity.

Digitized by Google

Dr. Waxham has been unusually successful with intubation, while operators in the same city, and operating under the same circumstances, have been markedly unfortunate. I am firm in the belief that the personal equation is an important element in the consideration of the results of both tracheotomy and intubation. Dr. Waxham has caught the spirit of the operation, and he sees indications for little details of treatment which escape the ordinary observer, and which, although apparently insignificant, have the greatest influence over the result. Death threatens from so many quarters after either operation, that to be successful the most painstaking care is necessary.

I must grant all the great advantages mentioned by the writers which intubation possesses over tracheotomy. There are some features of superiority in the latter operation, however, which should not be lost sight of. The accessibility of the trachea after tracheotomy, for the removal of membranes and desiccated mucus, and for the direct local treatment of the diseased mucous membrane, offers an advantage which can never be approached in intubation. Through a tracheal tube medicated steam may be almost continuously poured into the trachea. This, with the addition of medicated instillations and the insufflation of powders, has a great influence in the production of a favorable result.

The nutrition of patients with croup who are much exhausted by diphtheritic toxæmia is of prime importance. After intubation, proper nutrition is almost impossible; after tracheotomy, there is no such difficulty.

In regard to the comparative difficulty of performing the operation, I would say that to an inexperienced operator intubation may prove fully as difficult as tracheotomy. When one has acquired the necessary dexterity, intubation is a very simple matter, while tracheotomy, even to the most expert operator, must always remain a difficult procedure. Physicians may become expert in intubation who never would attempt a tracheotomy, and herein lies one of the greatest advantages of intubation. It will be practiced by many more physicians, and it will bring surgical relief to hundreds of cases of croup which would otherwise be left to die. While I do not pretend to say that this should be the general rule in determining between the two operations, I shall in the future, in the light of my past experience, urge tracheotomy for all cases offering reasonable chance of success, reserving intubation for the cases in which operative interference will give nothing more than euthanasia and those patients whose parents refuse to consent to tracheotomy.

Dr. Northrup (New York):—It seems to me the gentlemen have left a depressing feeling upon us. They have set up so many danger signals that I fear, if I never had begun on this operation, I should not have the courage to begin. They set up the dangers of accident so conspicuously that I fear an operator who has had one or two fatal cases would be wholly discouraged. I would like to say, that in an experience of thirty-three cases I have never had any difficulty in inserting the tube and but little in extracting it. I never have had any accident of any kind. I have no doubt but many gentlemen here present could aggregate several hundred cases in which there has been no accident.

There is only one drawback to the success of the tube, in my mind. This is in the interference with nutrition. It is many times difficult to keep up the full nutrition of the patient when swallowing is so interfered with.

In reply to the question of the President, I would say the operation has never failed, in my experience, to fully and promptly relieve the dyspnœa. I once operated on a case where the physician had gone home worn out. He had told the family the child must die; he could do no more He would not do tracheotomy; he did

Digitized by Google

not know of intubation. A little later the case fell into my hands. I found the child exhausted and comatose. After putting in the tube he aroused, and ultimately recovered. At the end of forty hours he coughed out the tube, and it was not reinserted. A child in the same family became croupy, and also recovered after intubation. I believe it fills all the functions that tracheotomy can.

Dr. WAXHAM (Chicago, Ill.):—In answer to Dr. Jennings, I would simply say that he has compared personal results with the total results of intubation, which certainly is not fair. Our brilliant and successful tracheotomists are few and far between. We should compare their results with the individual results of the most successful operators with intubation. If they would combine their results with those of others all over the country who have been less successful, and compare the total results with the total results of intubation, they will come nearer the truth of the matter.

In answer to the question raised in regard to statistics, I would say that a year ago I collected 306 cases of tracheotomy, with 58 recoveries, or 18.95 per cent. A few years since Prof. Jacobi collected 1024 cases of tracheotomy, performed in various parts of the world, with 220 recoveries or 21.42 per cent., in contrast to 26.71 per cent. saved by intubation.

Dr. O'DWYER, in closing the discussion, referred to the question raised by one gentleman, that intubation being a simple operation that meets with little opposition will, therefore, be resorted to in a certain number of cases that would recover without interference. While this may be true to some extent, it should also be remembered that for the same reason it will be practiced in the most hopeless cases as a means of euthanasia where tracheotomy would not be seriously considered.

In a large percentage of my hospital cases there was extensive catarrhal pneumonia following measles, with the addition of diphtheria and nephritis at the time they were intubated, and of course nothing was gained except relief to the suffering, and nothing more was expected. I would certainly never have proposed tracheotomy in any of these cases, and yet they would count in the statistics of intubation as much as the most favorable ones. No doubt, also, many will follow the plan suggested and practiced by Dr. Jennings, viz., do tracheotomy on all the most favorable, and reserve intubation for the unfavorable cases and where consent to do the cutting operation cannot be obtained.

INFANTILE CONVULSIONS IN CONNECTION WITH LATER NEUROTIC TENDENCIES.

CONVULSIONS ENFANTINES PAR RAPPORT À DES TENDANCES NEVROTIQUES.

KINDERKRÄMPFE IN VERBINDUNG MIT SPÄTEREN NEUROTISCHEN NEIGUNGEN.

BY J. A. COUTTS, M. B. CANTAB., M. R. C. P.,

Assistant Physician to the East London Hospital for Children.

The difficulties attached to the pathological conditions which give rise to the symptom convulsions have been frequently and ably dealt with by Dr. Hughlings Jackson and other writers. In adults this symptom, by itself, has been shown to afford no reliable indications for forming conclusions as to the brain lesions which produce or permit of it. The outward muscular manifestations may be the same whether the

Digitized by Google

exciting cause be cerebral hemorrhage, uræmia, the yet unknown pathological brain states associated with idiopathic epilepsy, and numerous others. Although some assistance in forming a diagnosis may, in many cases, be afforded by a good and trustworthy description of the convulsive attack, yet, to arrive at a justifiable one, the past and family histories, the present condition of nearly all the organs of the body, and all concomitant circumstances, must be taken into account. A diagnosis having been arrived at, in most cases, a prognosis may fairly be given. Thus, a patient with diseased arteries and cerebral hemorrhage will be liable to other attacks if he recovers from a present one; an epileptic is not likely to pass through life and be free from further fits, and so on. The difficulty, then, in adults, in dealing with the symptom convulsions, lies mainly in the diagnosis of the associated pathological brain conditions; prognosis in many cases, if not in most, being fairly easy and justifiable. In marked contrast to this, in infants under two years of age the difficulty is chiefly concerned with the prognosis rather than with the diagnosis of the pathological conditions accompanying the convulsions. Infants can be but rarely the subjects of cerebral hemorrhage, uræmia and all the varied lesions which embarrass the question in adults. In them, as in adults the subjects of idiopathic epilepsy, we are dealing with a pathological condition which seldom reveals itself to any known method of research, microscopical or otherwise. Post-mortem examination, in the vast majority of cases, affords no aid; all the changes found in the brain, that were formerly accepted as causes, are now believed to be rather the consequences of convulsions. All that we know with regard to the brain conditions accompanying most cases of convulsions in infants and epilepsy in adults is that, in both, we are concerned with enfeebled cerebral matter, whether this be brought about by occlusion of a small vessel, as a cortical vein, or due to the influence of a general disease, as pneumonia or an impending exanthem. Beyond this our knowledge, or rather ignorance, of the underlying brain condition does not extend. If for no other reason, this want of knowledge should make us cautious in predicting as to the future of a child who has suffered from a convulsive attack. Moreover, in many children the convulsive attacks continue on into adult age without remission for any length of time, and the conclusion gets forced upon one that they have been epileptiform from the very outset. Dr. Hughlings Jackson has lately, I believe, expressed the opinion that lower centres are implicated in the convulsions of infancy than in the epileptic attacks of adults, owing to the non-development of these higher centres at the infantile age. However this may be, I would contend that the sufferers from explosive nerve discharges in lower centres would probably be liable to similar discharges in higher centres at a later period of development. There is nothing in the outward muscular manifestations to distinguish between the convulsive attacks that continue on into later life as epilepsy and those that cease with the age of infancy. Under the most varied circumstances of disease the convulsive paroxysms may be perfectly similar. But little aid is gained from the outward symptoms as to the cerebral causes on which they depend. The fits of "so-called" eclampsia coincide in time and sequence with all the phenomena of typical epilepsy in an adult. All the tests that have been given as means of differentiating between the convulsions that are likely to be followed by epilepsy in later life and those of mere eclampsia have been found fallacious by experience. How, then, can any prognosis as to future fits or neuroses be held justifiable under these circumstances?

If we turn from the consideration of the mere paroxysm and its surroundings to the family history of convulsive infants, grave matter is offered for our consideration. In 100 cases of convulsions in infants, taken consecutively from my note books, in 29 there was a history of neuroses in the parents or near relatives. If we might include as neuroses convulsions in brothers and sisters, the family neurotic percentage would rise to as high as 63. All are agreed as to the importance of the family history in dealing

Digitized by Google

with neuroses and of the frequency of the transmission of the neurotic taint from parents to their offspring. One might fairly expect, then, that a large percentage of the children of such parents as are dealt with in my cases would turn out neurotic in later life, even apart from the consideration of their having had convulsions in infancy. These questions as to our ignorance of the brain conditions originating convulsions, the similitude of the paroxysms to those in adult epilepsy, and the family history, have been too much ignored by most writers on the subject. So much have convulsions come to be regarded as normal phenomena during the age of infancy that, apart from the typical seizures, only too many medical men apply the term loosely to various maladies where there is no reason to suspect their presence. Thus have been begot the terms screaming convulsions and that of internal convulsions as applying to the numerous and vague symptoms developed, under illness, by a nervous system during process of development. Mothers even make a subtle distinction between convulsions and fits. With none of these uncertain and unsatisfactory symptoms do I propose to deal. The question, also, as to the association of rickets with convulsions, which will be gainsayed by few nowadays, is foreign to my present paper. Elsewhere (*British Medical Journal*, June 4th, 1887) I have endeavored to show that the cerebral mal-nutrition in rickets, which may give rise to convulsions, is the cause of frequent neuroses in later life.

Much that is misleading as to the immediate gravity of convulsions is, I believe, to be found in the writings of those reported to be authorities on the subject. Dr. West states that the deaths due to diseases of the nervous system are 30.5 per cent. under the age of one year and 24.3 per cent. between the ages of one and five years, and that convulsions are responsible for these deaths in the proportion of 78.3 and 54.3 per cent. respectively. This would mean that nearly 23 per cent. of children under one year and 13 per cent. of children between the ages of one and five years die from convulsions pure and simple. Without taking into account the other circumstances associated with the convulsions, this, in my experience, would seem grossly fallacious and lending a false importance to the immediate consequences of the symptom convulsions. One knows how frequently an attack of convulsions is the closing scene in the short drama of the life history of an infant, but it is, as Dr. Hughlings Jackson expresses it, "only a mode of dying, and one incidental to the time of life," and replaces the gradual asphyxia or cardial failure in adults. When a child exhausted with diarrhœa or other disorder dies during an attack of convulsions, one is no more justified in ascribing the death to the latter than one would be in so doing when it was the termination of uræmic poisoning or tubercular meningitis. It is a non-discrimination between these cases of convulsions as "a mode of dying" and others that has led many to ascribe a false immediate gravity to the symptom itself. From this belief in the frequency and immediate danger of convulsions has, I fancy, sprung the common practice of ascribing the death of many, if not most, infants found dead to this cause.

Just as the immediate dangers of convulsions pure and simple have been greatly overrated, so, I believe, the gravity as regards future neuroses has been vastly underestimated by the general acceptance of the aphorism that convulsions in childhood are the equivalent of rigors in adults. It is true that in many cases of disease convulsions in children take the place, in time, of rigors in adults; but it is no fair inference from this fact that the one is of no more importance as regards the future than the other. That convulsions in a child with an unstable, nervous system should take place at the onset of severe disease is no more than one would expect. But I greatly doubt whether their occurrence is as frequent as is commonly supposed, or whether the percentage of cases in which they do occur is greater than that of children with a neurotic tendency to others with stable nervous systems. To illustrate the connection with, or difference between, rigors in adult age and convulsions in childhood, no better example can be taken than pneumonia.

Digitized by Google

In adults, every one is agreed as to the extreme frequency of rigors at its commencement. In children, however, in my practice, although pneumonia has been common enough, cases complicated with convulsions at onset have been somewhat of a rarity. Fearing that my experience might, perhaps, have been somewhat at variance with that of others, I asked Dr. Eustace Smith, a great authority, about what percentage of cases of pneumonia in young children he found associated with convulsions at onset. He replied that he had never made any actual statistics on the point, but that, as far as he could judge generally, at least ten per cent. had convulsions. To test the matter, he kindly placed several of his note books of in-patients in the East London Children's Hospital at my disposal. From these I gathered statistics of 99 cases of pneumonia, but rejecting 14 cases in children above the age of seven years, the number is reduced to 85. Of these 85 cases 44 were diagnosed as broncho-pneumonia, the remaining 41 as croupous pneumonia, pneumonia or pleuro-pneumonia. Of these 41 cases of pneumonia, two had convulsions at the onset of the disease, or, as nearly as possible, five per cent. of the total. But there are significant facts about both the convulsive cases; one, aged 21 months, was the child of a father who was epileptic, and its maternal aunt was insane; the other, aged four years, had had frequent one-sided fits, from the age of sixteen months. As bearing on the question of convulsions with pneumonia, it is interesting to note that three children, aged three years, four years, and 21 months, respectively, had had frequent fits previously, but passed through attacks of pneumonia without convulsions, although one parent in two of the cases was an epileptic. Again, three other children, that had had fits before, passed through severe attacks of broncho-pneumonia without a seizure. Deaths were frequent, and convulsions figure prominently at the end, but, as I said before, can hardly be held as responsible for the result. The number of cases is small, but I fancy, had it been greater, the percentage of cases with convulsions at onset would probably not have been materially altered. An attack of pneumonia commencing with cerebral symptoms, such as convulsions, makes a much greater impression on the memory than many cases without such accompaniment, and has led, I fancy, to an exaggerated notion of their frequency. Were convulsions at the onset of pneumonia in children as common as many think, a possible reason for their frequent association might be found in the suggestion that neurotic children are possibly more prone than others to pneumonia. To me, I confess, the mere fact of the frequency of convulsions at the onset of pneumonia, were it as great as commonly believed, would have little weight as determining the importance of the symptom as regards the future of an infant. Careful inquiry as to the presence or absence of prior convulsions and the family history as regards neuroses are of far greater importance in my mind. If such inquiry were made in all cases of pneumonia with convulsions, I have a shrewd impression that much greater importance will come to be attached to the symptom than if it were merely the age's equivalent of rigor in an adult.

Of the frequency of convulsions at the onset of exanthems, personally I have had but little opportunity of judging, such cases but seldom coming under the notice of those engaged at children's hospitals. From inquiries among friends with larger experience, I should judge that such cases were somewhat of a rarity. Just as in pneumonia, I fancy their occurrence makes a somewhat lasting impression on the memory, and has led to a belief of their being more frequent than statistics would bear out. Two friends had between them about 900 cases of exanthems under their care, and out of all this number but three children had convulsions at the outset, one child at the onset of what turned out to be diphtheria, and the two others with the beginning of scarlet fever. The family histories, and previous histories of the children as to convulsions, they could not furnish me with. I have heard the question propounded to a class of twenty qualified men, most of them in general practice, whether any one of them had ever seen a case of measles commencing with convulsions, and

Digitized by Google

not one of them could recollect ever having done so. Taking these circumstances into account, I cannot but consider convulsions at the commencement of exanthems an event of somewhat rare occurrence. Of course, convulsions are frequent enough in the later stages of measles, and also in scarlet fever without uræmia. But there are distinct and palpable pathological contingencies accountable for the symptom in the later stages of both, and convulsions in these later stages must be clearly differentiated from those occurring at the same period as rigors do in adults. It is matter of general acceptance that epilepsy has frequently its starting cause in an attack of scarlet fever, and sometimes, but less often, in one of measles.

From what has gone before, it will readily be surmised that I am inclined to attach a greater importance to the brain conditions which permit of such a symptom as convulsions than is the general custom. I cannot think that a nervous system can be so thrown from its balance and disturbed by the mere physiological process of a tooth working its way through a gum, or by the presence of undigested food in the stomach, or worms in the intestine. Is not such a condition of nervous system that has once been upset by causes, so slight, likely to remain a permanent source of weakness, and liable to reveal its shortcomings at times of greatest stress thrown upon it or the general system? It is true that many children suffering from convulsions recover with the removal of the exciting cause, such as dental irritation or undigested food, and remain to all appearances free for a time from neuroses of any kind. But I confess that I should look with grave suspicion on the future of such a child, however healthy it might seem, and should dread a call more than ordinary upon its nervous system. But all, unhappily, do not get well with the removal of the exciting cause. Epilepsy in later life is often continuous with convulsions in infancy or separated from it by an interval of a few years. Dr. Gowers has shown that ten per cent. of adult epilepsies have a causal influence in the same brain conditions that led to convulsions in infancy. Where the convulsions of infancy are directly continuous into the epilepsy of later years, no one, I take it, doubts the similarity of the pathological brain conditions giving origin to both. To few, however, it occurs to associate the cerebral state giving rise to epilepsy at such ages as second dentition or puberty with that originating convulsions in infancy. If this be the case in diseases presenting such similarity in their outward manifestations, the association of such diseases as chorea or migraine in later life with convulsions in infancy must, to such practitioners, seem to verge on the fantastical and absurd. For some time past, nevertheless, I have made inquiries on these points, and have satisfied my own mind that a fairly large percentage of sufferers from chorea and migraine, as well as epileptics, had been the subjects of infantile convulsions, a confirmation to my own mind of the importance to be laid on the latter symptom. Such inquiries are easily made, and the truth of the frequency of such associations readily verifiable. Another problem, what percentage of children that have suffered from infantile convulsions pass through life without manifest neuroses in later years, is much more difficult of solution. We have no statistics bearing on the question. But few men have the opportunity of following out the life history of many convulsive children into adult age, and but fewer still the time or inclination to collect statistics on such a subject. With a view to a partial clearing up the obscurities surrounding this matter, I have collected notes, with the assistance of medical friends, of sixty-eight cases of persons who have at least reached the age of second dentition, and who had been in infancy the subjects of convulsions. During the process of inquiry, the interesting fact seems to be brought out that a large percentage of children that had had convulsions died before the age of second dentition. This would mean either that such children were more prone to serious disease, or had constitutions less capable of resisting it when incurred. Of those collected, as far as could be learned from relatives' and others' accounts of events that occurred many years before, all had been the subjects of convulsions

Digitized by Google

in its most typical form; cases of those vague and ill-defined symptoms known as internal convulsions being rejected. Careful inquiry was made as to age of first onset, the number and frequency of attacks, the family history, the health of the child at the times, and as to the presence or absence of other disease, such as digestive or pulmonary troubles or exanthems. In these sixty-eight cases there was a history of neuroses in the parents in eighteen, or including cases of convulsions in brothers and sisters, in thirty-nine. The ages of first attack varied from three weeks to two and one-half years. Attacks had been frequent in nineteen cases, few in thirty-two, and in seventeen the attack had been a solitary one. In thirty-nine cases no information could be given as to the state of the child at the times of attacks; in nineteen cases teething was ascribed as the cause; in two, teething and diarrhœa; in four, whooping cough; in two, bronchitis and "congestion of lungs;" in one, bronchitis and inflammation of lung; in one, scarlet fever; and in two, the presence of undigested food in the stomach. No reliable information could be gained in the majority of cases as to whether the convulsions were general or with a tendency to one-sidedness, but in seven there was a fairly reliable history as to one side having been affected more than the other, and of these five turned out neurotic. The ages to which the cases had arrived when the information regarding them was taken, varied from eleven to sixty-three years. Unfortunately, many of them were under the age of puberty. Now, of these sixty-eight cases, thirty-one were the subjects of neuroses in later life. Of these thirty-one cases, eight had epilepsy in its severer form; three, petit mal; four were somnambulists; three had melancholia; four had chorea, and seven were subject to periodical migraine. Of the remaining two, one was somnambulistic in early life and had severe hystero-epilepsy later on, and her only child was mentally defective; the other was also somnambulistic in early life, and at the age of twenty-two had melancholia with suicidal impulses. But of the thirty-seven who had remained as yet free from distinct neuroses, six were described as being eccentric in their ways and conduct and irritable in manner, and nearly all the rest were, in intellect, below brothers and sisters who had been free from convulsions in infancy. There were distinct neuroses in nine of the brothers and sisters who had escaped convulsions themselves, among the thirty-one cases of convulsions with after neuroses, and in fifteen of the thirty-seven cases of convulsions without subsequent neuroses. But besides mere statistical evidence, there is in several of the family histories much that is suggestive; thus, out of a family of nine, the only one with convulsions as an infant became epileptic at the age of fifteen, while fairly up in a large public school, and was finally incarcerated in a lunatic asylum, there being no trace of neuroses in the rest of the family. The only two convulsives out of a family of eight turned out melancholic, the one at seventeen and the other at twenty-two, the rest having no special neurotic tendencies. Many other instances like these could be given from the notes, did space permit, of the only neurotics in families being the ones that had had infantile convulsions.

At the commencement of this paper I endeavored to show, from general grounds, that convulsions in infancy were likely to be followed by neuroses in later life. My statistics would seem to confirm what was deduced from mere theoretical bases. It is true that sixty-eight cases is a small number to argue from, but such cases are only collected together, with difficulty. Many of them, too, are subject to great disadvantage, in that the ages at which the statistics were taken were under that of puberty. Nevertheless, subject as they are to this disadvantage, and small as is their number, my statistics would seem to prove that children who suffer from infantile convulsions are prone to neuroses in later life in a degree out of all proportion incidental to the more fortunate ones who escape them.

Digitized by Google

DE LA NATURE DE LA COQUELUCHE ET DE SON TRAITEMENT ANTISEPTIQUE.

ON THE NATURE OF WHOOPING-COUGH AND ITS ANTISEPTIC TREATMENT.

ÜBER DAS WESEN DES KEUCHHUSTENS UND SEINE ANTISEPTISCHE BEHANDLUNG.

PAR LE DOCTEUR MONCORVO,

Professeur de Clinique des Maladies des Enfants à la Policlinique de Rio de Janeiro; Membre Titulaire de l'Académie de Médecine, etc., etc.

Depuis bien des années et à la suite d'une observation déjà longue, j'avais remarqué qu'il y avait de nouvelles recherches à entreprendre au sujet de plusieurs points de l'histoire de la coqueluche, maladie qui sévit encore aujourd'hui à peu près avec une pareille intensité dans tous les pays civilisés.

Quoique étudiée depuis la première moitié du quinzième siècle combien de controverses dominent encore de nos jours à propos de la nature, du siège, et tout particulièrement à l'égard du traitement, de cette cruelle entité morbide !

En ce qui touche son siège, personne n'ignore guère la variabilité des jugements portés à ce sujet par le grand nombre d'observateurs qui s'en ont occupé.

C'est ainsi, par exemple, que Webster faisait siéger la maladie dans l'encéphale; par contre Sanders, Pidock, Breschet la considéraient sous la dépendance d'une altération du pneumo-gastrique.

Bien avant ceux-ci, d'autres comme Stoll, Browzet, Pinel, Chambon et Valdschmidt ont cru que la coqueluche avait son siège dans la cavité gastrique, tandis que Butter et Huxham la rapportaint à l'intestin.

Padalme avait admis un siège plus complexe qui comprenait le poumon, l'estomac et le diaphragme.

La majorité pourtant des cliniciens ont signalé la coqueluche comme une affection de l'arbre bronchique; telle a été l'opinion de Haënnec, Walt, Vate, James Hamilton, Billard, Alberson, Broussais, etc.

Ces divergences constatées par rapport au siège de la maladie se présentent également en ce qui regarde sa nature.

On irait loin si on voulait rappeller toutes les théories proposées dans ce but.

Un grand nombre de pathologistes n'ont vu dans la coqueluche qu'une simple névrose de l'appareil respiratoire; cette opinion fut soutenue par Frank, Hufeland, Höbel, Albers, Roche, Grisolle, Blache, Schæffer, Barrier, Mattai, Jahn.

D'autres, comme Baron, Guersant, Trousseau, Richart, Tyfe, Mayer, Flint ont admis que cette affection était une bronchite greffée sur une névrose.

D'après d'autres auteurs, la coqueluche serait identique ou analogue aux fièvres éruptives; telle a été du reste la théorie soutenue par Barthez et Billiet, James Ducan, Neuman, Rokitansky et Valtz, Germain Sée, Roger.

Ce coup d'œil jeté sur les théories soulevées au sujet de la nature et du siége de la coqueluche laisse hors de discussion le peu de solidité de ces hypothèses si variables et si pleines d'insertitudes.

Cependant nous avons laissé à dessein d'énumérer parmi les précédentes la théorie d'après laquelle l'affection en question serait localisée à l'entrée de l'arbre bronchique.

Il y a trente-quatre ans, Gendrin, dans un de ses leçons, avait tâché de démontrer que la coqueluche était une phlégmasie des cryptes de la partie supérieure du tube aérien.

Watson, à cette même époque, en Angleterre soutenait que la maladie était engendrée par l'absorption d'un miasme toxique, agissant sur la surface respiratoire, en y entraînant une inflammation spécifique avec irritation des nerfs de la glotte.

Digitized by Google

Ces idées reçurent plus tard la confirmation de la part de Beau, Parrot et Wannebroug.

En 1883 et 1884, nous avons exprimé dans deux travaux sur la nature et le traitement de la coqueluche notre manière d'envisager le siège de l'affection.

Nous étions alors, comme aujourd'hui, parfaitement convaincus de la localisation de cette affection à l'entrée de l'arbre aérien.

Depuis bien des années nous avons dirigé particulièrement notre attention la plus rigoureuse sur l'exploration thoracique chez nos coqueluchеurs, aux diverses périodes de la maladie, et cela nous a permis de constater, sur une large échelle, la présence de coqueluches d'une assez forte intensité tout-à-fait dégagées de la phlégmasie bronchique; nous avons aussi été témoins d'un assez grand nombre de cas dans lesquels elle avait débuté par des crises spasmodiques en l'absence de toute manifestation bronchique.

C'est là un fait sur lequel nous insistons déjà depuis longtemps dans nos cours, en le faisant contrôler par nos élèves.

Les examens microscopiques apportés par quelques auteurs, qui prétendent établir des rapports intimes entre la coqueluche et le catarrhe bronchique vulgaire, n'ont pas, à notre avis, la valeur qu'ils avaient voulu leur prêter. La coqueluche exempte de toute complication sérieuse a, sans exception, une suite heureuse, et les lésions inflammatoires par les autopsies ne sont, à vrai dire, que l'expression anatomique de complications survenues à la maladie fondamentale.

Les données fournies par l'anatomo-pathologie pourraient venir en aide à cette prétendue localisation ; si les examens cadavériques eussent été pratiqués sur des enfants enlevés au cours d'une coqueluche exempte de complications thoraciques, par quelque maladie intercurrente ou par une autre cause accidentelle.

Quelques cliniciens de la plus haute valeur ont prétendu que la toux quinteuse était provoquée par les mucosités accumulées dans l'arbre bronchique; cependant une longue et attentive observation nous a permis d'enregistrer un grand nombre de faits contraires à leur affirmation. Chez un certain nombre d'enfants, nous avons été à même de pouvoir contrôler cette existence préalable de la replétion bronchique à l'occasion des crises, mais nous devons avouer qu'il ne nous a pas été donné de la retrouver que dans les cas où la maladie était compliquée d'une bronchite étendue. Le résultat de ces recherches fut d'autant plus clair que nous avons recours au stéthoscope de M. Constatin Paul qui a, comme on le sait, le grand avantage d'exagérer les bruits de la cage thoracique.

Il ne faut point oublier que l'enfant attrape souvent sa coqueluche au cours d'une bronchite préalable, qui constitue par elle seule une condition prédisposante de celle-ci; il peut arriver alors qu'on prenne, à un examen léger, le catarrhe bronchique comme la première étape de la toux convulsive.

Dans de pareilles conditions, oui, les quintes sont quelquefois provoquées par la réplétion de bronches due à la sécrétion muqueuse.

Le besoin de tousser qu'entraîne cette réplétion, amène, à son tour, la crise spasmodique, comme cela arrive également lorsque l'enfant avale de travers, s'effraie, etc.

Enfin une circonstance digne de remarque c'est le défaut de parallélisme entre la durée et la violence des quintes, et l'intensité et l'étendue de la phlégmasie bronchique.

Mais ce n'est pas tout, l'atténuation ou la disparition de la coqueluche à la suite d'une broncho-pneumonie intercurrente est encore une preuve de ce qu'aucun lien n'existe guère entre cette dernière phlégmasie et l'affection qui nous occupe. Ce fait est du domaine vulgaire. De plus, cette indépendance des deux affections est tout-à-fait confirmée par la physiologie expérimentale laquelle a démontré que l'excitation mécanique ou chimique des radicules bronchiques ou des alvéoles pulmonaires n'éveille absolument pas de toux.

Digitized by Google

En dernier lieu l'observation clinique rigoureuse des faits a laissé voir que la coqueluche, en l'absence de toute complication bronchique ou pulmonaire, n'est jamais une maladie fébrile.

Cela est un argument qui plaide hautement en faveur de la théorie que nous soutenons.

Ma longue observation m'engage à affirmer que, soit au début soit au cours d'une coqueluche, l'élévation de la chaleur est toujours l'indice de quelque complication du côté des poumons, ou l'expression de quelqu'autre maladie fébrile.

En résumé, tout porte à croire que la coqueluche est en définitive une affection essentiellement apyrétique, siégeant primitivement à l'entrée de l'arbre aérien.

Comment considérer la nature de la coqueluche ?

Dès l'apparition des premières épidémies au 15ème siècle, en Europe, les observateurs avaient reconnu la contagiosité de la maladie ainsi que l'immunité créée par une première attaque, cependant ignoraient-ils la cause de cette transmition par contagion.

Rosen de Rosenstein avait conçu l'idée que la coqueluche était engendrée par la pénétration dans les tuyaux bronchiques d'une matière étrangère ou un principe nuisible qui se répand et se propage comme celui de la petite vérole, parmi les individus qui n'en ont pas encore éprouvé l'impression.

Linné de son côté avait émis l'hypothèse de l'introduction dans les voies aériennes d'œufs infiniment petits d'une espèce particulière d'insectes.

Ce ne fut qu'à partir de 1867, que survinrent les recherches microscopiques pratiquées sur les crachats des coquelucheux par Poulet, Letzrich, Henken, Tschamer, de Gratz et Burger de Bonn.

Convaincus de la nature parasitaire de l'affection nous nous sommes livré à des recherches microscopiques, dont les premiers résultats furent publiés en 1883.

Les examens souvent répétés jusqu'à présent n'ont donné que le même résultat.

Voici ce que nous avons toujours observé.

Nous avons procédé à l'examen microscopique des crachats des enfants affectés de coqueluche, soit pendant la période catarrhale, soit dans la période spasmodique, non seulement de ceux qui ne se trouvaient pas encore sous l'influence d'aucune médication, mais aussi de ceux qui étaient soumis à l'usage de la résorcine, et dont plusieurs, parmi ces derniers, étaient bien près de la guérison.

Dans les crachats rejetés immédiatement avant l'examen, la simple inspection nous y a permis de constater une assez grande quantité de petites masses jaunâtres irrégulièrement arrondies du volume à peu près d'une tête d'épingle plongeant dans les mucosités.

Le nombre de ces corpuscules était proportionnel à la durée et à l'intensité de la maladie.

Par l'examen microscopique, sans l'addition d'aucune matière colorante, nous avons rencontré des cellules épithéliales pavimenteuses, polyédriques irrégulières et pourvues d'un noyau, des globules de pus plus ou moins nombreux, des globulins et une quantité considérable de micrococci ; tous ces éléments étaient plongés dans une matière amorphe, fortement agglutinée que constituait la gangue de ce magma.

Les cellules épithéliales se montraient sous des volumes différents et formes diverses en étant pourvues toutes d'un noyau et d'un double contour.

Ce qu'on y voyait de plus intéressant, c'était l'infiltration de spores infiniment petites et brillantes, des véritables micrococci, qui existaient en profusion dans l'intérieur des cellules, tout en y gardant la même disposition que dans la substance amorphe qui les entourait, c'est-à-dire la disposition en séries linéaires, en chaînes ou chapelets ou en groupes, qui variaient de deux, trois, quatre et plus, ou finalement elles étaient isolées et distinctes les unes des autres, quoique très rapprochées en raison de leur nombre considérable.

Digitized by Google

Ces éléments se rendent plus évidents en colorant la préparation avec le violet de méthyle, ou mieux, avec le violet de gentiane.

Dans d'autres points les micrococci sont groupés formant de zooglée.

Ces spores sont plus nombreux au cours de la période spasmodique et sont en diminuant au fur et à mesure qu'on pratique le traitement local antiseptique.

Aussi, ce traitement fait cesser le mouvement dont ces petits éléments étaient doués auparavant.

Nous avons ensemencé dans la gélatine stérilisée les micrococci contenus dans les crachats recémment rejétés, et nous avons pu y voir le développement du microgerme, en constatant la présence de mycèles entourés de petites cellules arrondies de 15 à 20 millièmes de millimètre de diamètre, pleines de petites spores.

En pratiquant avec cette culture des badigeonnages péri-glottiques sur des cobayes, nous avons observé à plusieurs reprises l'apparition de la toux quinteuse, notamment lorsque ces badigeonnages étaient précédés ou accompagnés d'un léger traumatisme à la partie supérieure du larynx en y déterminant la desquamation épithéliale.

De tout ce qui précède, il résulte pour moi que la coqueluche a son siège primitif, à l'entrée de l'arbre bronchique, et est dûe à la prolifération d'un microgerme sur la muqueuse qui tapisse cette région.

En admettant cette manière de voir, nous avons conclu que la thérapeutique devrait être locale et antiseptique, et nous avons recours de préference à la résorcine tant par sa presque nulle causticité, sa nulle odeur, que par son extrême solubilité.

En 1883, nous avons publié, 30 cas de guérison sur 30 enfants affectés de coqueluche plus ou moins grave et aux diverses périodes de son évolution.

La résorcine fut chez tous employée dans la proportion d'un pour cent.

En 1884, dans un nouveau travail, j'ai signalé encore 40 cas de coqueluche guéris dans un très-court délai par ce même moyen, mais déjà à la proportion de deux pour cent. ce dosage fut progressivement élevé au point que j'emploie aujourd'hui une solution résorcinique à huit pour cent.

Nous nous servons exclusivement pour cela de la substance chimiquement pure, la seule douée des propriétés ci-dessus indiquées.

En 1885, je me suis adressé au chlorhydrate de cocaïne (10 pour 100) en badigeonnages péri-glottique précédant celle de résorcine ; j'ai le premier recours à cet agent chez les coquelucheux dans le but de calmer l'excitabilité réflexe de la muqueuse laryngienne et de la sorte, il m'a été presque toujours possible d'amoindrir l'intensité et la fréquence des quintes, notammant au début du traitement, lorsque la résorcine n'a pas encore eu le temps d'anéantir les germes producteurs du mal.

La combinaison de ces deux agents constitue donc, à mon avis, le meilleur moyen de traitement de la coqueluche.

Postérieurement à la détaillée des 70 cas de guérison dans mes travaux de 1883 et de 1884, nous avons eu à soigner un très-grand nombre de cas de coqueluche selon ma méthode et le résultat a répondu à mon attente dans tous les cas où j'ai pu agir convenablement et pendant le temps nécessaire.

Au bout de quelques jours, parfois même d'un jour, l'inspiration sibilante est disparue et l'intensité des quintes s'atténue sensible et progressivement.

Dans quelques cas où il m'a été donné d'intervenir peu de jours après l'éclosion de la maladie j'ai pu même l'enrayer au bout de quatre à cinq jours.

Ces remarquables succès obtenus par nous et par nos élèves, ont répandu ma méthode thérapeutique dans presque tous les pays, et nous recevons constamment de nouvelles confirmations de son efficacité.

A l'étranger des contrôles aussi favorables ont eu déjà lieu ; je ferai mention toute particulière de celui de la part de mon éminent ami M. le Professeur W. Barlow, de Manchester, qui a publié sur ce rapport un long mémoire. (*The Lancet*, May, 1886.)

Digitized by Google

Toutes les complications broncho-pneumonique survenues dans le cours de la coqueluche sont traitées par les moyens ordinaires sans aucune interruption du traitement local.

EXPLICATION DES PLANCHES.

PLANCHE I.

Fig. 1.

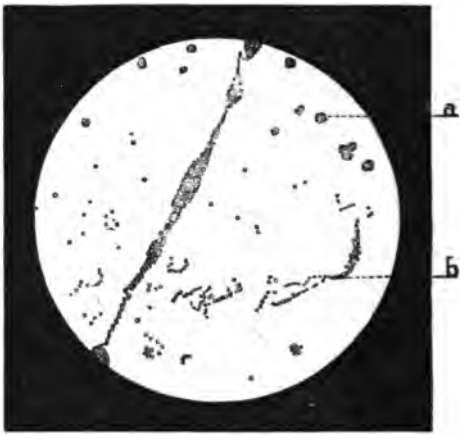


Fig. 2.

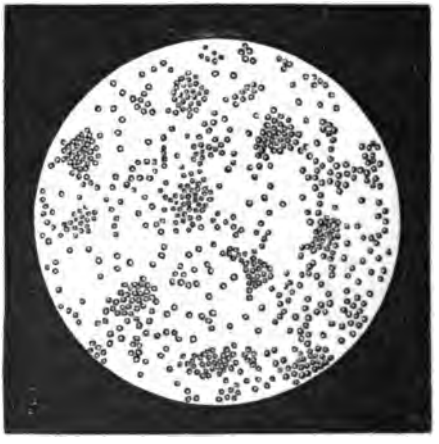


Fig. 1.—Préparation de mucosité laryngienne d'un enfant atteint de coqueluche aussitôt après son expulsion au moment d'une quinte.

Fig. 2.—Préparation de mucosité retirée du larynx d'un cochon d'Inde mort à la suite d'une broncho-pneumonie survenue au cours d'une coqueluche expérimentalement provoquée au moyen de badigeonnages périglottiques avec le germe provenant de la culture dans la gélatine.

Digitized by Google

PLANCHE II.

FIG. 1.

FIG. 2

Fig. 1.—Préparation de la couche profonde de la zone de culture dans la gélatine du germe de la coqueluche contenue dans des mucosités expulsées par un enfant au moment de la quinte. Les germes s'y présentent à un degré de développement moins avancé que ceux de la préparation suivante.

Fig. 2.—Préparation de la couche la plus superficielle de la même culture précédente; toutes les deux faites dans la même séance quelques jours après l'ensemencement.

Digitized by Google

DES HALLUCINATIONS CHEZ LES ENFANTS.

HALLUCINATIONS IN CHILDREN.

HALLUCINATIONEN BEI KINDERN.

PAR LE DR. BOUCHUT,

De Paris.

Tous les médecins connaissent ce qu'on appelle les terreurs nocturnes chez les enfants. Elles sont caractérisées par le réveil en sursaut avec frayeur, des cauchemars et des hallucinations engendrées par un rêve qui se poursuit chez l'enfant à moitié réveillé. Il s'agit de tout autre chose dans la communication que je désire faire au Congrès. Il s'agit des *hallucinations passagères de jour* et *dans l'état de veille*, chez des enfants bien portants et en convalescence et cela sans qu'il soit question d'aliénation mentale. J'en ai observé 72 cas chez des enfants de deux à quatorze ans qui ont été publiés en 1886, dans un travail du Dr. Fernand Bouchut, et ce sont ces faits qui doivent me servir dans cette communication.

Mais avant tout, il est bon de dire ce que c'est qu'une hallucination, et quelles divisions il faut établir dans leur étude, si on tient à ne pas laisser pêle-mêle des phénomènes semblables en apparence et dont la cause est absolument différente.

L'hallucination est, comme l'a dit Van Helmont, le rêve de l'homme éveillé.

C'est, dans l'état de veille, un trouble de l'intelligence et de la sensibilité caractérisé par la perception anormale, imaginaire et passagère des choses extérieures qui n'existent pas. Les mêmes phénomènes chez l'homme endormi constituent le rêve.

Ainsi, l'apparition d'images agréables ou terrifiantes de fantômes et d'animaux fantastiques, la perception de voix aimées ou menaçantes, de sons mélodieux, la sensation d'odeur agréables ou désagréables, les sensations douloureuses ou voluptueuses des différentes parties du corps qui surviennent pendant le sommeil, forment le rêve, et elles s'évanouissent au réveil sans avoir troublé l'intelligence et sans laisser dans la conscience la conviction intime de leur réalité.

Tout au contraire, les mêmes perceptions ou sensations, dans l'état de veille, sont rapportés à des objets extérieurs qui n'existent, pas et elles engendrent des connexions erronées qui annoncent un trouble de l'intelligence et caractérisent l'hallucination.

L'homme donne alors un corps au produit de son entendement, il rêve tout éveillé.

Les hallucinations se présentent sous deux formes admises par Esquirol :—

1. L'hallucination proprement dite, caractérisée par la perception imaginaire d'un objet extérieur, d'une odeur, d'un fumet ou d'une sensation qui n'a aucune réalité extérieure.

2. L'illusion sensoriale dans laquelle la perception s'exerce sur un objet du dehors dont la configuration se transforme, et fait par exemple : qu'un chapeau parait être une figure grimaçante ; qu'une odeur agréable est infecte ; qu'à cheval sur un bâton, le sujet se figure être sur un coursier fantastique.

Elles ont pour siège les différents organes des sens et affectent la vision, l'ouïe, l'odorat, le goût et le toucher.

Chez les enfants, ces perceptions anormales occupent surtout le sens de la vue, tandis que chez l'adulte, d'après Esquirol, Lelut, Parchappe, Baillarger, Delasiauve, Motet, etc., elles occuperaient le sens de l'ouïe.

C'est là une différence importante à noter.

D'après ce que j'ai vu il est nécessaire d'admettre que les hallucinations chez les enfant sont :

1. Idiopathiques et Suggestives.
2. Sympathiques ou réflexes.

Digitized by Google

3. Toxiques.
4. Organiques.

Mais toutes dépendant d'une modalité pathologique des cellules cérébrales de leur imbibition sanguine variable, et de la composition du sang qui les nourrit.

HALLUCINATIONS IDIOPATHIQUES.

Les hallucinations idiopathiques sont celles dont la cause occasionnelle est encore peu connue et résultent de la surexcitation cérébrale, de l'intelligence de l'imagination et des facultés affectives. On en voit chez les enfants très-jeunes que l'on effraie par des contes terrifiants et des histoires fantastiques, que l'on mène à des féeries, à des spectacles de bataille et chez des enfants plus âgés dont on excite mal à propos la terreur religieuse.

HALLUCINATIONS DIURNES ESSENTIELLES PAR SUITE DE FRAYEUR.

Un enfant de 12 ans qui faisait sa première communion à Montmartre fut si effrayée des terreurs de l'Enfer nées dans son esprit par l'influence du prédicateur qu'elle perdit connaissance et eût quelques mouvements convulsifs ainsi que plusieurs de ses compagnes.

Le lendemain, les attaques se reproduisirent et en même temps, pendant la journée, il y eut l'hallucination d'un crucifix rouge de feu au milieu de l'espace.

Cette hallucination se montra plusieurs jours de suite. Des lavements d'eau chloroformée la guérirent de ses attaques et de ses hallucinations.

J'ai observé trente cas de ce genre, dans lesquels la terreur religieuse, une frayeur ou des émotions morales de différentes natures ont produit des résultats semblables.

Dans tous ces cas, l'hallucination a eu lieu dans le jour, a été passagère et n'a pas eu de suite comme trouble de l'intelligence, de la raison ou de la santé.

HALLUCINATIONS SUGGESTIVES.

Mais, à côté de ces faits cliniques tirés de la pathologie infantile ordinaire, il y a des faits nouveaux, qui depuis les pratiques du magnétisme animal, de l'hypnotisme et de la suggestion hypnotique, attestent la facilité qu'il y a à provoquer des hallucinations passagères et très variées dont il serait bien difficile d'établir la cause anatomique autrement que par une modalité d'action et d'imbibition variable des cellules cérébrales.

Ces hallucinations suggestives faciles à déterminer chez l'adulte, se produisent également chez les enfants lorsqu'on peut les hypnotiser, les anesthésier et produire les phénomènes de transfert. J'en ai publié deux exemples.

HALLUCINATIONS RÉFLEXES, SYMPATHIQUES.

Les convulsions sont le phénomène nerveux le plus habituellement observée au début des maladies aiguës fébriles de l'enfance telles que la pneumonie, l'angine tonsillaire et les fièvres éruptives; mais de temps à autres on voit avant la convulsion ou après elle, des hallucinations passagères visuelles qui ne viennent qu'une fois, rarement d'avantage. Ce phénomène est quelque chose d'analogue au délire du début de la pneumonie et de la variole chez l'adulte, alors que rien n'est déclaré et qu'il est impossible de faire un diagnostic.

Il annonce le commencement d'une maladie, dont la localisation ne sera peut-être possible que deux ou trois jours après. L'organisme est atteint subitement et réagit aussitôt par le nerf sympathique, pour produire soit des convulsions, soit des hallucinations, soit du délire selon l'âge des sujets et leur idiosyncrasie naturelle ou acquise.

Chez les jeunes enfants, on n'observe guère dans l'état fébrile que les hallucinations ou les convulsions, ce que témoignent les vingt observations que j'ai recueillies. Chez

Digitized by Google

l'adulte, au contraire, c'est du délire et quelquefois du délire furieux, hallucinatoire, si le sujet est alcoolique.

Que se passe-t-il alors dans l'intérieur du cerveau et dans le fonctionnement des cellules cérébrales qui font la perception, l'intelligence et la motilité? Je ne veux rien préciser pour ne pas faire du roman scientifique, mais il est permis de croire que ces troubles étant passagers dépendent également d'un trouble passager de la circulation vaso-motrice cellulo-cérébrale et que l'hypérémie peut être considérée comme étant la cause de ces accidents. Ce qui peut autoriser à dire qu'il en est ainsi, c'est qu'un autre épisode de début des maladies fébriles, le frisson n'est qu'une convulsion partielle et la sensation prononcée du froid qui n'est qu'une illusion sensoriale, est accompagnée d'une élévation de température, 40 degrés, qui révèle un trouble vaso-moteur profond amenant la paralysie vaso-motrice passagère.

Sensation du froid malgré l'élévation thermique, perception d'images qui n'existent pas ou hallucinations, convulsion et délire à la première heure d'une maladie aiguë fébrile innommée sont des perturbations nerveuses de même ordre; ce sont des accidents réflexes jadis appelés sympathiques, dont la constriction ou la paralysie vaso-motrice peuvent seules rendre compte.

Rech a observé quatre cas d'hallucination survenue à la suite du froid.

Quatre enfants après avoir été exposés plusieurs heures à un froid très vif, passèrent brusquement dans un appartement très chauffé. Immédiatement ils devinrent anxieux en proie à des hallucinations. Le visage vultueux, le poux rapide, la température était normale.

Au bout de quelques jours, tout était rentré dans l'ordre, mais tous les quatre eurent une assez forte amygdalite avec bronchite.

Ces hallucinations je les ai observées dans vingt et un cas de pneumonie; d'angine de stomatite, de dentition, de scarlatine, et une fois l'accident passé, ainsi que la maladie provocatrice, il n'est resté aux enfants aucun trouble de l'intelligence et de la raison.

HALLUCINATIONS TOXIQUES.

Chez les enfants, on observe chez eux que des hallucinations toxiques accidentelles, par la belladone, le datura ou n'importe quelle autre solanée vireuse, empoisonnements dûs à une erreur de pharmacien, comme dans l'observation ci-jointe, ont causé par la gourmandise de petits enfants qui prennent dans les jardins des baies de belladone dont ils se régalent jusqu'à en mourir.

Ici la cause anatomique semble assez facile à indiquer, car, si l'on a vu des sujets soumis à des doses toxiques de belladone, on voit chez eux une telle turgescence des capillaires du visage des yeux, qu'il est impossible de méconnaître une paralysie vaso-motrice considérable, et puisqu'en même temps se produisent de terribles hallucinations et un violent délire, il n'y a rien de hasardeux à soutenir que ces troubles du système nerveux se rattachent à l'hyperhémie cérébrale. Les autopsies, le microscope et la physiologie sont là qui prouvent l'exactitude de cette assertion.

HALLUCINATIONS D'ORIGINE CÉRÉBRALE.

A côté de ces troubles vaso-moteurs passagers de la circulation cérébrale et de leur action sur les cellules cérébrales, il y a, chez les enfants comme chez l'adulte, des hallucinations qui dépendent d'une légion organique du cerveau et de néoplasme (gliôme) ou tubercules plus ou moins volumineux.

Ces néoplasmes amènent d'une façon intermittente des perturbations vaso-motrices de voisinage qui se révèlent par des hallucinations passagères ou qui engendrent un état aigu dans lequel les hallucinations sont le phénomène dominant. J'en ai vu un bel exemple dans lequel le diagnostic fort incertain a été tout-à-coup éclairé par l'examen de l'œil qui a fait découvrir un tubercule de la choroïde.

Digitized by Google

Dès lors il n'y eut plus de doute ; les hallucinations résultaient de l'hyperhémie cérébrale voisine d'un tubercule du cerveau. L'autopsie a permis de vérifier ce diagnostic.

HALLUCINATIONS DE LA FIÈVRE TYPHOÏDE, DE L'ICTÈRE DE LA CHORÉE.

Les hallucinations se rencontrent aussi, chez les enfants, dans les cas de fièvre typhoïde au début ou dans le cours de la maladie, dans l'évolution de la jaunisse, et enfin nous les avons vues dans la chorée soit au début, soit dans l'évolution de cette névrose congestive de la moelle.

THÉORIE ET MÉCANISME DES HALLUCINATIONS.

Les théories de l'hallucination sont nombreuses, et elles relèvent de l'ordre *surnaturel*, de l'ordre *psychologique* et de l'ordre *anatomique*, c'est-à-dire, de l'état cellulaire du cerveau.

Le rôle du *surnaturel* dans l'explication des hallucinations a été longtemps en faveur. C'est la théorie du mysticisme et des esprits dominés par les croyances religieuses de la démonologie ; de la sorcellerie et des possessions démoniaques, mais cette intervention des démons familiers des génies, des fées, des anges, de satan, de Dieu dans les hallucinations, si considérable dans l'antiquité et au moyen âge, n'a plus rien de scientifique. Rien dans la surnaturel ne peut satisfaire les exigences de la physiologie et de la pathologie moderne. Les inspirations de l'extase religieuse, les visions célestes, les voix divines et les illusions sensorielles de la possession démoniaque, de la magie, de la sorcellerie sont des manifestations de mysticisme religieux ou profane, et de la foi divine qui se rattachent à la nervosité morbide, et c'est à ce point de vue qu'il faut se placer si on veut se rendre compte des phénomènes hallucinatoires.

Le théorie *psychologique* qui dans la première moitié du siècle, a joui d'une certaine faveur, n'explique pas grand'chose, car elle se contente d'hypothèses hasardeuses rendant compte du fait par ce fait lui-même. Ainsi que veulent dire à côté de ce qu'on appelle les conceptions normales de l'intellect, ces autres conceptions intérieures dites anormales par M. Garnier, constituant les fausses perceptions, ou les hallucinations qui sont l'expression dans un langage différent du fait à analyser.

Pour être complet, il faudrait ajouter que ces conceptions anormales de l'halluciné s'accompagnent d'une perception imaginaire d'images extérieures, de bruits étranges de sons musicaux, de paroles bizarres, de contact cutané agréable ou douloureux, ce que la conception normale ne produit jamais.

Ainsi les yeux fermés, j'ai la conception d'un paysage, de la figure d'un ami, mais je n'en ai pas l'image ; ceci c'est un rappel mental d'une perception antérieure, tandis que chez l'halluciné ce rappel mental prend la forme extérieure d'un acte sensoriel déterminé entraînant la conviction d'un fait réel.

Le Dr. Peisse considère l'hallucination comme l'exagération du rappel mental ordinaire de la perception des sons et des images. C'est-à-dire, comme un acte de mémoire. Mais chez un sujet bien portant, les yeux ouverts ou fermés, le rappel mental ne donnent que des images confuses des objets et des couleurs de la nature sans perceptions réelles; que des mélodies sans bruit, que des souvenirs gustatifs, tactiles, sans aucune sensation réelles. Et, si forte que soit la tension de la mémoire dans l'évocation d'une forme idéale que l'on voit et que l'on pourrait prendre, ce sujet n'est pas dupe de la conception anormale imaginaire, tandis que l'halluciné est dupe de la conception visuelle, auditive ou autre.

C'est là une différence absolue.

Un autre philosophe, dont le nom est resté célèbre, Buchez a soutenu la même théorie du rappel mental donnant la conviction d'une extériorité qui n'existe pas, mais

Digitized by Google

cette explication est encore de celles qui rendent compte du fait par le fait lui-même et ne peuvent pas faire beaucoup avancer la science.

Pour Letut, l'auteur du démon de Socrate, les hallucinations sont la transformation des idées en sensations, chose contraire et opposée au fonctionnement normal de l'intellect qui s'élève de la sensation et de la perception à la formation des idées.

Ce sont, dit-il, des sensations fausses prises pour des sensations véritables; cela n'est pas exact, car chez les hallucinés, il n'y a que des conceptions anormales donnant lieu à des sensations véritables ce qui est bien différent.

Dans son *traité des hallucinations*, on voit que, pour Brière de Boismont, le rappel mental et la conception qui en résulte peuvent, dans leur exagération, donner lieu à de véritables sensations, ce qui amène l'hallucination physiologique qu'on peut observer chez les sujets nerveux et de haute intelligence par la force d'attention et de volonté.

Sans quelques différences dans le langage, Delasiauve, Parchappe, Fabret et d'autres aliénistes ont donné à ces théories le secours de leur talent et de leur expérience, mais comme on le voit par ces courts extraits, tout en restant ou à peu près à cette idée que l'exagération du rappel mental produit les fausses sensations, on tourne dans le même cercle, et il n'en sort rien de bien satisfaisant.

La *théorie anatomique* tirée de l'état cellulaire du cerveau, des troubles passagers qui s'y produisent a donné et donnera les meilleurs résultats.

Sandras a émis cette idée que l'hallucination est une manifestation pathologique à part dans les désordres dont le système nerveux nous montre une étrange collection. C'est un fait entièrement distinct de la pensée, de la réminiscence, de la sensation normale.

Sandras, qui avait eu de nombreuses hallucinations et qui les avait bien étudiées sur lui-même, était un médecin distingué de l'hôtel-Dieu dont la raison et l'intelligence étaient appréciées de ses collègues.

Il avait eu une petite hémorrhagie cérébrale avec hémiplégie et dont il avait guéri, c'est à la suite de cet accident qu'il avait de fréquentes hallucinations, qu'il distingusit très-bien de ses sensations normales.

Bourdier dans le même ordre d'idées, a soutenu que l'hallucination était toujours une opération pathologique et jamais physiologique.

Cela est vrai, mais il est allé trop loin en disant que raison ou hallucination s'excluaient réciproquement et que si l'hallucination était compatible avec l'intégrité de la raison.

Il est évident que c'est là une affirmation sans valeur démontre par le nombre des hallucinés célèbres jonissant d'une intelligence supérieure atteignant le génie.

La même pensée de localisation se trouve dans le mémoire de Baillarger sur les hallucinations qu'il attribue.

1. À l'exercise involontaire de la mémoire et de l'imagination.
2. À la suspension des impressions internes.
3. À l'excitation interne des appareils sensoriels. C'est là que se trouve la pensée localisatrice; les deux autres expliquent aussi bien le rêve que l'hallucination et confondent ces deux phénomènes qui doivent être séparés.

D'après tout ce que nous savons sur la physiologie du cerveau dans ses rapports avec le fonctionnement de l'intelligence de la mémoire, du langage, avec la détermination du siège de la sensibilité et de la motilité, il semble qu'on ne puisse séparer l'origine des troubles de l'intelligence, de la sensibilité et du mouvement, des modifications petites ou grandes passagères ou permanentes de l'état cellulaire du cerveau et en particulier des centres.

La psychologie, dans ses applications métaphysiques peut chercher autre chose, mais la médecine clinique ne doit pas avoir la prétention de s'élever si haut.

Digitized by Google

Elle doit se borner à la recherche des modifications de texture d'imbibition sous forme des cellules qui correspondent aux différentes fonctions du système nerveux.

En effet, à côté des grosses lésions d'un traumatisme, d'une rupture, d'une oblitération vasculaire, du développement d'un néoplasme ou de quelques entozoaires, il y a des modifications partielles de texture cellulaire cérébrale, hyperhémie, anémie par imbibition sanguine variable, résultant d'une insolation de l'action du grand sympathique, de son influence vaso-motrice. Est-ce qu'un homme frappé d'insolation qui déraisonne momentanément, qui a des hallucinations pendant une journée n'a pas eu de modification si légère qu'elle ne soit dans l'imbibition des cellules cérébrales correspondantes aux fonctions troublées? Deux jours après, tout rentre dans l'ordre, mais si l'insolation a été plus forte et les lésions plus accentuées, si ces lésions sont considérables et permanentes, l'intelligence peut être plus sérieusement troublée ou à tout jamais perdue.

C'est aussi dans cette voie que M. Luys et M. Ritti se sont engagés pour rendre compte de la formation des hallucinations.

Ainsi, pour eux, ce sont les vibrations des cellules de la couche corticale produite par les stimulations de la couche optique qui engendrent les perceptions sensorielles.

Mais, à l'état pathologique, le contraire à lieu, et les vibrations naissent spontanément dans les cellules des couches corticales produisent des perceptions anormales par suite d'automatisme, comme le dit Baillarger.

Le même phénomène d'automatisme pourrait avoir lieu également dans les cellules de la couche optique comme si une sensation réelle venait de les mettre en mouvement. Et M. Ritti (*Théorie de l'hallucination*, page 50) dit :

"À l'état normal, les objets extérieurs amènent l'excitation des nerfs sensoriels. Dans l'hallucination, au contraire, une excitation interne, celle des ganglions sensoriels, amène des représentations perçues par le malade, et qu'il objective comme si une impression sensorielle venait irriter le nerf sensoriel.

Puis il se résume en disant que les phénomènes du processus morbide de l'hallucination sont :—

1. L'activité spontanée des cellules de la couche optique provoquée par des causes variables.

2. L'irradiation de cette activité fictive vers les cellules de la substance corticale.

3. L'entraînement consécutif de ces mêmes cellules corticales qui mettent en œuvre ces matériaux erronés avec la même logique que s'ils étaient réels (page 51).

Quand on est bien au courant de la physiologie actuelle du cerveau, cette théorie de l'ébranlement et de la vibration spontanée des cellules du cerveau satisfait bien l'esprit. Mais on peut se demander qui a vu la vibration des cellules du cerveau? Evidemment, c'est là une brillante hypothèse.

Dans cette étude qui repose sur ces infiniment petits et sur des éléments invisibles, n'est-il pas préférable d'attribuer à un désordre anatomique cellulaire passager, sur un petit territoire cellulaire de la couche corticale, le trouble fonctionnel hallucinatoire que M. Ritti attribue à la vibration cellulaire? Sans rien affirmer à cet égard, il nous semble que cela pourrait être plus exact.

À la suite d'une grande surexcitation du cerveau par la religion, la politique, l'amour contrarié, l'ambition de la fortune ou des honneurs, une grande frayeur, il est bien évident que tous les actes de certains individus révèlent un fonctionnement cérébral irrégulier qui résulte d'une modalité pathologique des cellules correspondant aux facultés en désordre.

Sans pouvoir déterminer la nature exacte de cette altération il est difficile de ne pas en admettre l'existence.

Les hallucinations extatiques ou démoniaques, les substitutions de personnalité qui font croire à un politicien du bas étage qu'il est empereur ou roi ; les illusions senso-

Digitized by Google

rielles de celui qui, comptant des cailloux dans sa main, assure qu'il entasse des millions ; les ennemis que croit voir et entendre un homme qui a échappé à un grand péril ; les propositions deshonnêtes qu'une femme croit entendre derrière elle dans la rue; ne sauraient exister sans une modalité cellulaire pathologique au cerveau.

Les lésions ophthalmoscopiques trouvées dans ces cas font croire à la congestion cérébrale qui se traduit par la flexuosité des veines, la turgescence des artères et quelquefois par des hémorrhagies de la rétine.

A la première heure des maladies aiguës fébriles et des fièvres éruptives de l'enfance, alors que l'on ignore ce qui va se produire, si le grand sympatique par ses actions réflexes et vaso-motrices, détermine des hallucinations ou une convulsion, tous les physiologistes et les médecins admettront bien que, si l'état morbide fébrile agit sur le système nerveux central pour produire de tels désordres fonctionnels, cet état morbide a modifié l'état du cerveau, c'est que les cellules cérébrales directrices de la fonction troublée ont subi l'influence constrictive ou paralysante des nerfs vaso-moteurs, et qu'il en est résulté une maladie pathologique passagère à laquelle on peut attribuer les perceptions anormales de l'hallucination.

Le problème est plus délicat, plus difficile à résoudre aujourd'hui pour les hallucinations suggestives de la vue, de l'ouïe, du goût, de l'odorat, du toucher.

Comment ces hallucinations, de même que les paralysies de la sensibilité et du mouvement peuvent-elles se produire au commendement de l'expérimentateur et disparaître à sa volonté.

Nous n'avons aucune donnée certaine pour répondre à cette question et pour formuler l'explication de ces phénomènes morbides. Il est bien probable qu'il y a une influence morale agissant sur l'état physique et psychique.

Or, on sait depuis longtemps combien, en dehors de l'hypnotisme, cette influence est considérable et les traités de pathologie renferment les observations les plus curieuses à cet égard, montrant les troubles de la circulation générale ou vaso-motrice qui amènent la perturbation ou l'arrêt des fonctions nerveuses. Il est probable qu'il en est de même dans les hallucinations et les paralysies suggestives par hypnotisme.

La sensibilité du système vaso-moteur est telle qu'il n'est pas impossible que, sous l'influence morale exercée par l'hypnotiseur, il y ait des phénomènes de perturbations fonctionnelles nerveuses, phénomène d'arrêt ou autre, et qu'il survienne, en conséquence, des modalités cellulaires cérébrales amenant les troubles passagers de perception de sensation et de mouvement.

C'est là du moins ce que la réflexion peut inspirer quand on désire se rendre compte des phénomènes dont je viens de parler.

Hors de là il n'y a que des hypothèses peu satisfaisantes. Il est bien plus facile de se rendre compte des hallucinations toxiques par les solanées vireuses, les papavéracées, le haschisch, l'alcool.

Là, il y a un élément connu et certain, c'est le poison qui circule dans les vaisseaux et vient imprégner les cellules cérébrales et tant que le poison n'est pas éliminé il y a une modification des cellules cérébrales par le sang toxique qui vient les nourrir et qui trouble leur fonctionnement physiologique ; nous ne savons pas ce qui se passe dans les cellules autrement que par les troubles hallucinatoires ou délirants des malades, mais nous sommes sûrs qu'un des deux facteurs de l'exercice régulier, normal de ces cellules, est altéré et qu'il est certainement la cause des désordres nerveux.

L'observation des malades et l'expérimentation qui peut renouveller le phénomène à volonté en sont la preuve.

Quand aux hallucinations de cause organique cérébrale il semble que leur mécanisme soit des plus faciles à exposer et à comprendre. Il n'en n'est rien ; qu'il y ait tubercule, gliome, ancienne hémorrhagie, ou n'importe quel néoplasme dans un point quelconque du cerveau ou de la moelle, cela n'explique point l'origine des hallucinations.

Digitized by Google

En effet, les différences, les variétés, de siège de la lésion cérébrale excluent toute idée localisatrice chez les enfants.

Pour se rendre compte des hallucinations dans ces cas, il faut réfléchir que la lésion apparente n'est peut-être pas la cause directe du phénomène morbide et qu'elle n'en est que la cause occasionnelle, au même titre qu'elle est cause de la céphalalgie, du vertige et des vomissements.

En effet, par leur présence dans le cerveaux et la moelle épinière les néoplasmes entretiennent ou peuvent entretenir dans tout l'organe des troubles circulatoires et nutritifs qui engendrent différents désordres du système nerveux dans l'intelligence, la sensibilité générale et spéciale et le mouvement.

Elles peuvent aussi bien troubler l'exercice régulier des perceptions normales pour produire des perceptions anormales ou hallucinations.

Il suffit pour cela que, dans le centre d'origine des perceptions, l'état des cellules cérébrales soit modifié par une influence de voisinage éloignée, et alors on comprend que des hallucinations puissent se produire au même titre qu'elles se produisaient sous l'influence d'une irritation sympathique lointaine, viscérale ou autre.

On comprend aussi la possibilité des hallucinations avec des tumeurs placées en avant ou en arrière des hémisphères cérébraux, dans les noyaux centraux, la couche optique, dans le cervelet ou le moelle.

C'est moins la lésion elle-même que son retentissement sur les cellules du centre des perceptions.

De cette façon, quelle que soit la cause occasionnelle des hallucinations, leur véritable origine peut être rapportée à une modalité pathologique toujours la même, et cette modalité peut être considérée comme une lésion vaso-motrice des cellules cérébrales du centre des perceptions.

OBSERVATIONS SUR LA PNEUMONIE FRANCHE INFANTILE.

OBSERVATIONS ON TRUE INFANTILE PNEUMONIA.

BEOBACHTUNGEN ÜBER DIE ECHTE PNEUMONIE BEI KINDERN.

PAR LE PROFESSEUR ADOLPHE D'ESPINE.

De Genève

INTRODUCTION.

MESSIEURS—Pour répondre à l'appel de votre Président, j'ai rédigé quelques observations tirées de ma pratique hospitalière et privée, qui ont trait à la *pneumonie franche.* S'il est un sujet de la pathologie infantile, qui semble connu dans ses moindres détails, c'est assurément celui-là. Aussi n'ai-je point la prétention de vous entretenir aujourd'hui de choses nouvelles et me bornerai-je à attirer votre attention sur certains points spéciaux qui m'ont paru avoir une réelle importance pratique.

Je diviserai mon travail en trois parties :—

1. Observations sur la forme centrale ou congestive.
2. Observations de pneumonies franches mortelles.
3. Considérations sur le traitement de la pneumonie.

I. FORME CENTRALE OU CONGESTIVE.

Pneumonia Vera Levissiona.—Il existe chez l'enfant une forme de pneumonie franche, qu'on peut appeller *centrale* ou *congestive*, forme difficile à reconnaître, parce que les signes

Digitized by Google

physiques sont peu accentués ou incertains, et que les signes fonctionnels (au point de certains crachats) sont absents ou de peu d'importance. Cette forme est souvent méconnue ou peut-être prise pour une autre affection (début de fièvre continue, éclampsie, fièvre de croissance ou de dentition).

Grisolle* la connaissait déjà et l'a parfaitement caractérisée en ces termes: "La pneumonie peut être latente, soit parce qu'elle occupe un trop petit espace, soit que située à une grande profondeur et au centre du poumon, elle se trouve enveloppée de toutes parts par une portion du poumon complètement intacte."

Parmi les auteurs plus modernes, la plupart d'entr'eux ont signalé l'apparition tardive ou le peu d'importance des signes physiques dans certaines formes de la pneumonie infantile; je n'en citerai que deux. Meigs et Pepper† distinguent la forme *lobaire* qui est caractérisée peu l'infiltration classique et la forme *partielle* "dont les symptômes, disent-ils, sont beaucoup plus obscurs et incertains que dans la forme lobaire." Cadet de Gassicourt‡ distingue la pneumonie à signes stéthoscopiques dès le début, à signes stéthoscopiques tardifs et sans signes stéthoscopiques; ce qui revient à dire suivant lui; pneumonie atteignant dès le début les parties superficielles du poumon, ne les atteignant que plus tard, restant centrale jusqu, à la fin. Cet éminent observateur relate l'histoire d'une pneumonie centrale chez un garçon de 10 ans, qui présenta tous les signes rationnels de la maladie, mais sans expectoration et sans signes physiques appréciables.

On peut à ce sujet distinguer deux catégories de faits. Dans la première, la durée de la fièvre est semblable à celle de la pneumonie classique, c'est la *forme centrale* proprement dite. Dans la seconde, la durée de la fièvre est raccourcie, c'est la *forme abortive*. On pourrait donner aux deux catégories réunies le nom commun de *forme congestive* ou bien encore de *pneumonia vera levissiona*.

(*a*) *Pneumonie Centrale.*—C'est à la première catégorie de faits, qu'appartient indubitablement l'observation suivante:

Observation 1.—Pneumonie centrale du sommet droit—Herpès labial—Pneumococcus de Fraenkel dans un crachat—Fièvre modérée de huit à neuf jours.

G. M. 16 ans garçon, encore enfant par son développement physique, d'une bonne santé habituelle, tombe malade et s'alite le 28 Mars, 1887, après s'être senti légèrement indisposé le 25 Mars et avoir ressenti quelques frissons le 26 et le 27.

Température axill. le 28 Mars, au soir, 38.6°. Le 29 Mars (deuxime jour), température au matin, 38.1°, au soir, 40°.

Sauf la fièvre et l'état saburral des premières voies, rien de particulier à noter, ni à la gorge, ni au ventre, ni aux poumons. L'apparition d'un groupe d'herpès labial me fait chercher une pneumonie que je ne trouve pas.

Hentederioni. Le 30 Mars (troisième jour), température au matin, 38.9°, au soir, 39.9°.

Rien de nouveau, sauf un léger toussotement.

Le 31 Mars (quatrième jour), temperature au matin, 38.7°, au soir, 39.8°.

Un épistaxis dans la matinée. Un peu de vertige, quand le malade est sur son séant. Rate normale, ventre non balloné, pas de diarrhée. État subjectif, bon.

Le 1er. Avril (cinquième jour), température au matin, 38.5°, au soir, 39.6°.

La toux a un peu augmenté; il n'y a aucun point de côté. Après une exploration minutieuse, j'arrive à constater aujourd'hui dans la *fosse sus-épineuse droite* une submatité dont les limites inféro-externes dessinent celles du lobe supérieur. Dans cette étendue, diminution considérable du murmure vésiculaire et dans un point très limité

* *Traité Pratique de la Pneumonie*, 1841, page 436.

† *Practical Treatise*, 1874, 5th edition, page 875.

‡ *Traité Clinique*, 1880, tome I, page 66.

Digitized by Google

situé à la partie interne du triangle sus-épineux, j'entends un peu de respiration bronchique aux deux temps, ainsi que du retentissement de la voix avec timbre légèrement bronchique. Pas de râles.

Potion avec cinq grammes de benzoate de soude.

Le 2 Avril (sixième jour), température au matin, 38.5°, au soir, 39.4°.

Mêmes lignes.

Le 3 Avril (septième jour), température au matin, 38.5°, au soir, 38.5°.

Le matin, quelques crachats verts un peu collants sont examinés au microscope après coloration au bleu de gentiane. J'y constate uu nombre considérable de pneumococcus de Fraenkel, ce qui confirme le diagnostic pneumonie franche.

La journée a été beaucoup meilleure depuis un lavement qui a provoqué une toux naturelle. La toux a diminué. La matité au sommet droit est moins nette aujourd'hui qu'hier. On entend encore un peu de timbre bronchique dans la respiration et dans la voix.

Le 4 Avril (huitième jour), température au matin, 38°, au soir, 38.5°.

Le 5 Avril (neuvième jour), température au matin, 37.7°, au soir, 38.1°.

Pour la première fois la nuit a été bonne et la toux a entièrement disparu. À la percussion de la fosse sus-épineuse droite, encore légère dimiuution dans l'intensité du cou en comparaison avec le côté sain. À l'auscultation, pas de râles; on ne trouve plus qu'un léger retentissement de la voix.

Je constate par contre, pour la fin fois, une submatité avec son plus élevé (high-pitched), et de l'expiration prolongée dans le triangle sus-demiculaire droit.

L'urine est encore foncée. L'enfant a essayé de faire quelques pas, la marche est titubante. L'herpès existe encore sous forme de croûte, sur la lèvre, plus de fièvre. Ce n'est que le 7 Avril (onzième jour), que les signes physiques sont redevenus absolument normaux et que l'enfant a repris tout son appétit. Il est probable que cette affection aurait pu passer dans la litérature sous le nom de fièvre synoque ou de fièvre herpètique, si la pneumonie n'avait pas été démasquée.

Il est bon d'insister peut-être ici sur le mode d'exploration physique à suivre en pareil cas.

(*a*) *Fosse Sus-épineuse.*—La percussion de la fosse sus-épineuse doit se faire en enfonçant profondément le doigt plessimétrique et en percutant doucement; la percussion doit être faite de même des deux côtés pour apprécier les différences. Il y a de nombreuses chances d'erreur, qui proviennent de la position asymétrique de l'enfant, de la tension plus ou moins grande des muscles par le cri, etc. Aussi faut-il contrôler à plusieurs reprises le résultat avant de l'admettre comme certain. Si la limite de la matité obtenue dessine exactement celle du lobe supérieur, on peut la rapporter avec beaucoup de probabilité à un lésion du poumon.

Il faut ausculter en fermant l'oreille libre. Ce précepte donné par Ziemssen* dans sa monographie sur la pneumonie est important surtout pour saisir le timbre bronchique qu'accompagne le retentissement de la voix ou du cri. Le premier signe sthéthoscopique et souvent le seul dans les parties du lobe supérieur éloignées des bronches, c'est *l'absence du murmure vésiculaire* sous l'oreille. Comme on peut entendre facilement chez les petits enfants du murmure vésiculaire propagé des parties voisines ou les petites dimensions de la fosse sus-épineuse par rapport à l'étendue de l'oreille du médecin, le signe stéthoscopique habituel sera une *diminution notable du murmure* vésiculaire, en comparaison avec le côté sain.

(*b*) *Triangle Sus-claviculaire.*—Il ne faut jamais oublier d'explorer chez l'enfant le triangle sus-claviculaire. La percussion en est plus facile, que celle de la fosse sus-épi-

* Berlin, 1862.

Digitized by Google

neuse, pourvu qu'elle soit faite avec douceur et révèlera souvent une matité très nette dans la forme dite contrôle de la pneumonie.

L'auscultation peut en être faite avec un stéthoscope binauriculaire à petite embouchure, dit *stéthoscope américain*, qui permettra d'apprécier surtout la diminution du murmure vésiculaire.

Comment peut-on se représenter l'état antiseptique du poumon dans les formes légères de la pneumonie, telles que celle de notre observation? L'infiltration solide, fibrineuse, doit être réduite à son minimum. Son siège est en général assez fixe; il correspond à la partie profonde et interne de la fosse sus-épineuse, c'est-à-dire, à l'orifice des grosses ramifications de la bronche supérieure. C'est peut-être le fait que voulaient exprimer Meigs et Pepper, par le nom de pneumonie partielle. Cette solidification doit être centrale au début, comme le prouve dans certains cas l'existence d'un peu de murmure vésiculaire superficiel, à travers lequel l'oreille perçoit déjà le retentissement à timbre bronchique de la voix et du cri. Le reste du lobe supérieur est atélectasié par la congestion inflammatoire, il est engoué ou plutôt englué; l'air n'y pénètre plus, d'où absence des râles crépitants habituels chez l'adulte, disparition de la respiration et submatité, comme les, résultat de l'exploration physique. L'extension et l'intensité du souffle expriment probablement assez exactement l'extension de l'infiltration fibrineuse. Il est assez fréquent d'entendre dans cette variété de pneumonie infantile l'intermédiaire entre la vraie respiration bronchique et le murmure vésiculaire auquel Austin Flint a donné avec beaucoup d'à-propos le nom de *respiration bronchovésiculaire* (Unbestimmtes Athmen des Allemands).

(*b*) *Pneumonie Abortive.*—Le nom de *peumonie abortive* a été donné pour la première fois, à notre connaissance par M. Picot et moi * à cette forme de pneumonie franche avec infiltration légère qui évolue plus rapidement que la forme ordinaire, c'est-à-dire, au moins de cinq jours. Cette manière de voir a été adoptée aussi par d'autres auteurs, en particulier par Baginsky † qui lui consacre un paragraphe spécial. Elle a été combattue par Cadet de Gassicourt qui distrait son histoire de celle de la pneumonie franche et en fait une maladie nouvelle sui-generis, sous le nom de *congestion simple du poumon*. C'est l'opinion soutenue par Hiené ‡ qui a donné la première description de cette affection en 1876. Nous ne voulons pas rentrer ici dans la discussion de ce point de doctrine. Je me borne à constater qu'en démontrant l'existence de la même preuve anatomique avec la durée classique de la pneumonie franche et la présence des microbes caractéristiques dans ces crachats, je crois avoir donné un argument décisif en faveur de votre manière de voir.

Nous sommes disposés à croire que la pneumonie abortive est très fréquente dans la première enfance et qn'on peut lui rapporter un grand nombre de convulsions fibriles, de fièvres dites dentition, mais que les signes physiques sont souvent peu accusés, c'est-à-dire, que la pneumonie abortive reste souvent centrale.

Si nous ne comptons dans nos notes que les observations dans lesquelles les signes physiques étaient évidents, nous n'en trouvons que six, qui se répartissent comme suit, au point de vue de la localisation :—

Sommet droit, 3.

Sommet gauche, 1.

Lobe inférieur gauche, 2.

La durée de la fièvre a été dans deux cas de deux jours, et dans les autres cas de trois jours.

---

* *Manuel Pratique des Maladies de l'Enfance*, second edition, page 548, Septembre, 1879.

† *Practische Beiträge zur Kinderheilkunde*, I. Heft, page 17, 30 Avril, 1880.

‡ *Thèses de Paris*, 1876.

Digitized by Google

L'observation suivante a été choisie parmi les six cas, comme exemple de la pneumonie abortive, à cause des particularités intéressantes de sa symptomatologie :

Observation 2.—Pneumonie abortive du sommet gauche—Convulsions tétaniformes—Rash morbilliforme chez un enfant d'un mois et demi—Durée de la fièvre : trois jours.

Enfant de M., âgé d'un mois et demi, né à terme et robuste (poids à la naissance 3.470 grammes), nourri exclusivement au sein le premier mois, allaitement mixte depuis 15 jours.

Le 24 Février, 1886, le médecin de la famille, le Dr. Ferrière, de qui je tiens ces renseignements, est appelé à 3 heures de l'après-midi. Il apprend que l'enfant n'est pas bien depuis la veille au soir, à la suite d'une promenade où il aurait pris froid ; il a eu un vomissement dans la soirée, la nuit a été agitée, le matin, il vomit continuellement et refuse le sein. La figure est très-pâle, les traits sont contractés, la respiration est rapide et saccadée.

A 4 heures, l'enfant a une convulsion tonique avec bouche serrée et écume surtout des lèvres.

Appelé en consultation avec le Dr. Ferrière à 7 heures du soir, je constate une forte fièvre (40.2 dans le rectum), une submatité avec résistance au doigt dans la fosse sus-épineuse gauche avec respiration bronchique dans la partie interne. Sous la clavicule gauche, le son n'est pas aussi plein qu'à droite. Il n'y a pas de toux. Je diagnostique une pneumonie franche et porte un pronostic plutôt favorable.

Ordonnance :—Ne donner comme nourriture que le sein. Compresses de priessuites autour du front. Enveloppement ouaté des extrémités inférieures. Un peu de rhum dans de l'eau à boire. Bains tièdes, si l'agitation ne cède pas aux compresses.

Le soir à 11 heures, respiration vague, avec arrêts complets de 3 à 4 minutes ; sorte de Cheyne-Stokes, température 40°, 25 Février (deuxième jour), à 9 heure du matin, température 39.1°.

La nuit a été plus calme que la précédente, maisl a respiration est encore irrégulière. Matité diminuée un peu dans la fosse sus-épineuse gauche, mais plus nette qu'hier en avant où elle s'est étendue à toute la région sus- et sous-claviculaire. Respiration bronchique en cet endroit.

L'enfant n'a pas encore toussé depuis le commencement de la maladie.

I. Bain tiède ce matin, suivi d'une sorte de syncope ou faiblesse.

Je revois l'enfant en consultation à 3 heures de l'après-midi, après une nouvelle crise de convulsions toniques, qui a été moins forte que celle de hier. Température 39.6°, à 6 heures du soir.

L'enfant a mieux pris le sein qu'hier, on continue les compresses de Priessuites.

Le 36 Février (troisième jour), température 38.8° à 9 heures du matin.

Le Dr. Ferrière constate l'existence d'un exanthème rubéoliforme sur le tronc, les jambes, les bras et la nuque ; il n'existe presque pas à la figure.

La matité est encore marquée en avant à gauche. Souffle près de l'angle sterno-claviculaire.

On cesse les compresses et les bains. Consultation à 3 heures de l'après-midi : L'exanthème s'est généralisé, mais il a déjà pâli depuis le matin, c'est un rash.

A 7 heures du soir température 37.5°, l'enfant est calme.

Le 27 Février (quatrième jours), au matin le rash a disparu. La fosse sus-épineuse gauche est redevenue normale. Encore un peu de matité en avant, mais il n'y a plus de souffle.

Température au matin, 37.6°, au soir, 38°.

Depuis lors, l'enfant s'est bien porté.

Remarques—J'attire l'attention dans cette observation

1. Sur l'absence totale de toux.

Digitized by Google

2. Sur la durée exacte de trois jours de fièvre, continue avec défervescence caractéristique.

3. Sur le caractère tonique des convulsions, qui a été déjà indiqué comme un caractère spécial de l'éclampsie des premières semaines de la cei, par Soltmaun.

4. Sur l'apparition du rash, qui a été signalé déjà dans le cours de la pneumonie infantile, soit par Rilliet et Barbthez, soit par Cadet de Gassicourt.*

II. PNEUMONIES FRANCHES MORTELLES.

La pneumonie franche chez l'enfant est de l'avis de tous les auteurs une maladie essentiellement bénigne, qui a une tendance naturelle à la guérison. Néanmoins, il ne faut pas aller trop loin dans ce sens et il peut être utile de publier tous les cas mortels, afin d'être renseigné plus exactement sur les causes qui aggravent le pronostic et faire faire un pas en avant, si possible, à la thérapeutique de cette maladie.

Depuis mon établissement à Genève, c'est-à-dire, pendant 14 ans de pratique, sur un nombre très-considérable des pneumonies franches infantiles que j'ai soignées, je ne crois avoir eu que deux cas mortels. J'ai pu dans ces deux cas me rendre compte par l'autopsie de la cause prochaine de la mort. Je les rapporterai ici très brièvement.

Observation 3.—Pneumonie franche multé le bain droite terminé par gangrène—mort le onzième jour.

Cette observation a été déjà relatée en quelques mots dans notre manuel.†

Il s'agissait d'une fillette de trois ans et demi, d'une bonne santé habituelle. Le pronostic nous avait paru inquiétant dès le début à cause de l'élévation considérable de la température, de la prostration des forces et de l'extension rapide de l'inflammation du lobe supérieur au lobe inférieur.

La fièvre fut combattue par les bains tièdes, par la quinine et le salicylate de soude. Ce dernier médicament fut donné à la dose journalière moyenne d'un gramme. Au bout de deux jours, il avait déterminé une diminutiou notable de la fièvre. Ce jour-là, le cinquième de la maladie, nous trouvons dans l'observation les signes suivants: "Peau fraîche, l'enfant parait très-fatiguée. Figure pâle, légèrement bouffie." Le salicylate est supprimé le lendemain et remplacé par une portion tonique à l'extrait de quinquina et à la liqueur ammoniacale anisée.

La fièvre reprend de plus belle le soir du sixième jour et persiste jusqu'à la fin.

Le huitième jour, la fièvre et la dyspnie étant considérables, on ordonne des bains tièdes et un lavement de 0.50 de sulfate de quinine.

Le neuvième jour l'enfant prend un bain sinapisé et la potion au salicylate en alternant avec la potion excitante.

Le dixième jour, la fièvre a diminué, mais il n'y a pas d'amélioration réelle. L'angoisse respiratoire est grande. On se borne à prescrire un uni généreux à doses fréquemment répétées.

Le onzième jour, l'état va en empirant. Le pouls est extrêmement rapide et faible. L'enfant meurt dans l'après-midi sans avoir présenté à aucun moment de la fétidité ou de l'haleine et sans avoir expectoré. L'exploration physique avait seulement permis de constater une inflammation pneumonique du poumon droit qui s'était étendue du sommet à la base.

L'autopsie confirma le diagnostic de pneumonie franche, fibrineuse, mais elle révèla en outre, ce que je n'avais point soupçonné, la présence de trois infarctus cunéiformes gangréneux occupant la superficie du lobe supérieur. L'un d'entr'eux n'était pas encore ramolli et était nettement hémorragique; les autres étaient déja affectés, ramollis et excavès à l'intérieur et d'une couleur pis noirâtre sans fétidité aucune. Mon collégue M. le Professeur Lalen qui a bien voulu examiner la pièce, a constaté

* Loc. cit. p. 85.

† *D'Espine et Picot*, troisième edition, p. 653.

Digitized by Google

Digitized by Google

T. SINCLAIR & SON LITH PHILA

Disseminated centers of Pulmonary Gangrene
following true hepatization. Phlebitis of the Pulmonary Veins.

D'Espine.

Digitized by Google

une phlébite des veines pulmonaires dans les rameaux qui provenaient de ces foyers gangréneux. La paroi de la veine était jaunâtre; l'une d'elles contenait un thrombus adhérent, ramolli, qui pouvait être suivi jusque dans le foyer.

La figure ci-jointe peinte après la pièce représente le lobe supérieur incisé par le milieu au niveau des trois foyers de gangrène. On voit dans la partie gauche un thrombus adhérent et à droite la paroi vasculaire rouge et enflammée.

Observation 4.—Pneumonie franche terminée par hépatisation grise de tout le poumon droit. Mort le dix-huitième jour. Antopsie (résumée).

La jeune Marie A., âgée de 11 ans, entre dans mon service à l'Hôpital Cantonal le 18 Juin, 1885, au dixième jours d'une pneumonie que j'avais soignée en ville depuis le 11 Juin (troisième jour de la maladie). Un peu de délire les premiers jours avec fiévre trés élevée, oscillant entre 40.3° et 40.7° (R). J'avais constaté une pneumonie massive du lobe inférieur droit, qui s'étendit le dixième jour au lobe supérieur du même côté.

L'enfant fut traitée par les bains tièdes et les compresses de Priessnitz sur la poitrine, qui parurent les premiers jours amener une amélioration sensible. Le douzième jour, l'état subjectif était bon et la fièvre avait diminué. Mais elle reprit de plus belle le quatorzième jour, en même temps que la pneumonie s'étendait et devenait plus massive dans le lobe supérieur.

L'état général grave et les râles humides éclatants firent soupçonner un instant qu'il s'agissait d'une tuberculose aiguë à forme pneumonique. L'examen des crachats révéla la présence de nombreux pneumobacilles encapsulés de Friedländer, mais fut négatif pour les bacilles de Koch.

Dans les derniers jours, la fièvre fut très élevée, la dyspnie s'accompagna d'asphyxie et l'enfant succomba, malgré une médication au cognac et l'application de ventouses sèches, le 26 Juin (dix-huitième jour).

À l'autopsie, hépatisation complète de tout le poumon droit avec passage au troisième degré hépatisation prise. Epanchement modéré séro-purulent dans la cavité pleurale droite, contenant des flocons fibrineux et beaucoup de pneumobacilles encapsulés. Un peu de liquide séreux dans la cavité pleurale gauche. Le poumon gauche est acclimatié.

Dégénérescence graisseuse du foie et des reins (épithélium cortical).

Les autres cas de pneumonie franche infantile mortels, que j'ai observés ont été vus par moi seulement occasionellement, en consultation avec des confrères et n'ont pas été suivis d'autopsie. Quoiqu'on en soit réduit à des probabilités sur les causes de la mort, j'en donnerai un court résumé pour être complet.

Observation 5.—Enfant A., fillette de 16 mois, atteinte d'une pneumonie massive du sommet droit, avec hyperpyrexie. Vue en consultation avec son médecin le Dr. Solay, le 31 Janvier, 1786, le neuvième jour de la maladie.

L'enfant est dans le coma, les membres sont en résolution. Néanmoins, il n'y a pas de signes de méningite, c'est une pneumonie à forme cérébrale. L'enfant succomba dans la soirée.

Observation 6.—Alice M., belle fillette de deux ans, tomba malade le 9 Mars, 1882. Le Dr. Gœtz qui la soigne, a constaté une hépatisation massive du lobe inférieur gauche, qui s'est étendue le 16 Mars (sixième jour), au lobe supérieur du même côté. La température ce jour est montée à 41°.

Je la vois en consultation avec le Dr. Gœtz le 17 Mars, au soir (septième jours); Perte de connaissance; souffle énorme et matité absolue au sommet gauche; la sonorité commence à revenir à la base. Néanmoins, la température est encore élevée 40.2°, malgré plusieurs vésicatoires appliqués les jours précédents. Nous prescrivons des bains tièdes qui ramènent un peu de connaissance. Néanmoins, le lendemain, 18 Mars (huitième jour), la température était encore à 39.8° et l'enfant succomba dans la soirée.

Digitized by Google

Dans ces deux dernières observations, il ne semble pas y avoir eu de complication, et l'on ne peut attribuer la terminaison fatale qu'à l'intensité de la fièvre et de l'infection pneumonique. Dans ces deux cas l'infiltration a été massive et étendue à tout le poumon. Nous en tirons la conclusion, qu'il faut être réservé dans son pronostic, quand chez l'enfant on voit apparaître de bonne heure une matité considérable et un souffle tubeuré, surtout quand cette hépatisation massive s'étend à plusieurs lobes.

Nous résumons une dernière observation, dans laquelle la mort a été aussi peu *une embolie artérielle*, provenant probablement d'une thrombose cardiaque; nous n'avons jamais vu citer cette complication dans le cas de la pneumonie franche infantile.

Observation 7.—Anna L., deux ans. Bronchite et pneumonie du sommet droit qui a débuté vers le 9 Juin, 1886, et s'est étendue le 16 Juin à toute la partie postérieure du lobe inférieur droit.

Le 17 Juin (neuvième jour), l'amélioration est très sensible, la toux est devenue grasse. Tout fait espérer une convalescence prochaine, quand dans la nuit du 17 au 18, l'enfant est prise d'angoisse respiratoire et d'une grande agitation. Le Dr. Jeanneret qui est son médecin, constate le 18 au matin que la jambe et le pied droit sont froids, insensibles et couverts de taches bleues ecchymotiques.

Je vois l'enfant à midi en consultation avec le Dr. Jeanneret, nous constatons une action tumultuense, quoique régulière, du cœur, avec dilatation considérable des cavités droites, ainsi qu'une pneumonie massive avec souffle amphorique dans toute la partie postérieure du poumon droit, surtout dans le lobe inférieur. Nous formulons un pronostic absolument grave.

Le 20 Juin (douzième jour), les taches ecchymotiques sont plus nombreuses sur la jambe droite, qui est douloureuse au toucher. On constate aussi quelques ecchymoses moins nombreuses sur le pied gauche.

La fièvre continue et le poult est extrèmement fréquent.

Le 23 Juin (quinzième jour), le pied droit et la jambe droite jusqu'au milieu du du mollet sont froids et d'un bleu violet. Sur les orteils du pied gauche, il s'est formé des pustules qui ont conflué en phlyctènes jaunes opaques, se détachant sur un fond violet ecchymotique.

L'enfant est très affaiblie, tantôt somnolente, tantôt agitée. Les lèvres sont cyanosées. Elle meurt dans la soirée.

III. CONSIDÉRATIONS SUR LE TRAITEMENT DE LA PNEUMONIE FRANCHE.

On peut diviser les méthodes de traitement employées contre la pneumonie franche des enfants en méthodes nuisibles, inutiles et utiles.

(*a*) *Médications Nuisibles.*—Il ne faut pas oublier, avant d'ordonner une médication énergique contre la pneumonie franche, qu'il s'agit d'une maladie à forme cyclique, qui guérit dans l'immense majorité des cas par les seuls efforts de la nature.

Primum, Non-nocere.—Il n'est plus besoin aujourd'hui de faire les procès des *émissions sanguines* générales et de l'émétique; ils sont proscrits par tous les médecins d'enfants.

Il n'en est pas de même de la classe chaque jour plus nombreuse des *antipyrétiques internes* et je serais heureux si mon modeste travail donnait l'occasion aux sommités médicales de présenter dans le sein de la Section for Children Diseases de se prononcer au sujet de leur utilité dans la pneumonie.

Je mets à peu les sels de *quinine*, qui ne sont point dangereux à moins qu'on n'en hausse les doses et que j'ai employés occasionnellemnt avec avantage, soit en lavement, soit par la bouche, mais sans pouvoir leur attribuer dans le traitement de la pneumonie le même succès que dans celui de la fièvre typhoïde des enfants.

Quant au *salicylate de soude*, je l'ai abandonné, n'ayant pu m'empêcher de lui attri-

Digitized by Google

buer une influence hyposthénisante dans l'observation de gangrène pulmonaire que j'ai relatée plus haut (Obs. 3.).

L'antipyrine a un certain vague dans le traitement de la pneumonie infantile, qui je crois, est actuellement en voie de décroissance. Ce médicament, comme l'ont prouvé des expériences physiologiques récentes, a une action déprimante sur le cœur et je n'ai jamais pu me résoudre à l'employer à dose efficace contre la fièvre de la pneumonie franche.

L'antifibrine n'a pas tout-à-fait le même inconvénient, mais mon expérience n'est pas suffisante dans la pneumonie franche pour pouvoir me prononcer à son égard.

Quant à la *digitale*, dont l'usage est encore très répandu dans le traitement de la pneumonie, je ne puis m'expliquer son emploi. Quand on l'ordonne à faibles doses, ce médicament n'a aucune action appréciable sur la fièvre pneumonique et les accidents nerveux qui en dépendent ni sur la résolution de l'exsudat. Il a par contre souvent le grave inconvénient d'irriter l'estomac et de gêner l'alimentation, qui joue un rôle important en soutenant les forces jusqu'à la crise. À hautes doses, la digitale est dangereuse chez l'enfant.

(*b*) *Médications Inutile.*—Je range sous ce titre les *vésicatoires* qui sont sans action aucune sur la marche locale de la pneumonie et ont le grave inconvénient de rendre impossibles les bains et autres applications externes. Ils augmentent souvent l'agitation pendant la période fébrile et doivent être réservés pour les cas où l'induration du poumon persiste après la défervescence ou bien contre les complications pleurales et péricardiques.

(*c*) *Médications Utiles.*—Je n'en connais que deux, les *applications réfrigérantes externes* et les *excitants diffusibles* à l'intérieur.

Les premières peuvent être employées dès le début de la fièvre pneumonique sous la forme de *compresses de Priessnitz* sur le tronc et de *bains tièdes* administrés deux ou trois fois dans les 24 heures, au moment des exacerbations. Si elle ne suffisent pas pour ramener la température normale, elle combattent efficacement les symptômes nerveux de la fièvre, l'insomnie, l'agitation, le délise, l'adynamie et les convulsions. Nous avons pu constater, dans un cas de pneumonie cérébrale à forme typhoïde d'une extrême gravité chez une fillette de quatre ans, une amélioration passagère après chaque bain et la disparition définitive du coma après quelques jours de traitement.

Nous avons eu la conviction, que dans ce cas, nous avons dû le salut de notre malade au traitement balnéaire employé avec persévérance et associé à quelques lavements de quinine ; la fièvre tombe le septième jour.

Les *excitants diffusibles* sont d'un usage si répandu contre la faiblesse cardiaque dans la pneumonie, que nous n'avons pas besoin d'insister sur leur utilité. Nous mettons au premier rang les *alcooliques* et le *musc* associé au *benzoate d'ammoniaque.*

Nous n'avons pas d'expérience chez l'enfant sur les injections sous-cutanées d'éther qui rendent de si éminents services dans les pneumonies asthéniques d'un autre âge.

Digitized by Google

ÜBER DIE IM KINDESALTER VORKOMMENDEN GESCHWULSTBILDUNGEN, UND SPECIELL ÜBER MEDIASTINALTUMOREN NICHT CONGENITALER NATUR BEI KINDERN.

ON THE FORMATION OF TUMORS OCCURRING DURING INFANTILE LIFE, ESPECIALLY ON MEDIASTINAL TUMORS OF NON-CONGENITAL NATURE IN CHILDREN.

SUR LA FORMATION DES TUMEURS CHEZ LES ENFANTS, SPÉCIALEMENT, SUR LES TUMEURS MÉDIASTINES D'UNE NATURE NON-CONGÉNITALE.

VON PROFESSOR DR. OSCAR WYSS,

Dirig. Arzt des Kinderspitals zu Zürich.

Unter den seit Eröffnung des Züricher Kinderspitals, anno 1874, bis heute in diese Anstalt aufgenommenen 2768 Spitalspatienten fanden sich 16 mit Neubildungen im engern Sinne des Wortes, d. h. nach *Ausschluss* aller Angiome, ferner der auf tuberkulöser oder syphilitischer Basis beruhenden Geschwulstbildungen, aller Missbildungen oder mit solchen im Zusammenhange stehenden Geschwülste. Nach dem Geschlecht vertheilen sich diese Geschwülste so, dass neun dem männlichen, sieben dem weiblichen angehörten, und auf die verschiedenen Altersjahre vertheilen sich die Fälle in folgender Weise: Es kamen auf das

1.	Lebensjahr (0–1)	3 Fälle,	nämlich	1 Knabe,		2 Mädchen,	
2.	"	1 Fall,	"	1	"	0	"
3.	"	1 "	"	0	"	1	"
4.	"	1 "	"	1	"	0	"
5.	"	1 "	"	0	"	1	"
6.	"	2 Fälle,	"	2 Knaben,		0	"
8.	"	2 "	"	2	"	0	"
9.	"	1 Fall,	"	1 Knabe,		0	"
12.	"	1 "	"	0	"	1	"
14.	"	2 Fälle,	"	0	"	2	"
15.	"	1 Fall,	"	1	"	0	"
		Summa.....		9 Knaben,		7 Mädchen.	

Auf die verschiedenen Körperorgane vertheilen sich diese Neoplasmen in folgender Weise: Es war der Sitz der Geschwulstbildung zweimal im Gehirn, zweimal im Darm, zweimal im Oberschenkelknoten, zweimal im Ovarium, je einmal im Mediastinum anticum, im Beckenknoten, Pharyngolaryngealraum, im Muskel, Axilla, subutanen Bindegewebe und Lymphdrüsen. Der anatomischen Structur nach handelte es sich um Sarkome, Lymphosarkome, Osteosarkome, Kystosarkome, Fibrosarkome, Myxosarkome und Kystome. Zu diesen Geschwulstbildungen fügen wir kurz noch einige, die wir ausserhalb des Kinderspitals zumeist in der Consultativ-Praxis zu sehen Gelegenheit hatten. Es handelte sich um zwei Fälle von diffuser sarkomatöser Degeneration des Peritoneums; um zwei Fälle von primärem Lebersarkom, und um einen Fall von Sarkom am Schädel in der Stirngegend, wohl ausgegangen vom Periost.

Unter allen diesen Geschwulstbildungen, die wir bei Kindern beobachtet haben, bot mit das meiste diagnostische Interesse der von uns anno 1882 beobachtete und diagnosticirte Fall von *Mediastinaltumor*, theils wegen seiner grossen Seltenheit, theils auch der klinischen Symptome wegen. Es handelte sich um einen vierzehnjährigen Knaben, Fritz M., der wegen "Compression der Trachea durch Struma" in's Züricher Kinderspital geschickt wurde und nach Kurzem auch wegen Erstickungsanfällen tracheotomirt werden musste. Da in der That eine erheblich vergrösserte Glandula thyreoidea vorhanden war, schien jene Diagnose plausibel. Aber nachdem die Trachealkanüle

Digitized by Google

eine kurze Zeit gelegen hatte, kehrte die Dyspnoe, kehrten die Symptome der Tracheostenosis wieder; eine längere König'sche Kanüle beseitigte die Dyspnoe. Aber wiederum nur für eine kurze Zeit lang; dann musste wieder eine längere, tiefer in die Trachea hinabreichende Kanüle eingelegt werden, um die Respiration wieder frei zu machen. Die Tracheostenose, die also anfänglich etwa in der Mitte der Trachea Statt hatte, rückte allmählig weiter nach unten, an eine Stelle, wo allem Anscheine nach die Struma nicht mehr auf die Trachea einen Druck ausüben konnte. Um diese Zeit traten auch anderweitige neue Symptome auf: die Lungenränder wurden von oben hinter dem Sternum auseinander gedrängt; es entwickelte sich eine zwischen Jugulum Sterni und oberer Herzgrenze befindliche, oben breitere und nach unten etwas schmäler werdende, die obere Hälfte des Sternums und die angrenzenden Partien des Thorax einnehmende Dämpfung des Percussionsschalles. Dann kamen neuralgiforme Schmerzen in der linken Schulter und im linken Arm, Lymphdrüsenanschwellungen über und unter der linken Clavicula. Es bildeten sich sodann Oedeme und Cyanose im Gesicht aus, Oedeme des linken Armes, Venenectasien, namentlich in der Gegend des Brustbeins: Symptome von Compression der Vena cava, während am Herzen, abgesehen von etwas pericarditischem Reiben im Beginn, das aber späterhin vollständig verschwand, und auf den Lungen, ausser etwas catarrhalischen Geräuschen, keine Anomalie sich nachweisen liess. Es bildete sich späterhin, nachdem schon vorher Schlingbeschwerden bestanden hatten, gegen das obere Ende des Sternums eine Verwölbung der obern Hälfte des Brustbeins in seiner Nachbarschaft aus, die Dyspnoe nahm wieder zu, der Patient musste beständig sitzen, und er starb nach neun Monate dauernder Krankheit. Die Section ergab einen kindskopfgrossen, von der Thymus ausgehenden Tumor mediastini mit Compression der Trachea in grosser Ausdehnung; etwa die untere Hälfte war comprimirt; Compression der grossen Venen, zumal der Vena cava superior, der Speiseröhre. Die Neubildung hatte übergegriffen auf die Bronchealen und peritrachealen Lymphdrüsen, auf die Drüsen unter und über der linken Clavicula; sie setzte sich auch auf den Herzbeutel fort, hatte diesen an seiner Basis infiltrirt und zudem eine hämorrhagische, eitrig fibrinöse Pericarditis herbeigeführt. In beiden Pleurahöhlen etwas Hydrothorax mit nachfolgender Lungencompression.

In der *Literatur* haben wir nur eine geringe Zahl ähnlicher Fälle von Mediastinaltumor, bei Kindern beobachtet, verzeichnet gefunden. Unter den zwischen den Jahren 1742 und 1870 publicirten Fällen fand sich nach Riegel keiner aus dem Kindesalter. Es sind bis zur Zeit nur etwa neun oder zehn solcher Fälle beschrieben, und die Mehrzahl derselben nicht so eingehend, dass man ein klinisches Bild aller dieser Fälle zusammenfassen kann. Wir sehen hierbei gänzlich ab von jenen *congenitalen Thymusgeschwülsten* (resp. *Hypertropien der Thymus*), über die vor Kurzem Dr. G. Somma in Neapel im *Archivo di Pathologia infantile*, anno II, fasc. 4, 1884, eine vortreffliche Abhandlung schrieb. Diese bilden eine besondere Krankheitsgruppe für sich, verschieden von der in Rede stehenden Affektion.

Die neun genauere Angaben bietenden Fälle, incl. der unsrige, lassen Folgendes über die *Mediastinaltumoren der Kinder, die von der Thymus ihren Ausgang nehmen*, feststellen. Bezüglich *Aetiologie*. Unter 9 Patienten befanden sich 7 Knaben und bloss 2 Mädchen. Auf die Altersjahre kommen: ein Fall auf das 1. Lebensjahr; auf das 6. zwei Fälle, auf das 7. ein Fall, auf das 9. zwei Fälle, auf das 12., 13. und 15. Jahr je ein Fall. Es sind also die ersten fünf Lebensjahre viel seltener von dieser Krankheit heimgesucht, als die zwei folgenden Lustren. Andere ätiologische Momente waren keine zu eruiren. Dass aber möglicherweise auch bei der Aetiologie der Neubildungen des Kindesalters die Heredität eine Rolle spielt, das deutet die Erfahrung an, die wir neulich machten und die darin bestand, dass bei *zwei Kindern, die Geschwisterkinder waren*, maligne Unterleibsgeschwülste vorkamen.

Pathologische Anatomie. Ihrer anatomischen, resp. histologischen Beschaffenheit

Digitized by Google

nach sind die Mediastinalgeschwülste der Kinder nicht wie beim Erwachsenen meistens Carcinome, sondern *Lymphosarkome.* Einzig Prof. Vogel in Dorpat hat einen seiner Fälle als Carcinom taxirt, zwar ohne genauere Details anzugeben. Nur in ganz wenigen Fällen liegen genauere Angaben über die histologischen Verhältnisse vor; so von Grützner und von Steudener. Die von uns untersuchte Geschwulst zeigte ein an verschiedenen Stellen des Tumors verschieden mächtiges, bindegewebiges Stützgewebe von alveolarem Bau. In den festeren, derberen Theilen der Geschwulst war dieses Bindegewebsgerüste dicker, derber, reichlicher, zeigte lockiges, welliges Bindegewebe, auch glänzende, festere, aus elastischen Fasern, sowie nach den Rändern der Geschwulst hin vielfache andere, im Zustande der Atrophie befindliche Gewebspartien (Muskelnfasern, Fettgewebe etc.), eingeschlossen. Manchenorts waren in dasselbe auch Rundzellen eingelagert, theils isolirte, theils in Reihen angeordnete, theils gruppenweise zusammengelagerte. Wo die Geschwulst von weicherer, markschwammartiger Beschaffenheit war, war das Bindegewebsgerüste sparsam, zarter, die Maschen grösser und die Septa sehr dünn; aber doch überall deutlich nachweisbar, auch mit Farbstoffen imbibirbar. In den Alveolen, den kleinen wie den grösseren, lagen, dicht nebeneinander gelagert, anscheinend ohne Kittsubstanz, doch leicht zusammenhängende Rundzellen von der Grösse etwa der weissen Blutkörperchen. An einigen Stellen (wohl bei gewisser Richtung der Schnittführung) war die Dimension der Alveolen so sehr gross, dass dieselben als schlauchähnliche oder drüsenähnliche, mit Rundzellen ausgestopfte Gebilde neben einander gelagert erschienen. Der Bau der Geschwulst war somit derjenige eines *Lymphosarkoms.* Unzweifelhaft haben alle diese Tumoren der Thymus (resp. des Mediastinum anticum des Kindes) einen ähnlichen Bau; und wenn auch die Lymphdrüsen des Lungenhylus, diejenigen an der Bifurcation, an der Trachea durch tuberkulöse oder andere Entartung zu Geschwulstbildung Veranlassung geben können, so müssen doch bei diesen sowohl die anatomischen als auch die klinischen Erscheinungen ganz andere sein. Freilich gibt es ja auch noch andere pathologische Vorgänge in der Thymus, die klinische Symptome machen können: die Hypertrophie (vgl. die Arbeit von Somma), Abscesse etc.; aber diese sind in gewisser Beziehung doch different von denen bei unserer Affektion. Wir ziehen deshalb diese Thymusaffektionen nicht in den Kreis unserer Betrachtung, sondern halten uns an die Symptome des Lymphosarkoma thymicum infantum.

Symptomatologie. Die Mediastinalgeschwülste, die bei Kindern beobachtet wurden, machten sämmtlich in der ersten Zeit ihrer Entwicklung keine Symptome. Ja, einmal wurde eine solche Geschwulst zufällig bei einer Section eines an Pneumonie gestorbenen Kindes beobachtet (von Steudener), ohne dass intra vitam darauf bezügliche Erscheinungen beobachtet worden wären. Ist jedoch das Wachsthum der Geschwulst bis zu einer gewissen Grösse gediehen, so übt sie auf die Nachbarorgane einen Druck aus und es stellen sich Erscheinungen ein, 1. Seitens des *Respirationsapparates,* und 2. Seitens desjenigen der *Circulation.* Die *Respiration* wird behindert, beengt; die Athemzüge werden hörbar. Beim Laufen oder Springen, beim Abliegen im Bett steigern sich diese Erscheinungen; erst vorübergehend, später bleibend, werden die Athemzüge, In- und Exspiration, doch namentlich die erstere, verlängert, keuchend und pfeifend. Die Kranken müssen eine ganz bestimmte aufrechte Position einnehmen; nicht zu weit nach vorn übergebeugt, nicht zu stark nach hinten; und oft auch in einer bestimmten Richtung im Sinne von Rechts nach Links oder mit einer bestimmten leichten Rotation der Wirbelsäule: offenbar so, dass die Stellung der comprimirten Trachea so ist, dass ihr Lumen noch so weit als möglich offen erhalten wird durch die entsprechende Körperposition. Besonders in der Nacht treten heftigere Attaquen von Dyspnoe bis Orthopnoe auf. Bei dem von uns beobachteten Patienten steigerte sich dieser qualvolle Zustand so, dass man sich eines Tages genöthigt sah, die Tracheotomie vorzunehmen (ex indicat. vitali) und eine König'sche Kanüle einzulegen. Dies hatte günstigen

Digitized by Google

Erfolg, aber nach einiger Zeit kehrte die Dyspnoe wieder und konnte nur dadurch gehoben werden, dass man eine noch längere, d. h. noch weiter in die Trachea hinabreichende Kanüle einlegte. Offenbar rückte die Verengerung der Trachea, die zuerst höher oben ihren Sitz hatte, weiter nach unten. In keinem der in der Literatur verzeichneten Fälle von Mediastinaltumor bei Kindern musste die Tracheotomie, die bei unserem Kranken mit Erfolg in Anwendung kam, gemacht werden; dass bei Neugeborenen mit Hypertrophie der Thymus, Tracheostenose und Erstickungstod vorkommt, ist zwar längst bekannt, neuerdings aber durch Somma's zwei sehr werthvolle Beobachtungen wiederum constatirt. Dass bei Erwachsenen mit Mediastinaltumor zwar Compression der Trachea vorkommt, jedoch sehr selten, hat Riegel ("Krankheiten der Trachea und der Bronchien," in *Ziemssen's Pathologie und Therapie*, p. 201) hervorgehoben. Husten, Tracheabronchialcatarrh, auch durch objective Symptome, Rhonchi etc. nachweisbar, kommt zu einer gewissen Zeit des Leidens begreiflicherweise vor. Auch Hämoptoe; zwar in unserem Falle wegen der vorausgegangenen Tracheotomie und geringen Quantität des entleerten Blutes ohne Bedeutung; in einem andern Falle aber spontan und reichlicher: wohl doch mit der beträchtlichen venösen Stauung im Zusammenhang. Ferner wurde bei unserem Kranken hinten, rechts oben, in der Fossa supraspinata lautes Bronchialathmen in grosser Ausdehnung constatirt: ein Symptom, das wir gleichfalls auf den, die Lunge nach aussen verdrängenden und nach hinten gegen die Wirbelsäule hin wachsenden Tumor und die Drüsendegeneration der über dem rechten Bronchus und an der Bifurcation liegenden Lymphdrüsen beziehen müssen. Hydrothorax und Compression der unteren Lungenpartien durch letztere, wiederholt beobachtet, waren Terminalalterationen.

Von den Störungen von Seiten des *Circulationsapparates* sind in erster Linie die *Oedeme* im Gesicht, sowie in den oberen Extremitäten hervorzuheben. Sie sind entweder nur auf der einen Körperhälfte oder, wenn beiderseitig, auf der einen Seite stärker vorhanden. Es ist nicht immer die gleiche Seite stärker afficirt; in dem von Gützner (1869 Berlin, Diss.) mitgetheilten Falle waren die Oedeme auf der rechten Seite, bei unserem Patienten stärker links, im Gesicht sowohl, als auch später am Arm und der linken Hand, und ähnlich in den Fällen von Gamgee und Vogel. Die Oedeme waren bei unserem Patienten jeweilen stärker am Morgen und nahmen im Laufe des Tages ab. Auffälliger noch als die Oedeme waren *Venenectasien*, theils auf der Brust, wie sie namentlich in der eben citirten Beobachtung von Grützner und in unserem Falle, theils aber auch an Hals, Gesicht und Arm auf der einen Seite stärker sich bemerkbar machten. Und ebenso die *Cyanose des Gesichtes*, der Lippen und Schleimhäute sowohl, wie der äussern Haut. Am Herzen constatirte Vogel bei dem einen seiner Kranken ein *systolisches Blasen;* bei unserm Patienten war im Beginne der Beobachtung ein *pericarditisches Reibegeräusch* vorhanden, das später verschwand. Sonst war am Herzen in den übrigen Fällen nichts Abnormes constatirt worden.

Einmal, von Dr. Gamgee in Edinburgh, war das *Blut microscopisch* untersucht worden und eine *Vermehrung der weissen Blutkörperchen* gefunden worden. Gleichzeitig bestanden in diesem Falle an den unteren Extremitäten *Petechien*, die bei keinem andern Patienten notirt worden sind.

Störungen im Bereiche des *Nervensystems* wurden wiederholt gesehen. In unserem Falle bestanden *neuralgische Schmerzen* in der linken Schulter, ausstrahlend in den Arm und in die Hand. Wir führten sie auf die secundären Lymphdrüsentumoren über und unter der linken Clavicula zurück. Wahrscheinlich waren diese auch der Grund, warum bei unserem Kranken die Oedeme auf der linken Gesichtshälfte und im linken Arm so beträchtlich waren. Grützner beobachtete analoge neuralgische Schmerzen wie wir.

Der *Verdauungsapparat* bot in ein paar Fällen bestimmte Störungen. In Folge directen Druckes der Geschwulst auf die Speiseröhre kamen *Schlingbeschwerden* vor, bei

Digitized by Google

Grützner's Kranken und bei dem unsrigen, so dass feste Dinge gar nicht mehr genossen werden konnten. In einem Falle bestanden zudem noch heftige Schmerzen im Gaumen. Ob Recurrenslähmungen bei Kindern mit Mediastinaltumoren vorkamen, wissen wir leider nicht, da in keinem der Fälle die laryngoscopische Untersuchung vorgenommen wurde.

Die *Vorwölbung des Sternums* an der Stelle des Tumors wurde wiederholt beobachtet und ist natürlich ein äusserst wichtiges Symptom. Sie entwickelt sich langsam und spät; ist schmerzhaft, anfänglich ohne Veränderung der Haut, später mit ausgedehnten Venen, auch eventuell geröthet, aber ohne Pulsation und ohne Fluctuation.

An den *Abdominalorganen* wurde meist wenig oder nichts Abnormes beobachtet. Einige Male Ascites; bei unserem Kranken war der untere Leberrand, mehr in Folge venöser Hyperämie der Leber als in Folge des Hinabgedrängtseins durch Tumor und pericarditisches Exsudat, nach unten verdrängt. Die *Milz* war geschwellt.

Abmagerung bestand meistens in Folge der Kachexie; auch terminale Oedeme an den unteren Extremitäten. Nur einmal, von Steudener, wurde bei der Section der Ernährungszustand noch als ein guter notirt.

Dauer der Krankheit. Prognose. Die Krankheitsdauer genau zu bestimmen, ist geradezu unmöglich, weil sie eine Zeit lang bestehen kann, ohne Symptome zu machen. Die am längsten dauernde Krankheitsdauer von über zwei Jahren wurde von Dr. Rosenberg in Göttingen notirt; doch wurde diese Beobachtung nicht usque ad finem durchgeführt. Zweimal war die Krankheitsdauer nur wenige Wochen (Vogel); zweimal 4 Monate, einmal 5½ Monate und einmal ca. ¾ Jahre. Die *Prognose* ist nach unserem heutigen Wissen immer eine infauste, lethale.

Diagnose. Die Diagnose wird sich auf die oben geschilderten Symptome zu stützen haben: den langsamen fieberlosen Verlauf; die Symptome von allmählig zunehmendem Druck auf die Trachea, die grossen Gefässe, zumal die Vena cava superior, später auf den Oesophagus und das Sternum; die Verschiebung der Lungenränder, die eigenthümliche Dämpfungsform der Geschwülste im Mediastinum anticum. Von Wichtigkeit für die Diagnose eines *malignen* Tumors ist sodann der Nachweis der secundären Lymphdrüsenerkrankungen. Gegen die einfache Hypertrophie der Thymus und die damit im Zusammenhange stehenden Respirationsbeschwerden wird das Alter des Patienten sehr in's Gewicht fallen: jene tritt in einer viel früheren Zeit des Lebens auf, innerhalb des ersten Lebensjahres, als die bei uns in Rede stehende Affektion. Gegen Abscesse in der Thymus, syphilitische Vorgänge daselbst: die Anamnese, wie auch der objective Befund. Gegen anders woher stammende Abscesse im Mediastinum, Congestionsabscesse von der Wirbelsäule her, von Lymphdrüsenvereiterung her: die genaue Berücksichtigung der Temperaturverhältnisse, sowie der objective Thatbestand; und ebenso dürfte die Diagnose von tuberculösen Drüsenpaqueten an der Bifurcation und im Lungenhylus, um die grossen Bronchien herum keine Schwierigkeit haben, da auch hier Temperaturverlauf, Anamnese und objectiver Befund entscheidende Anhaltspunkte geben werden.

Therapie. Ist die Diagnose auf Lymphosarkoma thymicum gestellt, so wird die Therapie allerdings kaum mehr grossen Erfolg aufzuweisen haben. An eine operative Beseitigung der Geschwulst ist wohl bei der grossen Nähe der wichtigsten Organe: Herzbeutel, Herz, grosse Gefässe, Pleuren, kaum zu denken; immerhin, sofern die Geschwulst blos auf die Thymusdrüse noch beschränkt wäre, nicht absolut undenkbar. Aber da ist wohl die Diagnose noch nicht möglich oder noch unsicher! Von internen Mitteln wird in erster Linie *Arsen* in vorsichtig, langsam, allmählig zu steigender Dosis und in seinen Wirkungen streng überwacht, zu empfehlen sein. Wir ordiniren ihn bei Kindern selten anders als so: Solutionis arsenicalis Fowleri 5. Aquä Cinnamomi 10. M. Detur ad Vitr. Compte gouttes. Dt. Drei Tage lang Morgens, Mittags, Abends, ½ Stunde nach der Mahlzeit einen Tropfen auf einem Stückchen Zucker zu

Digitized by Google

nehmen. Am 4., 5. und 6. Tage Morgens 1, Mittags 2, Abends 1 Tropfen. Am 7., 8. und 9. Tage Morgens 1, Mittags 2, Abends 2 Tropfen zu nehmen. Am 10., 11. und 12. Tage Morgens 2, Mittags 2, Abends 2 Tropfen zu nehmen. Am 13., 14. und 15. Tage Morgens 2, Mittags 3, Abends 2. Am 16., 17. und 18. Tage 2, 3, 3, und so wird langsam und sehr allmählig jeweilen erst Mittags, dann Abends, dann zuletzt Morgens die Tagesdosis jeweilen nach drei Tagen um einen Tropfen gesteigert, und kann im Laufe der Zeit bis auf 15 Tropfen dreimal per Tag gestiegen werden. Das Aussetzen der Tropfen für einige Tage wird angeordnet, wenn Digestionsstörungen sich einstellen; das Aufhören dieser Therapie darf nicht plötzlich geschehen, sondern muss langsam und allmählig stattfinden. *Jodkali*, *Jodeisen* wird weniger wirksam sein.

Unter bestimmten Indicationen, bei Suffocationsgefahr durch Compression der Trachea, wird die *Tracheotomie* und das Einlegen einer König'schen Kanüle indicirt sein, und dadurch dem Patienten sein Dasein bedeutend erleichtert, sein Leben, wie in unserem Falle, um viele (6) Wochen verlängert werden können. Wir bezweifeln, dass hier die Tubage an die Stelle der Tracheotomie treten kann: die grosse Länge und relativ beträchtliche Enge des Rohres würde kaum die Respiration lange ungehindert gestatten. Dass Narkotica nicht immer werden vermieden werden können, begreift sich aus der Schilderung der Symptome und des Krankheitsverlaufs, und zwar glauben wir hier unter Umständen nicht blos Morphium, oder Opium, Chloral Hydrat etc., sondern wegen der oft momentan heftigen Dyspnoe auch auf kurze Inhalationen mit kleinen Mengen von Aether, Chloroform oder anderen ähnlichen Präparaten hinweisen zu wollen. Eine zweckmässige Lagerung, so dass die Kranken mit fast aufrechtem Körper auch schlafen können, ist für diese armen Patienten mit eine grosse Wohlthat.

Und was resultirt aus dieser Betrachtung? Dass es gewisse unheilbare Krankheiten bei Kindern gibt? Allerdings. Aber sie lehrt uns auch, dass offenbar diese äusserst seltenen Erkrankungen dennoch nach einem gewissen Typus verlaufen, dass ihnen ein bestimmtes Krankheitsbild zukommt, das wir kennen müssen. Sie lehrt uns, dass dieses Krankheitsbild ein anderes ist beim Erwachsenen, ein anderes im kindlichen Alter. Und wenn auch viele Symptome die gleichen oder sehr ähnliche sind, so sind andere wiederum bedeutend abweichend und für Den, der die Sachlage nicht ganz genau kennt, irreführend. Sie beweist von Neuem, dass die Pädiatrie eine ihrer Existenz berechtigte Tochter der Medicin im engern Sinne des Wortes ist; dass es aber auch unsere Pflicht ist, mit allem Fleiss und aller Energie dies Terrain zu bebauen, das zwar bereits manche schwere Errungenschaft aufweist, aber doch noch unendlich viele Lücken besitzt. Daher auch von Alt-Europa her der Wunsch den Herren Collegen jenseits des Atlantischen Oceans: möge dort die Pädiatrie eine glückliche und für das Menschengeschlecht segensreiche Entfaltung gewinnen und ihr manche Heimstätte wissenschaftlichen Ringens und Strebens gegen Krankheit und Leiden der lieben Kinderwelt beschieden sein!

Digitized by Google

SUR LE TRAITEMENT DE LA DIPHTHÉRIE PAR L'ESSENCE DE TÉRÉBENTHINE ET LE SULFURE DE CALCIUM, DANS UNE EPIDÉMIE EN L'ANNEÉ 1885, CAS NOMBREUX DE GUÉRISON.

ON THE TREATMENT OF DIPHTHERIA BY OIL OF TURPENTINE AND SULPHITE OF CALCIUM, IN AN EPIDEMIC OF THE YEAR 1885; NUMEROUS CASES OF CURE.

ÜBER DIE BEHANDLUNG DER DIPHTHERIE DURCH TERPENTINÖL UND SCHWEFELCALCIUM, IN EINER EPIDEMIE DES JAHRES 1885; ZAHLREICHE FÄLLE VON HEILUNG.

PAR LE DOCTEUR ÉMILE POUSSIÉ,

De Paris, France.

J'ai l'honneur de faire une communication sur une épidémie de diphthérie que nous avons observée le Dr. Michaut Alfred, de Blandy, et moi dans l'arrondissement de Melun, Seine et Marne, France.

Une jeune femme de Paris dont les parents habitent Moisenay a été prise de diphthérie dont elle est morte. Le 4 Août les parents rapportent son linge à la campagne et ramènent sa petite fille agée, de 10 mois. 4 jours après c'est-à-dire le 8 Août, 1885, le père la mére et une jeune fille de 13 ans, sœur de la morté, sont pris eux mémes d'angine diphthéritique. Traitement: pilules de térébenthine, 5 centigrammes, 8 par jour, gargarisme phénique 2%, toucher les plaques diphthéritiques avec solution acide citrique $\frac{1}{20}$. Le mieux au bout de 5 jours. Alimentation substantielle tonique, vin généreux, alcool. Le 16 Août derniére visite; les 3 malades sont guéries.

Voyant ces personnes atteintes de diphthérie j'ài fait renvoyer le 9 Août à Saint Germain, à une lieue de là, l'enfant de 10 mois chez des parents qui ne sont jamais entrés dans la famille contaminée. Dans cette maison il y a 6 enfants dont 2 jumelles de 8 ans ont eté prises de diphthérie le 13 Août, c'est-à-dire 3 jours après l'arrivée de l'enfant dans la famille, preuve que la maladie a été amenée par cette enfant qui n'étant pas contaminée elle méme n'apu transporter les germes que par ses vêtements. Le même traitement est institué pour ces 2 nouvelles malades qui guérissent l'une bien, l'autre est atteinte de leucocythémie, de paralysie du voile du palais et même du coté du pneumogastrique, car elle présentait de l'orthopneé, de l'arythmie du cœur et de plus de l'albuminurie. Après avoir institué un traitement tonique cette enfant finit par guérir au bout d' 1 mois et demi.

Trois jours après que ces 2 petites filles tombaient malades, c'est-à-dire le 16 Août, un garçon de la famille, âgé de 12 ans, était pris á son tour et était guéri au bout de 8 jours mais il lui est resté une paralysie du voile du palais et une paralysie presque complète du nerf optique pendant 6 jemaines. 4 jours après, c'est-à-dire le 20 Août, son frére âgé de 6 ans était atteint de diphthérie, mais rebelle à toute espèce de traitement succombait le 24 Août.

Une amie de la première malade en contact continuel avec elle est tombée également malade le 9 Août atteinte de diphthérie dont elle guérit au bout de 8 jours, même traitement.

Nous avons eu en quelques mois 54 cas de diphthérie dont 48 malades dans la même commune. Aussi l'école dut-elle étre fermée pour circonscrire cette épidémie et empecher la propagation dans le voisinage. Sur ces 54 malades nous avons obtenu 46 guérisons grâce croyons-nous à notre traitement. Beaucoup d'enfants eurent des paralysies du voile du palais; 5 enfants eurent en plus de la Surdité qui chez un garçon de 12 ans dura plus de 6 semaines. 3 enfants eurent un écoulement sanieux du nez,

Digitized by Google

dont une fille ayant de fausses membranes énormes. Enfin un garçon de 10 ans eut de la paralysie du nerf optique et une fille de 8 ans de la leucocythémie, Paralysie du voile du palais, albuminurie arythmie pendant 6 semaines. L'épidémie n'était donc pas bénigne. Le traitement que nous avons institué fut: Traitement externe, injection nasale, eau phéniquée à 2 pour cent. 4 fois par jour.

Toucher les fausses membranes avec l'acide citrique au $\frac{1}{10}$ toutes les 4 heures.

Gargarisme, eau phéniquée 2% Si le gargarisme était impossible lavage au pinceau trempée dans la solution d'acide phénique.

Traitement interne. Adultes. Essence de Térébenthine, Capsules gélatineuses de 5 Centigram et même de 10 centigr.—8 caps. par jour en 4 fois, nos 9 adultes malades supportèrent très bien ce traitement. Enfants. Chez eux nous avons toujours commencé par l'essence de térébenthine, en émulsion 30 centigrammes en émulsion dans jaune d'œuf No. 1. Sirop simple et eau de fleurs d'oranges 120 gr. par cuillereés à bouche toutes les heures.

Si les enfants éprouvaient de la répulsion ou de la fatigue d'estomac nous donnions des pilules de Sulfure de Calcium de 1 centigramme No. 20 toutes les 3 heures 2 ou 3 pil. Toutes les habitations ont été laveés a l'hypochlorite de chaux, les murs et les escaliers avec un lait de chaux.

Ses linges ont été trempés dans l'eau de Potasse et les crachats ont été brûlés, jetés au feu.

Grâce au traitement institué assez facile a faire suivre aux enfants à partir de l'âge de 2 ans, nous n'avons eu que 8 morts. Toutefois je ferai observer que ces cas se décomposent ainsi.

3 enfants complètement rebelles à tout traitement.

2 enfants morts de paralysie intestinale survenue chez l'un au 9e jour, alorsque tous les symptômes diphthéritiques étaient notablement amendés, et chez une autre environ 8 jours après disparition des plaques.

2 enfants étaient âgés de 8 jours, on sait toute la gravité de la diphtherie à cet âge.

Enfin une fille de 8 ans était mourante à notre arrivée "Cette épidémie était caractérisée par le manque des symptômes habituels de strangulation du croup à pas un des enfants morts, à l'exception de ceux rebelles à tout traitement, au nombre de 3, n'est mort par obstruction des voies respiratoires. La plupart de ces enfants sont forts mais lymphatiques, pas toujours très bien nourris.

La relation de cette épidémie nous a paru intéressante à noter en ce que l'étiologie est nettement démontreé par le transfert d'habits contaminées et de plus le traitement institué par le Dr. Michaut et moi nous a donné des résultats tels que nous croyons devoir les soumettre à votre appréciation dans ces grandes assises de la science tenues en ce pays delibre discussion.

DE L'HÉRÉDO-SYPHILIS ET DU RACHITISME AU BRESIL.

ON HEREDITARY SYPHILIS AND RACHITIS IN BRAZIL.

ÜBER DIE HEREDITÄRE SYPHILIS UND RACHITIS IN BRASILIEN.

PAR LE DOCTEUR MONCORVO.

Professeur de Clinique des Maladies de l'Enfance à la Policlinique de Rio de Janeiro.

La syphilis fut avec toute probabilité introduite au Brésil par les premiers portugais qui sont venu l'habiter. La majorité de ces colons était représentée par des individus tirés des prisons ainsi que par d'autres condamnés au bannissement en ces lointains parages. Or, rien n'admet à croire qu'aucune mesure prophylactique fût prise depuis

Digitized by Google

dans le but de restreindre en quelque sorte la dissémination de la syphilis et sa transmission par la voie de l'hérédité. Même plus tard, tant sous le domaine colonial qu'après la fondation de l'Empire Brésilien (1822) jusqu'à l'époque actuelle, aucun règlement de la prostitution ne fut décrété. Enfin pas une seule mesure hygiénique ne fut encore prise au Brésil dans le sens de mettre obstacle aux ravages de la syphilis.

Dans de telles conditions tout doit porter à croire au prime abord à la fréquence exagérée de l'hérédo-syphilis au Brésil. En me bornant à mon observation faite à Rio de Janeiro et à la province du même nom, je me crois autorisé à affirmer que dans le tableau de la pathologie infantile la syphilis héritée figure au chiffre de 60 pour 100. Elle contribue puissamment à la déchéance physique, chaque fois plus accusée, de la majorité de la population brésilienne. Après l'impaludisme aigu ou chronique, c'est la syphilis héréditaire qui représente le rôle le plus important au milieu des causes de la mortalité de l'enfance, soit directement soit par la gravité qu'elle imprime à presque toutes les maladies qui affectent les jeunes sujets. Enfin elle constitue, à mon avis, le plus grand facteur qui entrave l'accroissement de la population au Brésil, soit par la mortalité des enfants, soit par les fausses-couches et les morts-nés dont le nombre prodigieux est démontré par les statistiques.

Aucune race n'est plus que l'autre à l'abri de la syphilis au Brésil. Toutes les manifestations caractéristiques de l'hérédo-syphilis soit récentes, soit tardives y sont sous tous les points de vue identiques à celles décrites par les auteurs européens et américains du nord.

Le rachitisme se montre dans mes statistiques à la proportion de 45 pour 100, sur les enfants malades que j'ai eus sous mon observation. Cela montre l'improcédence de l'assertion de l'illustre Prof. Ch. West, au Congrès de Londres en 1881, lorsqu'il affirma que, tandis que la syphilis était fréquente au Brésil, le rachitisme par contre y était inconnu. Cette affection revêt au Brésil les mêmes formes que celles observées par les auteurs anglais, français, allemands et américains du nord.

Plus de deux tiers des rachitiques que j'ai observés tant à Rio qu'aux différents endroits de la province du même nom, portaient les signes actuels ou anciens de l'hérédo-syphilis. D'autre part il est assez rare de trouver un enfant hérédo-syphilitique qui ne présente pas de la déformation rachitique des os, encore même qu'elle ne soit que trop légère.

Le rachitisme au Brésil n'est point le privilége des enfants des classes pauvres; rien au contraire plus commun que de le retrouver chez ceux appartenants aux familles du rang même le plus élevé.

Un examen approfondi des conditions étiologiques du rachitisme au Brésil, m'a fait voir que l'alimentation ne joue qu'un rôle secondaire pour sa production. Il est fort commun d'y voir un enfant sujet dès le début au régime le plus irrégulier sans la moindre trace de rachitisme, tandis que d'autres soumis à l'allaitement maternel, dans les meilleures conditions possibles, deviennent rachitiques.

Sans la moindre prétention de vouloir trancher la question encore si litigieuse de l'étiologie du rachitisme, basée sur une observation déjà longue et attentive sur près de trois mille enfants rachitiques, je m'incline à voir dans l'hérédo-syphilis, sinon la cause invariable de cette affection osseuse, du moins celle qui contribue à la majorité des cas à sa production.

Dans le traitement de la syphilis héréditaire précoce je donne la préférence à l'emploi de la liqueur de Van-Swieten et aux frictions avec l'onguent napolitain. Chez les enfants plus âgés je m'addresse au sirop de Gibert, au protoiodure et au tannate d'hydrargyre. Les iodures de potassium ou de sodium s'ajoutent avec le plus grand profit à ces derniers notamment dans les cas de complications de lésions viscérales.

Pour ce qui concerne la thérapeutique spéciale du rachitisme j'ai essayé dans un certain nombre de cas aux diverses périodes de son évolution le traitement par le phos-

Digitized by Google

phore proposé par M. Kassowitz, et, à l'exception d'une modification favorable du côté de la nutrition générale, je n'ai pu constater jusqu'ici aucune modification aussi prompte ni aussi appréciable des conditions osseuses, comme l'a fait observer le savant Prof. Viennois.

TRAITEMENT DES TUMEURS ÉRECTILES PAR L'ÉLECTROLYSE.

TREATMENT OF ERECTILE TUMORS BY ELECTROLYSIS.

BEHANDLUNG DER ERECTILEN GESCHWÜLSTE DURCH ELECTROLYSE.

PAR LE DR. P. REDARD,

Chirurgien du Dispensaire Furtado-Heine.

Un nombre considérable de méthodes ont été proposées pour le traitement des tumeurs érectiles et cirsoïdes. Après avoir expérimenté les différents procédés thérapeutiques recommandés, nous sommes convaincus que *l'électrolyse* est la méthode de choix qui présents sur ses concurrentes une supériorité incontestable.

Si l'on cherche cependant des observations sur ce sujet, on voit que depuis 1882, époque à laquelle Ciniselli de Crémone publia sa première observation de guérison de tumeur érectile par l'électrolyse, très peu de chirurgiens ont eu recours à ce puissant procédé.

Quelques spécialistes électriciens ont toujours recours à l'électrolyse, la majorité des chirurgiens se servent de preuves rares qui sont quelquefois insuffisants et dangereux.

Les causes de l'abandon de l'électrolyse sont multiples : la méthode paraissait compliquée, les appareils électriques étaient imparfaits, d'un prix élevé, les galvanomètres d'intensité ne permettaient pas d'apprécier exactement l'intensité des courants. On accusait l'électrolyse d'être un traitement très long et surtout très douloureux, on pensait qu'elle pourait produire des eschares, des cicatrices vicieuses, des suppurations prolongées.

Les auteurs n'étaient pas fixés sur la pratique de l'opération et à l'exemple de Ciniselli appliquaient la méthode d'une façon empirique tantôt plongeant deux aiguilles dans la tumeur, tantôt une seule, se servant de courants trop faibles ou trop forts, produisant de véritables destructions des tumeurs traitées au lieu de décomposition des tissus avec atrophie vasculaire.

Le but de cette communication est de démontrer que l'électrolyse est la méthode de choix pour le traitement de toutes les tumeurs érectiles qui permet *grâce à une technique opératoire regulière*, d'obtenir, et sans aucun accident, des guérisons rapides. Nons voulons surtout démontrer que l'électrolyse est *pratique* et que tous les médecins peuvent l'exécuter avec facilité et sécurité.

Nous avons déja traité plus de soixante cas de tumeurs érectiles par l'électrolyse soit dans notre service du Dispensaire Furtado-Heine soit dans notre clientèle. Plusieurs des tumeurs présentaient un volume considérable, avec dilation vasculaire très notable, quelques unes des tumeurs avaient déjà été traitées sans succès par d'autres procédés. M. le Dr. Dubois de Gonzae qui avait assisté à nos opérations de l'hôpital nous a adressé de très importantes observations sur ce sujet.

Nous regrettons de ne pouvoir donner ici ces observations en détail. Nous citerons seulement le cas d'une tumeur érectile très volumineuse de la face interne de la joue gauche, traitée sans succés par des cautérisations ignées répétées et que nous avons guérie par 8 séances d'électrolyse.

Digitized by Google

L'appareil instrumental nécessaire pour la pratique de l'électrolyse est des plus simples. Une pile à courants continus fonctionnant régulièrement, particulièrement celles de Gaiffe, de charderi, un galvanomètre d'intensité bien gradué, des aiguilles en or ou en platine de ½ millimètre de diamètre et de 8 à 10 centimètres de long les aiguilles doivent être acérées, afin de s'introduire facilement et sans résistance, dans la tumeur.

L'enfant doit être soigneusement immobilisé. L'emploi du chloroforme ne nous paraît pas nécessaire. La durée des séances ne devra pas être prolongée, l'opération étant moyennement douloureuse.

Trois procédés peuvent être employés pour l'introduction des aiguilles :—

1° A l'exemple de Ciniselli et de la plupart des auteurs, les deux aiguilles sont introduites dans la tumeur à peu de distance l'une de l'autre (Ciniselli, Bœckel, Lincoln, Knott, Duncan, Delore, Voltolini) ;

2° L'aiguille *positive* seule est plongée dans la tumeur, l'autre pôle sous forme de plaque étant placé sur un point quelconque du corps (Monoyer, H. Pitoy) ;

3° L'aiguille *négative* seule est placée dans la tumeur (Althaus).

Le choix du procédé d'introduction des aiguilles a une influence considérable sur le résultat opératoire et nous désirons insister sur cette partie de notre communication.

Une connaissance plus parfaite des actions électrolytiques nous permet d'apprécier exactement aujourd'hui l'action produite par les aiguilles sur les tissus vasculaires.

Nous savons qu'au pôle positif se produit une action chimique intense que des caillots durs, résistants, adhérents aux parvis se forment dans les vaisseaux et les oblitèrent, les tissus se rétractent et se décomposent, il ne se produit pas d'eschares, la peau est respectée.

Au pôle négatif au contraire, il y a véritable destruction et cautérisation des tissus avec eschare consécutive, les caillots formés sont mous et diffluents.

D'après cela, il est facie de comprendre les avantages et inconvénients suivant que l'on plonge une ou les deux aiguilles dans la tumeur.

Au début de nos opérations nous nous servions des deux aiguilles placées dans la tumeur. Les résultats étaient certainement satisfaisants, mais nous avions souvent des hémorrhagies au niveau des piqûres, et des eschares avec des cicatrices consécutives.

Dans le procédé l'aiguille négative doit être changée plus souvent de place que l'aiguille positive, sous peine de voir se produire une eschare quelquefois assez importante avec suppuration consécutive. Les courants employés doivent être plus intenses que lorsque l'on emploie l'aiguille positive seule.

Les eschares ne peuvent être évitées, l'emploi d'aiguilles spéciales permettant l'isolement au niveau de la peau ne nous a jamais donné de bons résultats.

Les inconvénients de l'introduction des deux aiguilles doivent faire rejeter ce prouvé dans la plupart des cas surtout lorsqu'il s'agit de tumeurs siégeant sur la face, ou les cicatrices doivent être soigneusement évitées. Nous conservons cependant le prouvé dans quelques cas exceptionnels de tumeurs voluminaires, ne siégeant pas sur la face, lorsque nous désirons obtenir une destruction rapide, la rétraction produite à la chute des eschares accompagne des guérisons souvent rapides.

La piqûre au moyen de l'aiguille positive seule nous paraît surtout devoir être recommandée. Le pôle négatif sons forme de plaque doit être placé aussi près que possible de la tumeur, quelquefois à la partie superieure du bras.

L'aiguille doit être plongée obliquement à la périphérie de la tumeur, nous recommandons de placer quatre aiguilles à la fois reliées par de courts fils au fil principal de la pile. On obtient ainsi une action simultanée et rapide en plusieurs points de la tumeur. Les aiguilles doivent être laissées en place deux à quatre minutes et réappliquées ensuite en d'autres points. Le courant employé doit être de 12 à 20 milliampères.

Digitized by Google

Les séances qui doivent être de cinq à six minutes peuvent être rapprochées et faites tous les huit jours.

Le temps nécessaire à la guérison varie nécessairement suivant l'étendue de la tumeur, dans les cas où la tumeur avait le volume d'un petit œuf, quatre à cinq séances nous ont suffi pour obtenir une guérison parfaite. Une à deux séances suffisent pour les petits nævi-vasculaires.

Dès la première séance, surtout si l'on emploie quatre aiguilles à la fois, la tumeur blanchit, durcit très notablement, la circulation diminue, et à la deuxième séance les modifications obtenues sont déjà très importantes. Dans les cas de tumeurs cirsoïdes, lorsque des vaisseaux volumineux s'observent à la périphérie de la tumeur, l'aiguille doit être plongée obliquement dans l'axe des vaisseaux et laissée en place une à deux minutes.

Au début de nos recherches nous avions presque toujours un écoulement sanguin au moment où nous retirions notre aiguille. Le caillot adhérait, en effet, à l'aiguille positive, une partie de ce caillot déplacé et arraché permettait au sang de la tumeur de s'écouler par la figure extérieure.

Plus tard nous avons pu éviter *à coup sûr* toute hémorrhagie. Il suffit, en effet, avant de retirer l'aiguille, de renverser le courant pendant quelques secondes et de ramener à zéro.

L'aiguille ne doit être enlevée que lorsque l'on n'éprouve plus aucune résistance.

La peau qui recouvre la tumeur conserve son apparence à peu près normale, dans les tumeurs érectiles de la face, ou n'observe jamais de cicatrices.

La douleur notée au moment du passage du courant est supportable. En se servant de courants modérés on évite les mouvements des courants traversant une partie sacrée du corps. Il faut éviter, surtout si l'on opère au voisinage du crâne, de dépasser 25 à 30 milliampères.

Avec le procédé de figure avec deux aiguilles, ces chiffres peuvent être très notablement dépassés.

On doit dans tous les cas diminuer progressivement l'intensité du courant et éviter toute secousse.

La durée des séances ne doit pas dépasser cinq à huit minutes.

Nous recommandons enfin de désinfecter avec grand soin les aiguilles, avec de l'éther ou par le flambage.

Comme conclusion nous dirons :

L'électrolyse est la méthode de choix dans le traitement des tumeurs érectiles. Elle réussit toujours, là où d'autres méthodes ont échoué.

Grâce à une technique opératoire régulière, l'électrolyse met à l'abri de tout accident d'escharе, de suppuration, de cicatrice, etc.

Elle permet d'agir avec sureté et précision. La monopuncture positive doit être employée dans la grande majorité des cas.

Digitized by Google

NOTE SUR L'IMPALUDISME CHEZ LES ENFANTS AU BRÉSIL.

NOTE ON THE MALARIA OF CHILDREN IN BRASIL.

BEMERKUNG ÜBER DIE MALARIA BEI KINDERN IN BRASILIEN.

PAR LE DOCTEUR MONCORVO.

Professeur de clinique des maladies de l'enfance à la Policlinique de Rio de Janeiro.

Dans le tableau pathologique de la ville de Rio de Janeiro, capitale de l'Empire du Brésil, l'une des affections qui dominent d'une façon plus accusée et qui contribuent de beaucoup à l'augmentation de la mortalité à de certaines époques de l'année, c'est sans contredire l'infection malarique.

Mon observation personnelle faite sur plusieurs milliers d'enfants de cette ville m'autorise à affirmer que ce fléau sévit d'une façon plus accusée tant par sa fréquence que par sa gravité pendant toutes les périodes de l'enfance. De fait la malaria atteint les enfants de tous les âges, sa gravité étant à la raison inverse de ceux-ci. Les nouveau-nés sont eux-même les victimes les plus communes de l'infection, car chez eux, bien plus souvent qu' à toute autre époque de la vie, la malaria se cache très souvent sous des formes bizarres et insidieuses aux yeux de l'observateur peu expérimenté.

D'une façon générale toute élevation de la chaleur cutanée qui ne dépasse guère 38° ou 38.5°, reste aisément inaperçue chez les petits sujets qui sont encore emmaillotés dans des langes. Des légers accès se présentent et se succèdent parfois plusieurs jours de suite sans que la mère ou la nourrice s'en rendent compte, notamment lorsqu' ils survienent dans la nuit, ce qui est très fréquent. Bref un accès grave éclate, la chaleur monte à 39°, 40° et même plus, le système nerveux central se prend et le petit malade ne résiste guère à la majorité des cas à l'explosion très bruyante de cette infection qui minait en cachette sa santé depuis un temps plus ou moins long. Cette méprise devient fort commune dans les familles qui ne font point usage du thermomètre aussitôt que quelque altération de la santé de l'enfant est soupçonnée. Rien de plus trompeur que le cortége des symptômes ou mieux les formes que revêt la malaria chez les petits enfants qui n'ont pas encore atteint la première année. Dans un grand nombre de cas tout se borne à un changement de caractères du petit patient qui pâlit, a des selles grumeleuses et catarrhales plus ou moins fréquentes, perd l'appétit, vomit souvent le lait, mais la température de la peau semble normale notamment aux parties ordinairement examinées, c'est à dire, la face et les extrémités. Mais si on tâche de prendre à plusieurs reprises la température rectale, on la trouve augmentée soit dans la journée, soit dans la nuit. Dans le premier cas cette augmentation de la chaleur coïncide d'habitude avec un abattement général plus ou moins accusé, voire même du sommeil anormal, le refroidissement (pas constant du reste) des extrémites et finit très souvent par l'apparition de sueurs à la tête, au cou, parfois au tronc et bien plus rarement généralisées. C'est là le tableau symptomatologique qui précéde ordinairement de quelques jours et parfois de quelques semaines l'éclosion d'un accès pernicieux si on ne parvient pas à temps de l'empêcher en soumettant dès le début le petit malade à l'usage de la médication spécifique, c'est à dire, la quinine.

Rien de plus irrégulier que le type de la fièvre paludéenne chez les tout petits sujets ; les types classiques ne s'observent que rarement chez eux. Ces accès se répétent souvent plusieurs fois dans les vingt-quatre heures et ne gardent point de l'uniformité dans quelques jours consécutifs. Aussi lorsque la fièvre est franchement intermittente il est rare qu' on observe la suite classique des trois états, de frisson, chaleur et sueur.

Un des éléments les plus utiles dans d'autre pays pour le diagnostic de la malaria à toutes les périodes de la vie, l'hypertrophie de la rate, s'observe ici bien plus rarement,

Digitized by Google

chez les adultes, que celle du foie ; chez les tout jeunes enfants au contraire, l'augmentation du foie constitue la règle, notamment dans le cas d'infection aiguë. Ceux donc qui veulent se baser sur ce signe pour établir chez les tout jeunes sujets le diagnostic de la malaria aiguë, tombent presque souvent en erreur ne la reconnaissant point et exposant de la sorte la vie de leurs petits malades.

Lorsque l'enfant a dépassé les cinq ou six premiers mois les mères au temps que les médecins peu expérimentés veulent souvent voir dans les manifestations sus-indiquées l'expression d'une dentition pénible, ce qui est bien peu acceptable aujourd'hui, alors que la presque majorité des cliniciens s'accorde sur l'exagération du rôle qu' on avait fait jouer jusqu' ici à cette fonction naturelle par rapport aux maladies du premier âge.

Dans d'autre circonstances la malaria s'accompagne d'une inflammation viscérale laquelle masque tellement son origine qu'elle est souvent prise par une maladie purement locale et protopathique. La bronchite aiguë ou subaiguë, la broncho-pneumonie, la méningite, l'entérite reconnaissent très fréquemment pour cause à Rio l'intoxication paludéenne. Les symptômes de ces phlegmasies là s'imposent fort à l'attention du clinicien pour la détourner de leur nature infectieuse. Cette méprise entraînera les plus fâcheuses conséquences, car en l'absence du traitement spécifique il est rare de voir une terminaison heureuse.

Beaucoup d'autres formes peuvent être encore observées, quelques unes tellement bizarres et même bénignes en apparence, qu' une très grande sagacité devient nécessaire de la part du médecin pour les rattacher à leur cause vraie. Sans l'intervention prompte et active de la quinine la mort peut survenir parfois au bout d'un jour et de quelques heures même. La forme syncopale, celle congestive cérébrale ou pulmonaire, celle cholériforme, l'algide, tels sont les types les plus communs de l'impaludisme aigu larvé.

L'intoxication malarique chronique domine plus particulièrement chez les sujets qui ont déjà dépassé la deuxième année. Elle compromet fort la nutrition et l'évolution de l'organisme de l'enfant en l'exposant par cette déchéance physique à une extrême réceptivité morbide.

Dans ces conditions la malaria agit d'une façon toute particulière sur l'appareil digestif ; depuis la plus légère dyspepsie jusqu' aux diarrhées les plus graves et les plus réfractaires (séreuses, bilieuses ou lientériques) on peut observer toutes les nuances des phlegmasies gastro-intestinales chroniques. Parfois celles-ci s'accompagnent de légers accès fébriles survenus à des intervalles fort variables.

Dans la majorité des cas les diarrhées chroniques encore même qu' elles soient précédées de fièvre à type intermittent ou rémittent n'engendrent plus à la suite la moindre élevation de la chaleur, mais elles offrent souvent ceci de particulier : qu' elles surviennent à un certain moment du jour ou de la nuit étant quelquefois précédées ou suivies d'évacuations tout à fait normales. Ces sortes de diarrhées maremmatiques suivent d'ordinaire une marche très lente et résistent presque invariablement à tout autre traitement que celui par les sels de quinine.

Dans une communication faite à l'Académie des Sciences de Paris en 1884 sur la température de la paroi abdominale dans les cas de diarrhée chez les enfants, je fis voir la coïncidence et la corrélation qu' il y avait entre la marche du flux et la chaleur du ventre, celle-ci montant de 35° à 37.5° et s'abaissant parallèlement avec le retour des selles à ses conditions physiologiques.

Bien plus communément que dans les cas aigus l'impaludisme à marche lente s'accompagne de la congestion du foie, mais il n'est pas aussi fréquent de retrouver la sclérose hépatique chez les jeunes sujets infectés que chez les adultes dans des pareilles circonstances. Dans les endroits marécageux pas très éloignés de Rio où la malaria sévit endémiquement, le sang des enfants qui y restent subit bientôt une profonde altération, consistant notamment dans la destruction des hématies. L'œdème partiel ou

Digitized by Google

généralisé, l'ictère, la pâleur extrême de la peau et des muqueuses sont les conséquences de l'hydrémie et de l'hypoglobulie. Ces enfants présentent un aspect très caractéristique de la dystrophie malarienne. Chez eux il est d'ailleurs très commun de retrouver l'ectasie gastrique consécutive à une gastropathie due parfois exclusivement à cette infection. Ce fait que j'ai été le premier à signaler en 1882, à été largement observé par moi et par mes élèves.

Une circonstance que je ne pourrai manquer de signaler par rapport à l'impaludisme aigu chez les enfants, c'est sa fréquente coïncidence avec les fièvres exanthématiques, de façon à en obscurcir la marche cyclique. Dans d'autres cas la fièvre palustre éclate pendant la période terminale de l'exanthème lui prêtant en apparence une marche irrégulière et inattendue ; de sorte qu' à une fièvre éruptive bénigne et à marche régulière dont la terminaison favorable est justement prévue, survient parfois une nouvelle et brusque ascension thermique avec d'autres symptômes graves, et le petit malade succombe souvent à cette rechute apparente, qui n'est en réalité qu'un accès pernicieux greffé sur la maladie antérieure. Il faut être prévenu contre cette possible coïncidence pour sauvegarder la vie de ces petits malades au moyen du traitement quinique employé à temps. Dans le traitement de l'impaludisme chez les enfants, l'agent vraiment spécifique est, comme pour celui des adultes, la quinine.

Pour ce qui concerne les fièvres malariques, mon guide pour le dosage de cette substance est non seulement l'âge de l'enfant, mais surtout l'intensité de la fièvre ou la gravité des symptômes qui l'accompagnent. Tout d'abord je dois signaler que les enfants (au Brésil du moins) présentent une très grande tolérance pour la quinine, relativement bien plus accusée que chez les adultes ; c'est donc sans raison d'être la crainte qu 'ont les médecins peu expérimentés en pathologie infantile d'administrer à leurs petits malades des doses quelque peu élevées. C'est ainsi que je ne reste presque jamais au deçà de 25 centigrammes par jour dans les cas les plus bénins, et je monte à plusieurs grammes dans les cas graves, sans m'inquiéter beaucoup de l'âge de mon petit patient. Quand le cas est pressant par sa gravité actuelle ou attendue, je m'adresse presque invariablement à la méthode hypodermiqne pour l'administration du sel de quinine, car il ne faut compter que trop rarement dans ces conditions sur la tolérance gastrique et encore moins sur celle de l'intestin. Je dois ajouter que les injections faites profondément et avec tout le soin antiseptique n'engendrent presque jamais le moindre accident. J'en ai fait cinq mille environ et c'est à peine si dans quelques cas assez peu nombreux, de petits abcès se sont succédé ; même dans ces cas le traitement a réussi.

Parmi les sels de quinine j'emploie de préférence le chlorhydrate, tant par sa solubilité que par sa plus grande proportion d'alcaloïde.

Dans les cas d'hyperthermie je m'adresse à l'emploi simultané de l'antipyrine dont j'ai constaté les effets remarquables sur plus de 200 enfants avec la plus parfaite tolérance.

Dans les cas d'impaludisme chronique je recours aussi à l'emploi de l'arsenic, auquel je m'adresse également comme le complément du traitement quinique de l'intoxication aiguë. Enfin, mon observation personnelle m'engage à voir dans l'arsenic un très bon moyen prophylactique contre l'empoisonnement malarien.

Digitized by Google

SCARLATINAL NEPHRITIS.

NÉPHRITE SCARLATINE.

DIE SCHARLACHNEPHRITIS.

BY HENRY ASHBY, M.D., M.R.C.P.,

Physician to the Hospital for Sick Children, Manchester, and Lecturer on Diseases of Children, Owens College.

There is no disease which presents so many complications and sequelæ as does scarlet fever, and no one of these is of greater interest and importance than the albuminuria which so frequently attends its febrile stage or follows on during the weeks of convalescence, when the fever itself has spent its force. Much has been written on the subject, and many observations have been made, both in the wards and the dead-house, which have helped to elucidate it, but notwithstanding this, it appears to me that most of the recent writers have ignored some of the principal points and confused together forms of the disease which are essentially distinct. Some confusion has also apparently arisen by keeping the clinical and pathological observations too much apart, the work in the laboratory being done in ignorance of the symptoms presented during life.

It would be out of place for me to attempt to present to the International Congress an exhaustive paper on scarlatinal albuminuria which shall include an account of the work done by all observers who have made the subject their study. All I hope to do is to present as concisely as I can an account of my own observations; in short, a picture of scarlatinal nephritis as I have seen it, both in the wards and in the post-mortem room. That it must be short and concise, must be my excuse for many errors of omission and for much being omitted which I would gladly have found a place for. My own experience has been gained in the fever ward of the Hospital for Sick Children, Pendlebury, Manchester, where, during the past eight years, upwards of 1500 cases of scarlet fever have been under my care.

Following Friedlander, we may divide the subject into three divisions, in the following way:—

1. The "initial albuminuria," which is frequently present during the first few days when the fever is high, and which, in uncomplicated cases, disappears as the fever subsides.

2. The albuminuria which is often present during the second and third weeks (though it may have existed from the first), in those cases where the fever is maintained in consequence of the foul state of the fauces and the presence of glandular inflammation. In these cases the albumen in the urine is associated with changes in the kidneys of a septic character.

3. The albumen which comes on during convalescence—usually after an interval of apyrexia—though it may supervene in cases where the fever has been maintained in consequence of various complications. The usual time is from the sixteenth to the twenty-fourth day; in its typical form it is accompanied by œdema, with blood casts and epithelial débris in the urine.

(1) Concerning the initial albuminuria there is but little to be said; albumen is absent in the urine of mild cases with but slight or transient fever, and often present in traces or moderate quantities in the deeply colored, highly concentrated urine of those cases where the fever (103°–105°) runs high. It is, however, absent in some rapidly fatal cases, just as it is absent in some malignant cases of diphtheria. I have never seen blood in sufficient quantities to give the urine a red or smoky appearance in the early stages. This "febrile albumen" is present in typhoid fever, pneumonia, erysipelas, as well as in the early stages of scarlet fever, and is probably due to the

Digitized by Google

same cause in all these instances, whether to changes in the nutrition of the epithelium, consequent on the febrile state, or in some changed pressure in the capillaries. An examination of the kidneys in fatal cases of scarlet fever during the first few days or week does not show, according to my experience, any gross changes. The cortex and surface of the kidneys are pale, resembling in this respect the pallid appearance of the liver and heart muscle, due to the cloudy condition of the epithelial cells, which appear, under a high power, to be more granular than usual. The microscope also shows congested capillary vessels, coagulated albumen in the tubules, a slight extravasation of leucocytes here and there around the capillary veins, and an increase of the nuclear elements in the glomeruli and capillaries.

2. During the second and third weeks, in those cases where there are sloughy throats, cellulitis and adenitis, albumen is mostly present in the urine in variable quantities, though often there is not more than a distinct trace present and never in quantities approaching to what is sometimes seen in post-scarlatinal nephritis. I have not seen blood present in sufficient quantities to tinge the urine, but that blood is not infrequently present is shown by the careful testings of Stevenson Thomson, and the microscopical examination of the kidneys in fatal cases shows that minute hemorrhages take place into the tubules. Epithelium, fibrinous or blood casts are not, or only exceptionally, present in such cases. Œdema or puffiness about the face is likewise absent, unless the post-scarlatinal variety to be described shortly supervenes. The post-mortem changes found in the kidneys in these cases varies considerably, being least marked in those dying early in the disease, and best marked in those who have died toward the end of the third week, where the fever has been high, the fauces sloughing, and there has been much suppuration and inflammation of the glands and the tissues around. In the former case the kidneys do not appear much changed to the naked eye, except some pallor and injection of the vessels, and the microscopical changes found are those already mentioned, namely, effusion of leucocytes around the capillary vessels and glomeruli, some exudation of albuminous matters into the capsular space, cloudy swelling of the epithelium and increased nuclei in and around the glomeruli. In the latter case the changes are often very striking. The kidneys are enlarged; thus in a girl of 2¾ years, who died on the twentieth day, they weighed 14 ounces (heart, 3½ ounces; spleen, 8½ ounces); in a boy of 5½ years, 9¾ ounces; in another of 3 years, 8½ ounces. They are limp and flabby, pale or of a cream color, the surface is mottled with minute hemorrhages and injected capillary vessels. On section the cortex is increased in width and of the same mottled appearance as the surface; the pyramids show marked injection of their vessels. In some cases minute abscesses or cheesy spots are seen at the base of the pyramids and extending along the interlobular vessels into the cortex. These abscesses vary in size from a pin's head to a split pea. On examining sections microscopically under a low power, the most striking features are the masses of leucocytes which surround an interlobular vein or artery and the glomeruli supplied by it. Sometimes whole tracts of kidney are infiltrated with leucocytes, and the glomeruli which are surrounded have undergone hyaline degeneration. The capillaries are often dilated, tortuous and gorged with blood; small hemorrhages have taken place between and into the tubes; often there are some leucocytes and amorphous-looking matter in the capsular space around the glomeruli. The epithelium is notably changed, being coarsely granular. In a few cases micrococci-laden emboli are present in the vasa recta or capillary vessels. All these changes are undoubtedly septic in character, being the result of the inflammatory lesions taking place about the throat, septic materials being transferred from the primary source, by means of the circulation, to the kidneys. The inflammation starts from a number of foci, and is not a general inflammation, as is the post-scarlatinal variety; as a consequence, the kidneys rarely become choked and entirely obstructed, and, more-

Digitized by Google

over, death usually takes place in consequence of the general septicæmic state, perhaps independently of or only assisted by the kidney lesion.

I have already referred to the albumen in the urine during life being usually moderate in quantity and unaccompanied by blood to any extent, casts or epithelium. Are there ever any definite and distinctive symptoms of kidney disease apart from the presence of albumen? In connection with the urine, it must be said that children so suffering are acutely ill, and, especially in the younger ones, it is impossible to measure the quantities passed; often the whole of the urine is passed in bed and cannot be saved for examination.

It is well in all cases where septic kidneys are suspected to test the urine daily for the coloring matter of the blood, by means of tincture of guaiacum and a solution of peroxide of hydrogen, for blood may be present in quantities too small to be detected by the eye, and if present, it is exceedingly probable that a septic nephritis exists.

While death in the majority of such cases appears to be due less directly to the kidney lesion than to the general state of septicæmia, the kidney lesion being part only of a general septicæmic state, yet in exceptional cases symptoms of irritation of the kidneys may be present, to be followed by complete suppression of urine and death from a choked kidney. In a few cases I have known the temperature to be maintained during the second week, in consequence of excessive glandular inflammation, but the temperature has continued elevated after the subsidence or improvement of the lesions about the neck, without any local cause being discovered. The fauces, the cervical glands, the middle ear, the lungs may be searched in vain for the cause of the fever, and the event may prove that a septic inflammatory condition is in progress in the kidneys and leads to a fatal result. In such cases I have noted excessive quantities of urine passed, 40–90 ounces, of low specific gravity 1004–1008, and containing small quantities of albumen, but no deposits of either coloring matter of blood or casts, to be followed suddenly, at the end of the third week, by almost complete suppression of urine, death following in a few days, with uræmic phenomena. Dr. Dreschfeld tells me his experience has been somewhat similar.

CASE I.—In one of my cases somewhat similar to the above, which died on the twenty-first day, the death was immediately brought about by suppression of urine, though the sloughy condition of the throat and septic pneumonia present made it clear that life would not have been long prolonged in any case. The kidneys were enlarged, pale and flabby, whole tracts of the kidney were infiltrated with leucocytes, and minute hemorrhages had taken place, both inter- and intra-tubular, apparently effectually choking the kidney and preventing its normal function.

In the vast majority of cases, however, septic inflammation of the kidneys gives no symptoms which can be differentiated from those of the general septic infection. There is no general inflammatory congestion, but rather numerous points of inflammation, sufficient fairly normal kidney structure being left to carry on the functions, and consequently there is no general œdema, diminished urine or uræmic symptoms. It is only when the inflammatory lesions are more than usually extensive, or some sudden hemorrhage takes place, that suppression of urine arises.

3. *Post-Scarlatinal Nephritis.*—It is the nephritis which occurs during convalescence which is best known, and surpasses all the forms in interest and importance. Septic nephritis accompanies and is practically lost in a general septicæmia; post-scarlatinal nephritis either supervenes in an apparently healthy individual, or sets in as the last and fatal complication in a patient who has had a severe attack, and is apparently just over the worst. This form of nephritis has been described as "tubal," "acute desquamative," or "catarrhal nephritis," but the term post-scarlatinal sufficiently identifies it without committing to any notion as to its pathology. The term "desquamative" is especially bad, inasmuch as the shedding of the epithelium is a purely mechanical act, the result

Digitized by Google

of the fibrinous cylinders being forced along the tubules by the force of the urine behind. The albuminuria usually comes on from the fourteenth to the twenty-sixth day, *i.e.*, during the third or fourth weeks, or at least appears in larger amounts than before, as albumen is frequently present, as already considered, during the first and second weeks, and the post-scarlatinal variety is apt to supervene in cases of a mild septic character, which are on their way to recovery. There can hardly be a doubt that during the attack of scarlet fever, even in mild cases, the kidneys undergo irritation in separating the poison from the blood, and are left, like the lungs after measles, in a condition in which they readily take on inflammation of a fibrinous or croupous type. In both cases the pneumonia or nephritis may follow mild attacks, but the experience of hospital practice shows that where reasonable care is taken, the mild cases rarely suffer from nephritis, at least in its severer forms; the worst and most fatal cases are those where the nephritis supervenes on a severe and prolonged attack of the primary fever, where there has been much exudation about the fauces and glandular inflammation. The severity of the attacks vary immensely; sometimes they are so mild that they might easily escape the attention of a casual observer. A slight puffiness of the face and a small quantity of albumen in the urine may be all there is to notice, both disappearing in the course of a few days or weeks. On the other hand, the attack may be sudden—an acute inflammatory congestion taking place, to be quickly followed by uræmic symptoms. In the majority of cases, the two earliest and most characteristic symptoms are puffiness of the face, and a diminished quantity of urine; these may be present before any albumen can be detected in the urine. I have often noticed, in passing round the wards, a slight pallor or puffiness of the face in children who had no albumen, but in whom albumen appeared in a few days. The gradually diminishing amount of urine passed has frequently called my attention to a case of commencing nephritis. An evening rise in the temperature is often present, to become normal again in the morning; a rise in the temperature is frequently followed by the passage of bloody urine, as if a temporary congestion of the kidneys had taken place, which was relieved by the transudation of blood into the urine; the following evening, perhaps, a repetition takes place, the blood disappearing or is in diminished quantity during the period in which the temperature is normal. The temperature becomes sub-normal in uræmia. The amount of albumen present varies considerably; there may be none, or the urine may set solid on boiling; the specific gravity is apt to vary also; it is mostly from 1010–1020. Nephritic urine on standing usually deposits a precipitate which resembles the flocculent deposit of beef tea, and which consists of epithelial débris, blood corpuscles, and fibrinous casts of the tubes. The urine diminishes still more, perhaps only a few drops to a few ounces being passed; œdema of the face and extremities, vomiting and other uræmic symptoms follow, when suddenly a crisis like that of pneumonia takes place; large quantities of smoky urine (2000 to 4000 cc.) of low specific gravity are passed, the œdema quickly disappears and the patient is convalescent again.

The following case illustrates such attacks:—

CASE II.—*Scarlet Fever, Nephritis Uræmia.*—B. 92, Frank B., aged eight years. First day, sore throat; second day, rash; admitted August 1st, 1882. Third day of illness, pulse 120; temperature 102°–104°; moderate rash; tonsils congested and covered with secretion; rash disappeared seventh day; temperature remained 100°–101° till sixteenth day, rose to 102.8°, and the urine became dark brown with much deposit, and contained blood corpuscles and fragments of epithelial casts; passed 250 cc., averaged 800 cc. previous week. Seventeenth day, pulse 80; temperature 99°–102°; urine 350 cc.; specific gravity 1015; dark brown; about one-third albumen; many casts; vomited three times; pulv. jalapæ co. mist. pot. citr. Eighteenth day, temperature 98°–100°; urine 100 cc.; ext. jaborandi liq., ♏ xx, and packs at 110° repeated several times; pulv. fol. jaborandi, gr. v. st.; moderate perspiration. Nineteenth day, pulse 68; temperature

Digitized by Google

98°–99°; urine 175 cc.; smoky, with dark-brown flocculi; specific gravity 1015; one-third albumen; two packs at 110°, preceded by pulv. fol. jaborandi, gr. x.; fair perspiration. Twentieth day, 300 cc. of urine; specific gravity 1013; one-third albumen; jaborandi and packs. Twenty-first day, much headache; sweated profusely after jaborandi; passed 450 cc.; specific gravity 1010, and one-fourth albumen; systolic bruit at apex. Twenty-second day, temperature 98°–102°; much headache; no sleep; vomited; urine 750 cc.; specific gravity 1008. Twenty-third day, uræmic convulsions; muscular twitchings; foaming at mouth; convulsions arrested by chloroform. Twenty-fourth day, urine 250 cc.; specific gravity 1012; still smoky, but less blood; no return of convulsions. Twenty-fifth day, urine 150 cc. Twenty-sixth day, urine 1000 cc.; after this date gradually improved, passing 1000–2000 cc. daily for next week or two. Discharged fifty-seventh day, with trace of albumen; a month or two after his urine was free. (Has remained quite well since August, 1887.)

Occasionally such attacks are fatal in the early stages, being very acute from the first in the following case:—

CASE III.—*Scarlatinal Nephritis.*—B. 192, Mary Ann N., aged nine and a half years; father died of phthisis, a sister of scarlet fever; always a delicate child; first complained of sore throat a fortnight ago; rash appeared soon after. Admitted November 3d, 1884. November 4th, general œdema; face anæmic and puffy; pulse 60; temperature 99°; cardiac sounds normal, except second accentuated, apex beat normal; no desquamation; urine passed 700 cc. during night, specific gravity 1014, clear, not smoky, no deposit, much albumen, one-third. November 5th, pulse 84, temperature 98°; face puffy; urine 750 cc., no blood or deposit, less albumen, one-sixth. November 7th, temperature rose suddenly, pulse 136; temperature 104.6°; is tossing about and slightly delirious; pulse high tension; urine 1400 cc. passed; frequent micturition; urine opaque, dark, with much deposit; albumen about one-third; vapor bath; cupping to loins; vin. ant. m. v. t. d. s. November 8th, pulse 132; temperature 103°–105°; much œdema of face; semi-delirious, muttering to herself; tongue coated and dry; constantly vomiting; urine specific gravity 1012, dark red, opaque, much deposit; 200 cc. only; about half albumen; pilocarpine nitr. gr. one-eighth and packed; tr. digitalis and ammonia. November 9th, pulse 140; temperature 105°–105.6°. Is drowsy and dull; frequent vomiting; heart's impulse diffused; no crepitation in lungs, but respiration is slightly impaired at both bases, and vesicular murmur is weak. Not possible to save urine from fæces; apparently very little passed. Some diarrhœa, bowels acted five to eight times daily for last three days, without medicine. Died early November 10th.

Post-mortem.—Only possible to procure one kidney, weight 4 oz. Surface dark red; vessels intensely injected; capsule strips. On section much blood exudes; cortex dark red; Malpighian bodies injected; pyramids chocolate color. The whole organ is in a state of acute congestion.

Microscopical examination shows the kidney to be in a state of acute inflammation. There has been a transudation of leucocytes from the vessels and glomeruli; and whole tracts of the kidney, especially near the capsule, are occupied by leucocytes; while the contorted tubes are distended with blood. No micrococci were discovered.

In the two foregoing cases there can hardly be a doubt that there was an acute inflammatory congestion of the kidneys, of the type of a croupous pneumonia; in the one case the engorged vessels are relieved by the exudation of liquor sanguinis into the air vesicles of the lungs, in the other case into the tubules of the kidneys. The plugs of fibrine which are thus formed in the tubes, are forced down by the pressure of the fluid behind them, stripping off the epithelium, and thus damaging the secreting powers of the kidney. Death rarely takes place in this stage, inasmuch as sufficient urine is secreted, and the fibrinous plugs expelled in sufficient numbers to relieve the blocked-up tubes. It is only in extreme cases, like the one just referred to, where the kidneys

Digitized by Google

are acutely engorged and appear to secrete blood instead of urine for the time being, that death ensues from the completely choked condition of the organs. In other cases, instead of the congestion being relieved and recovery taking place, as in the first case related, secondary changes take place in the kidneys, which gradually interfere more and more with the secreting powers of the kidneys, and death results directly or indirectly in consequence of the retention in the blood of the waste products which it is the office of the renal organs to separate. These changes are the result of the chronic congestion of the kidneys. These consist in—

1. Periglomerular nephritis. This consists in a hyperplasia of the endothelium lining Bowman's capsule; the cells accumulate and occupy the space between the glomerulus and capsule. They gradually become converted into fibroid tissue and compress and obliterate the glomeruli.

2. Glomerular changes. In most cases the glomeruli appear enlarged, and their nuclei increased; the capillary loops often contain fibrinous plugs (thrombi). It is difficult to satisfy one's self whether the nuclear increase takes place in the nuclei of the cells lining the capillaries (Friedländer), or in the nuclei which are outside the capillaries. These glomerular changes lead to still further obstruction of the blood current, and, consequently, lessen the secretion of urine. The epithelium degenerates, becoming granular; the tubules in places become enormously dilated, in consequence of being plugged by casts.

These changes are usually found in cases dying during the fourth or fifth week, or later, after the nephritis has lasted ten or fourteen days or more. Such kidneys are enlarged, œdematous; the cortex pale and mottled, with injected vessels; the Malpighian bodies are enlarged, pale, and, with a lens, may be seen to be of irregular outline. Death may be brought about either from uræmia or quite as often from cardiac dilatation and failure, pericarditis, pleuro-pneumonia or peritonitis.

The following cases are illustrations of these lesions:—

CASE IV.—*Scarlatinal Nephritis.*—B. 72, Thomas R., aged five years; father strong; mother dead. Always good health till present illness; April 28th, vomited and complained of his throat, and was feverish; rash next day. Admitted April 30th. He is a well-nourished boy. Third day of illness pulse 120; temperature 101.2°; remains of rash on thighs; tonsils enlarged and red, with some yellow spots of secretion on them; urine, no albumen. Sixth day pulse 120; temperature 101°; remains of rash. Eighth day, pulse 144; temperature 102°; rash gone, throat improved. Eleventh day, pulse 120; temperature 102°; urine 770 cc., no albumen. Sixteenth day, temperature normal for first time since admission; there has been some bronchial catarrh. Nineteenth day, temperature has touched normal for last two or three days; discharge from right ear; urine, yesterday, 550 cc., acid, clear, no albumen; specific gravity 1010. Twenty-second day, urine contains blood and much albumen, acid, 550 cc. Twenty-fifth day, urine still much blood, 300–500 cc. in last day or two; sick and drowsy. Twenty-seventh day, is very sick; temperature, intermittent for last two days; 400 cc. Twenty-ninth day, vomits everything, urine has passed 780 cc., hot packs, and potassium citrate and digitalis. Thirtieth day, still vomits; pulse 84; temperature keeps sub-normal; passed a little urine into bed; pilocarpine nitrate; much perspiration; is much wasted; tongue dry and coated. Thirty-second day, drowsy; temperature sub-normal; pilocarpine and hot packs; no urine for last forty-eight hours; some diarrhœa. Thirty-fifth day, only 25 cc. in last three days; lies quite quiet; pupils contracted; much subsultus; no œdema; bleeds from nose; unconscious; some diarrhœa; no convulsions; death.

Post-mortem.—Much emaciation; slight rigor mortis in legs. Lungs: purulent mucus in bronchi; lungs gorged; some minute hemorrhages on surface and in section. Heart: pericardium adherent but separable; muscle flabby; right ventricle dilated;

Digitized by Google

right ventricle wall thickened, cavity dilated. No valvular lesions. Liver 22 ounces, pale and fatty. Spleen 2 ounces. Kidneys 7 ounces, slightly enlarged; surface pale and mottled; cortex not increased; Malpighian bodies are pale, enlarged, and stand out like grains of sand; pyramids normal.

Microscopical examination shows no normal glomeruli; all are seen in different stages of periglomerular nephritis. The glomeruli are surrounded by a fibro-cellular growth, which compresses, and in some instances has destroyed the capillary loops; the capsule is surrounded by a collection of leucocytes. The convoluted tubules are dilated and filled in many places with blood corpuscles. The straight tubes contain many blood casts.

CASE V.—*Scarlatinal Nephritis.*—B. 7, Richard R., aged six years. Has always been a healthy boy. Two weeks ago he vomited and was ill; no definite history of a rash; three days ago it was noted that he was desquamating, and two days ago his face swelled. Admitted January 15th. Face much swollen; eyes nearly closed; discharge from left ear; cervical glands swollen; desquamating about feet, Pulse 80, temperature 101.4°. Lung sounds normal; urine smoky; much brown deposit; about two-thirds albumen, blood cells and casts. January 16th, pulse 92, temperature 101°; has passed 60 cc. of urine, half albumen; is drowsy; "packs" and injected pilocarpine one-sixth gr. January 17th, pulse 116, temperature 101.6°; still drowsy; much sweating; only passed 11 cc. of urine. 18th, pulse 88; drowsy; vomited three times; "packs" and jaborandi; much albumen. 19th, pulse 72, temperature sub-normal; vomited five times; tongue coated and dry; very drowsy; urine 26 cc.; muscular twitchings about face; pupils sluggish. 20th, much vomiting; some diarrhœa; vapor baths. 21st, pulse 80, temperature sub-normal; less drowsy; urine dark red, much deposit; half albumen, 17 cc. passed. 23d, pulse 104, temperature 100.6°; tongue brown and dry; urine 77 cc.; diarrhœa. 24th, muscular twitching of face; grinding teeth; general convulsions; unconscious; death four hours after onset of convulsions. Duration of illness about twenty-three days, but history not reliable.

Post-mortem.—Nutrition poor, slight general œdema. Lungs: slight excess of fluid in pleuræ; a few flakes on surface; lungs gorged and œdematous. Heart four ounces, all cavities, but especially right, distended with colorless clot; left ventricle, wall hypertrophied, cavity small; no endocarditis; excess of fluid in peritoneum, some flakes of lymph. Intestines distended, mucous membrane injected. Liver 22 ounces; pale, centres of lobules slightly injected. Kidneys eight ounces, large and œdematous, contain much serous-looking fluid; surface pale pink color, consisting of red points on a cream-colored ground; cortex not relatively increased; pale cream-color with irregular mottling; Malpighian bodies appear enlarged and stand out like grains of sand; pyramids injected and of darker color than cortex.

On Microscopical examination very few of the Malpighian bodies are normal, the majority having undergone marked changes. Many of the glomeruli are compressed and wasted by a fibro-cellular growth situated in the capsule space, apparently derived from the glomerular epithelium. In some cases there is an accumulation of leucocytes around the Malpighian body outside the capsule. Many of the glomeruli are gorged with blood, in others small thrombi of clear fibrin are present. The epithelium of the convoluted tubes is coarsely granulated and misshapen, the lumen of the tubes being blocked in some places by epithelial débris and coagulated albumen, in others by blood or fibrinous casts. Some of the tubes are dilated and their epithelium flattened as if they had been over-distended by urine. The interlobular plexus is gorged with blood.

CASE VI.—*Scarlatinal Nephritis.*—B. 175, James McG., aged four years. Always been a strong child. Three weeks ago began with vomiting and purging, sore throat, and rash. Admitted October 1st, 1884 (about twenty-second day). Is desquamating, face pale and puffy, cervical glands enlarged, tonsils irregular, ulcerations. Pulse 88; tem-

Digitized by Google

perature 100°. Heart's impulse in fifth space just within l. n. l. First sound murmurish; second accentuated. No general œdema, urine smoky, albumen about one-fifth. Twenty-fourth day, face more puffy; pulse 100; temperature 97°–100°; urine 270 cc.; specific gravity 1014; some brownish deposit. Is drowsy and sometimes sick. Tr. digitalis ♏5 and pot. citr. 4 hor. Twenty-sixth day, pulse 120; temperature 99°; urine 300 cc.; blood, albumen and much brown deposit. Twenty-seventh day, pulse 84; intermittent; (?) systolic murmur at base, omit digitalis. Twenty-ninth day, pulse 128; temperature 99°; pulse regular; vomited three times; urine 350 cc., dark; much brown deposit; specific gravity 1012; albumen about one-sixth; cardiac impulse diffused in l. n. l. In the evening became restless, and died very suddenly.

Post-mortem.—Nutrition good; much purpuric staining. Lungs: some fluid in both pleuræ; œdematous old adhesions; both lungs œdematous. Heart: two ounces of serum in pericardium; some dark clot in right auricle; right ventricle empty; left auricle empty; some venous blood in left ventricle. All cavities dilated; some hypertrophy of wall of left ventricle. No valvular lesions. Some ascites. Liver 26 ounces; normal. Spleen five ounces; normal. Kidneys six ounces; large capsule strips; surface cortex not increased; Malpighian bodies pale, stand out prominently; papillæ normal.

Microscopical examination shows the glomeruli enlarged, and their nuclei increased. In some there is a marked peri-glomerular nephritis. The epithelium is granular; the tubes contain many blood casts, including the convoluted, looped tubes and straight tubes. Some of the convoluted tubes have undergone dilatation. There seems to have been an exudation of leucocytes from some of the different arteries of the glomeruli, and also around the latter.

CASE VII.—*Nephritis.*—W. 557, Robert McC., was in C. H. P. two years ago, suffering from consumption (?). Been well since discharge. Was well up to July 16th, when mother noticed his face was swollen, and he noticed his trowsers tight; his urine was scanty and dark-colored; nose bled yesterday, and he complained of shortness of breath. Boy died very suddenly soon after his admission to hospital, July 21st. No certain history of scarlet fever.

Post-mortem.—Nutrition good; general œdema, though not excessive; purple staining of dependent parts. Lungs: right, three ounces of serum in pleural cavity; left, adherent at base and side by old watery adhesions; right, upper lobe gorged and œdematous; lower lobe solid, on section dark purple, friable, sinks in water; left lung closely resembles right, but is less advanced. Heart: no clot in cavity, only some dark liquid blood. Right ventricle cavity much dilated; wall thin and flabby. Left ventricle cavity dilated; wall thickened and pale. Both auricles dilated; thin walls; no valvular lesions; some fluid in the peritoneum. Liver large and gorged. Spleen very soft; pulp easily washes away. Kidneys: capsule strips readily, in places bringing away some of cortex; surface pale with injected stellate veins. On section, cortex pale and wide; Malpighian bodies stand out prominently like grains of sand; interlobular vessels; well-marked pyramids, dark purple.

Microscopical Examination.—The Malpighian bodies are enlarged, the glomeruli being apparently swollen, occupying the whole of the capsule space, and not being surrounded by any fibro-cellular growth. The nuclei of the glomeruli are much increased in number; whether these are the nuclei of the epithelium lining the capillaries or belong to the extra-capillary connective tissue or epithelium, it is difficult to say; apparently they are extra-capillary. The epithelium of the contorted tubes is swollen and coarsely granular, and in some places completely choked with blood corpuscles; the straight tubes contain much blood, and the capillaries are gorged.

These cases are fairly illustrative of the changes which take place in the kidneys, following on to the condition of acute inflammatory congestion. In Case VII it is un-

Digitized by Google

certain whether the nephritis was of scarlatinal origin or not, but it is of interest in illustrating a class of cases in which the changes are glomerular and not peri-glomerular.

CONCLUSIONS.

1. The "initial albuminuria" is of little importance in prognosis; it is mostly present where the fever is severe, but is also sometimes absent even in malignant cases.

2. Cases in which the throat complications are severe and the fever continued into the second or third weeks, are often complicated by a form of nephritis of a septic character. There is usually albuminuria, but rarely œdema or uræmic phenomena.

3. The post-scarlatinal form of nephritis belongs to croupous inflammations; secondary changes are apt to follow, which mostly affect the glomeruli and lead to more or less stoppage of the circulation. In chronic cases degeneration of the epithelium and dilatation of the tubules take place.

ON THE ANATOMICAL CHARACTERS OF SCARLATINAL NEPHRITIS.

SUR LES CARACTÈRES ANATOMIQUES DE LA NÉPHRITE SCARLATINE.

ÜBER DIE ANATOMISCHEN KENNZEICHEN DER SCHARLACHNEPHRITIS.

BY FRANK GRAUER, M. D.,

Instructor in the Carnegie Laboratory of Bellevue Hospital Medical College; Pathologist to Harlem Hospital, New York City, and to the Workhouse and Almshouse Hospitals, Blackwell's Island.

The different forms of nephritis that occur in scarlatina may be well classified from a pathological point of view, according to Friedländer, under one of the following three types :—

1. The initial catarrhal nephritis.
2. The large flabby hemorrhagic kidney.
3. Acute glomerulo-nephritis, or nephritis post-scarlatina. The first two forms are not as frequently met with post-mortem as the latter, and as their pathological changes are well agreed upon, the writer will pass hurriedly over them and devote most of his time to the third form, upon which there still exists differences of opinion among pathologists as to the microscopical changes that occur in the same. The first form, namely, the initial catarrhal nephritis, is met with in the first week of the disease accompanying the exanthema, and is only recognized by a chemical and microscopical examination of the urine, in which you find albumen, mucus and hyaline casts, occasionally red and white blood corpuscles, renal epithelium and granular casts. It is of short duration and rarely leads to death. It is only in those cases in which children die from complications that are associated with the disease, among which diphtheritis, bronchitis and broncho-pneumonia play an active part, that we are able to study or witness the following pathological changes. The kidneys are enlarged and hyperæmic, the capsule is readily stripped off, and on a cut surface there is seen thickening of the cortical substance with more or less loss of striæ and the glomeruli appearing as red dots.

Microscopical examination shows granular degeneration with desquamation of the epithelium lining especially the convoluted tubules. Hyaline and occasionally granular casts are found in the straight tubes, and where the process has been more severe the beginning of a round cell infiltration in the intestinal tissue. Friedländer noticed proliferation of the cells lining the convoluted tubules. From the above description

Digitized by Google

one would say the changes were those of a parenchymatous inflammation; why not call it parenchymatous nephritis instead of initial catarrhal nephritis, so called by Friedländer? According to Virchow's definition, parenchymatous inflammation leads to a fatty degeneration of a cell with disturbance of its function. In the above form of nephritis we have no fatty degeneration of the epithelial cells lining the urinary tubules. Again, in parenchymatous inflammation we do not, as a rule, have cellular proliferation.

The second form of nephritis, namely, the large, flabby, hemorrhagic kidney, is not often met with. Friedländer found it present in only twelve cases of the two hundred and twenty-nine that he examined. It generally occurs after the first week of the scarlatina and runs a rapid course. Œdema is rarely present, and in some cases the urine is normal up to within twenty-four or forty-eight hours before death. This form of nephritis is found in those cases of scarlatina which are accompanied by extensive angina and diphtheritic inflammation. The microscopical appearances of the kidneys are as follows: They are enlarged and softened. The cortical substance is somewhat thickened and grayish-red in color. The striæ are lost, and the glomeruli, as a rule, are invisible. The cortex on its surface is studded with ecchymoses and large hemorrhagic infiltrations, and in some cases we often have small miliary abscesses. Microscopically, the tubules are found to contain red and white blood corpuscles, desquamated and degenerated epithelium and the various forms of casts. In a great many cases you have an acute interstitial nephritis that is situated in the cortical substance around the glomeruli and between the convoluted tubules, characterized by a round-cell infiltration. Occasionally small spots of coagulation necrosis are seen, in which, by means of one of the aniline dyes, you are often able to detect micrococci. Whether these micrococci have any distinct relation with the etiology of scarlatina, or whether they are merely one of the various forms of micrococci that are found in acute abscesses, such as the staphylococcus pyogenes aureus, I am unable to state, not having made cultivations from the same, but hope at some future day to be able to do so. This form of nephritis is probably to be looked upon as a septic inflammation of the kidney.

The third form of nephritis, namely, acute glomerulo-nephritis, or nephritis post-scarlatinosa, rarely occurs in other diseases, and is almost characteristic of scarlatina. The writer met with it once in a case of heart trouble in which the diagnosis was verified by a microscopical examination. Klebs found it present only in scarlet fever. To quote his own words, he states that the purest form of acute glomerulo-nephritis is found following scarlatina. This form of nephritis occurs in the third or fourth week of the disease. The patient is convalescing; there is no fever, and the urine is normal. Everything seems to go along nicely, when unexpectedly œdema is noticed about the eyes and lower extremities. Examination, then, of the urine shows evidences of albuminuria. It is turbid, contains albumen, is diminished in amount and of a high specific gravity. Microscopical examination of the urine shows renal epithelium, pus cells, red blood corpuscles, and hyaline casts, occasionally epithelial and blood casts. In favorable cases the œdema disappears within a few days, the urine approaches the normal type, and in a short time the patient fully recovers. In other cases the œdema increases, fluid accumulates in peritoneal, pleural and pericardiac cavities, the urine still diminishes, it becomes bloody, varying in color from a slight smokiness to a dark brown, and contains a large amount of albumen. If examined microscopically at this stage it is seen to contain a large number of red blood corpuscles, pus, and renal epithelial cells, hyaline, blood, granular and epithelial casts. If the process increases we have uræmic symptoms setting in, with anuria and death.

As there still exist differences of opinion among histologists as to the microscopical anatomy of a glomerulus, and as the microscopical changes are limited to that part of

Digitized by Google

the kidney, it may probably be advisable, with the kind permission of the Section, to consider the structure of the same here, before proceeding to its pathological changes.

The uriniferous tubules all arise within the labyrinth of the cortex by means of a globular enlargement, $\frac{1}{100}$ to $\frac{1}{125}$ of an inch in diameter, called Bowman's capsule, which invests the tufts of capillary blood vessels called the glomerules. This capsule consists of a homogeneous membrane lined by a layer of flattened epithelial cells. The nuclei of these cells are generally arranged in groups, thus resembling in this point of structure the epithelium lining the air cells of the lung. The vasa afferentia which is derived from the interlobular artery enters the capsule at the side opposite the urinary tubule. Within the capsule it breaks up into a plexus of capillaries called the glomerulus. A vein, the vasa efferentia, which is smaller than the afferentia, proceeds from the centre of the glomerulus and leaves the capsule at the point at which the afferent vessel enters it. Each glomerulus is covered by a layer of flattened, nucleated epithelial cells which dip down between the capillaries, thus separating them from each other. The epithelial cells that cover the capillary loops are readily distinguished from those that dip between them. The former appear as thin arched plates, being concave only on one side, which is attached to the convexity of the loop. The latter appear wedge-shaped.

As regards the capillaries, their walls consist of two distinct coats: first, an external homogeneous membrane, called by Langhans basal membrane; second, an internal nuclear or endothelial layer, the protoplasm of which is so fine and delicate that it is hardly perceptible, but which, in certain pathological conditions, becomes thickened and is readily detected. In slight waxy degeneration of the kidney, these two layers are shown very nicely by staining with methyl-violet. The external homogeneous layer having undergone waxy degeneration, is stained violet, the internal layer being normal, is stained blue. With reference to the nuclei of the endothelial cells, they are few and far between, as first pointed out by Ribbert. Most authorities agree that there is no connective tissue situated between the capillary loops and the glomerulo-epithelium, as first described by Axel Key and still sustained by Klein and others. In an examination of over one hundred teased and cut sections, the writer was unable to detect the slightest evidence of connective tissue between them.

To Klebs is due the honor of first calling the attention of the profession to the fact that the glomeruli were affected in scarlatinal nephritis, and that the diminution and suppression of urine was produced by a compression of the capillaries, caused by a proliferation of the capsular epithelium and the connective tissue cells that are situated between the capillary loops.

Klein, in an examination of twenty-five bodies of those dead of scarlet fever, observed the following microscopical changes in the kidney: first, increase of nuclei, probably epithelial, covering the glomeruli; second, hyaline degeneration of the elastic interna of the minuter arteries, especially the afferent arterioles. The interna of these vessels appear, from place to place, swollen up into cylindrical or spindle-shaped hyaline masses, which produce a distinct narrowing of the lumen of the vessel. This hyaline degeneration was also observed in some of the capillaries of the Malpighian corpuscles, in the course of which greater or smaller parts become obliterated. The degenerated parts are at first hyaline, later on they assume a more fibrous aspect. A third change that he observed was multiplication of the nuclei of the muscular coats of minute arteries, and a corresponding increase in the walls of these vessels. Klein does not think that the anuria and uræmic poisoning are due to compression of the vessels of the glomerulus, but attributes them to the changed state of the arterioles.

Langhans' results in the examination of twelve cases were as follows: The capillaries were dilated, cloudy and filled with a rich nucleated protoplasm. Long and cross sections of the capillaries show in their lumen a reticulum with fine meshes, in which

Digitized by Google

there were occasionally red and white blood corpuscles. In some capillaries thickening of the endothelial layer was only noticed. There was also swelling of the glomerulo- and capsular epithelium, with slight desquamation of the former.

Ribbert, in his examination, came to the following conclusion: That glomerulo-nephritis consists in a swelling and desquamation of the glomerulo- and capsular epithelium, with more or less accumulation of the same in a capsular space. The nucleated mass in the lumen of the capillaries he regards as thrombosed white blood corpuscles. The writer's observations are based upon nine cases of glomerulo-nephritis that occurred at the Allgemeines Städtisches Krankenhaus, in Berlin, and were conducted at the pathological laboratory of the late Prof. Carl Friedländer, to whom he was indebted for the above material. The results of the post-mortem examinations were briefly as follows:

CASE I.—A. K., aged ten years. Sudden death on the twelfth day after beginning of the first symptoms of scarlatina. No albumen or casts were found in the urine. Strong built child. Slight œdema of the lower extremities. No fluid in the abdominal or pleural cavities. Hypertrophy and slight dilation of left ventricle. No valvular lesions. Weight of the heart 185 grammes. Bloody imbibition of the endocardium. Left lung showed hypostatic congestion. Extreme redness and swelling of the larynx. No diphtheritis; spleen swollen. The kidneys were slightly enlarged and the cortical substances were hyperæmic. The glomeruli were very pale.

CASE II.—M. K., aged eight years and three months. General anasarca. Weight of body 25,840 grammes. Fluid was found in the abdominal, pleural and pericardial cavities. Weight of heart, 155 grammes. Extensive hypertrophy of the left and dilatation of the right ventricles. No valvular lesions. The kidneys were enlarged and the glomeruli were pale and prominent. The cortical striæ were somewhat obliterated. The brain showed evidence of a pachymeningitis. Suppuration. The ventricles were small, and contained an excess of clear fluid.

CASE III.—J. I., aged eight years. Death in the sixth week of the disease. Œdema of the lower extremities noticed in the third week. No fluid in the abdominal, pleural, or pericardial cavities. Excessive hypertrophy of the left ventricle. No valvular lesions. Weight of heart, 180 grammes. Kidneys were slightly enlarged. The glomeruli were prominent and pale. No other changes.

CASE IV.—M. B., aged three years and six months. Death in the third week of the disease. Albuminuria in the second week of disease. Œdema was present during the last three days of life. Slight œdema of lower extremities and face. Excessive hypertrophy and dilatation of the left ventricle. Weight of heart, 110 grammes. The kidneys were swollen, the cortical substance was hyperæmic, and the glomeruli were slightly enlarged and pale.

CASE V.—F. S., aged eight years. Death in the fourth week of the disease. General anasarca. Fluid in abdominal and pleural cavities. Œdema of the lungs. Left lobe showed hypostatic pneumonia. Weight of heart, 163 grammes. Hypertrophy of both right and left ventricles. No valvular lesions. Kidneys were not enlarged. They were apparently normal, with the exception of the glomeruli, which were very pale.

CASE VI.—C. S., aged six years. No œdema. Fluid in the abdominal and pleural cavities. Both ventricles of the heart hypertrophied. Weight, 150 grammes. Œdema and congestion of both lungs. Swelling and congestion of the mucous membrane of the larynx and trachea. The kidneys were enlarged. Medullary and cortical substances were slightly congested. The glomeruli were enlarged and pale.

CASE VII.—R., aged twenty-nine years. Œdema fourteen days before death. There was fluid in the pleural and the abdominal cavities, with œdema of lungs. Left ventricle of the heart greatly dilated and hypertrophied. Weight of heart, 430 grammes. Kidneys were enlarged, the cortical substance was swollen and congestive, and the glomeruli were very pale.

Digitized by Google

CASE VIII.—D., aged fourteen years; death in the fourth week of the disease; œdema of the lower extremities and face; fluid in the pleural and abdominal cavities; left ventricle of the heart slightly enlarged; extreme swelling and inflammation of the larynx and trachea; spleen enlarged; the kidneys were smooth and the cortical substance was apparently normal; the glomeruli were pale and prominent.

The record of the ninth case was, unfortunately, lost. Then, to recapitulate, the following are the microscopical changes: The kidneys are enlarged and hyperæmic; there is no loss of cortical striæ, and in some cases the cortices may be somewhat thickened. The glomeruli are more or less enlarged, pale and prominent.

Microscopical appearance shows that the uriniferous tubules are apparently normal. Occasionally, now and then, one sees evidences of a slight parenchymatous inflammation. When examined with a low power, the glomeruli are found larger than normal and covered with a mass of nuclei. With a higher power they are apparently bloodless. You very rarely see a red blood corpuscle in the lumen of a capillary, especially when stained deeply with eosin, which stains the red blood corpuscles a rose color. With an immersion lens the following is observed: In some capillaries the only change that is noticed is a thickening of the endothelial layer, which becomes more granular. In others the lumen of the capillaries are filled with a rich nuclear protoplasm. These nuclei are smaller and darker than the nuclei of white blood corpuscles. Some resemble somewhat one of the pictures of nuclear segmentation. The question that arises is, if they are not the nuclei of white blood corpuscles, what are they? My answer to the same is that they are the nuclei of proliferated endothelial cells. Again, I have seen an endothelial cell, not a glomerulo-epithelial cell, swollen and projecting into the lumen of a capillary like a cubical epithelial cell and completely obliterating its caliber.* In certain loops you have the lumen filled with a reticulum, as described by Langhans, in the meshes of which you have red and white blood corpuscles.

In reference to the glomerulo-epithelium, I have noticed swelling and proliferation of the same. It is still considered by some authorities that it is a proliferation of the glomerulo-epithelium that produces a compression of the capillaries, thereby obstructing the circulation of the blood in the same. In all the specimens that I examined, although proliferation of the glomerulo-epithelium was present, the loops of the capillaries were larger, as a rule, than normal, showing that the pressure was from within and not from without. Although proliferation of the capsular epithelium occurs in glomerulo-nephritis, I can only say that I did not observe it in 182 sections that I examined. I think we can safely say that it is a proliferation and thickening of the endothelial cells that is producing an obstruction of the blood through the capillaries. The vasa afferentiæ are occasionally found dilated to two and three times their normal diameter. This is due to the obstruction in the loops. You have noticed, in the records of the autopsies, that hypertrophy of the left ventricle of the heart existed in each case. This was first pointed out by Friedländer, and is probably due, I think, to two causes: First, changed state of the blood, due to the existence in it of some toxic element, whatever it may be being still questionable. Second, obstruction to the circulation of the blood in the Malpighian bodies, which compel the left side of the heart to do more work on account of the damming back of the blood. Before concluding, I would like to state that the term glomerulo-nephritis ought to be limited to those affections in which there is an obliteration of the loops of the capillaries in the Malpighian bodies, and not applied to those affections in which there is only a proliferation and desquamation of the glomerulo- and capsular epithelium, as this change has been noticed in all forms of chronic nephritis.

---

* The microscopical sections were seen by Profs. Weigert, Ziegler, Von Recklinghausen, Friedländer and Vanwerck, and all agreed to the writer's description of the same.

Digitized by Google

ÜBER ACETONURIE BEI KINDERN.

ON ACETONURIA IN CHILDREN.

DE L'ACÉTONURIE CHEZ LES ENFANTS.

VON DR. ADOLPH BAGINSKY,

Privatdocent an der Universität Berlin.

Nachdem von Jacksch das Auftreten von Aceton (CH_3 CO CH_3) und Diacetsäure (CH_3 CO CH_2 COOH) im Harn von Kindern, welche hochgradig fiebern und unter Convulsionen erkrankt sind, erwiesen hat, war es wichtig, die Frage der Acetonurie der Kinder einer eingehenden Untersuchung zu unterziehen. Bei den klinisch zu beobachtenden Beziehungen zwischen Rachitis, Laryngospasmus und Eclampsie war innerhalb dieser Untersuchung auf die Bedeutung der Acetonämie für die Entwickelung der Rachitis besonders zu achten.

Zum Nachweis des Aceton im Harn dienten die jedesmal neben einander angewendeten Reactionen von Lieben (Bildung von Iodoform bei Zusatz von Jodjodkalium zum alkalisch gemachten Harndestillat), von Legal (carmoisin-rothe, bald veränderliche Farbe auf Zusatz von Nitroprussidnatrium zum schwach alkalischen Harndestillat), von Reynolds (Lösungsvermögen des Harndestillats für frisch gefülltes Quecksilberoxyd). Nur wo diese drei Reactionen sämmtlich eintreten, wurde die Anwesenheit von Aceton im Harn angenommen.

Die Untersuchungsergebnisse waren folgende :—

1. Bei ganz gesunden Kindern konnte in geringen Mengen Aceton im Harn nachgewiesen werden ; indess ist die Anwesenheit des Körpers nicht constant, vielmehr kann derselbe im Harn auch fehlen.

2. Im Harn von Kindern, welche hochgradig fiebern (bei verschiedensten Krankheiten), findet sich reichlich Aceton ; auch geht der Acetongehalt analog der Fieberhöhe und verschwindet mit dem kritischen Temperaturabfall.

3. Kinder, welche unter heftigen eclamptischen Anfällen erkrankt sind, lassen einen ausserordentlich reichen Gehalt von Aceton im Harn erkennen.

Ueber die Quelle dieser Arten von Acetonurie wurden (zum Theil aus Thierversuchen) folgende Thatsachen ermittelt :—

4. Wenn man Thiere mit stickstoffreichem Nahrungsmaterial füttert, so tritt bei denselben reichliche Acetonurie auf. Das Aceton verschwindet aber aus dem Harn vollständig bei möglichst stickstoffarmer Nahrung (Amylaceen und Fett).

5. Künstliche Behinderung der Respiration und selbst bis zur Asphyxie getriebene Kohlenoxydvergiftung bewirkt bei stickstoffarmer Kost keine Acetonurie. (Versuch am Hunde.)

6. Weder in den Fäces, noch im Mageninhalt dyspeptischer Kinder lässt sich Aceton nachweisen.

7. Bei der Milchsäuregährung treten so geringe Mengen von Aceton auf, dass diese nicht als die Quelle der Acetonurie angesehen werden kann.

Aus diesen Thatsachen ist zu schliessen, dass die Acetonurie ihre Quelle in einem eigenartigen Zerfall der Eiweisskörper hat.

Ueber die Beziehungen der Acetonbildung zur Rachitis und den dieselbe begleitenden nervösen Symptomen, wurden im Wesentlichen nur negative Ergebnisse gefunden. Es stellte sich heraus :—

8. Kinder, welche an Rachitis leiden und laryngospastischen Attaquen unterworfen

Digitized by Google

sind, zeigen entweder keine Acetonurie, oder Aceton tritt nur in geringen Spuren im Harn auf. Auch bei Chorea wurde Aceton im Harn vermisst.

9. Langdauernde reichliche Fütterung von Aceton und von Acetessigäther (CH_3 CO CH_2 COO C_2 H_5) erzeugt bei jungen Hunden weder eclamptische Krämpfe, noch Rachitis. Die Thiere bleiben durchaus gesund; auch eine Erkrankung der Nieren tritt nicht ein.

10. Die Stoffwechselanomalie, welche die Rachitis erzeugt, steht sonach mit dem die Acetonurie bedingenden Eiweisszerfall in keinem direkten Zusammenhange.

[ABSTRACT.]

ON ACETONURIA IN CHILDREN.

BY DR. ADOLPH BAGINSKY,

Of Berlin.

After von Jacksch had detected the presence of acetone (CH_3 CO CH_3) and diacetic acid (CH_3 CO CH_2 COOH) in the urine of children affected with high fever and convulsions, it was important to submit the question of acetonuria of children to a more thorough examination. In the relationship of rachitis, laryngospasm and eclampsia, clinically to be observed within the scope of this examination, particular stress was to be laid on the importance of acetonuria for the development of rachitis.

To prove the presence of acetone in the urine in each case, the reaction of Lieben (formation of iodoform on addition of iodized iodide of potassium to the distillate of alkalized urine), of Legal (crimson color readily changeable by the addition of nitrocyanide of sodium to the slightly alkaline distillate), of Reynolds (capacity of the distilled urine to dissolve freshly precipitated oxide of mercury), were employed.

Only when these three reactions took place, the presence of acetone in the urine was accepted.

The results of the examination were as follows:—

1. In perfectly healthy children very small quantities of acetone could be shown in the urine; the presence of this body, however, is not constant, and it may be absent.

2. In the urine of children with high fever (in the most different diseases) acetone is found abundantly; the quantity of the acetone rises with the fever and disappears with the critical fall of the temperature.

3. Children with violent eclamptic attacks show an extremely high amount of acetone in the urine.

In regard to the source of these different forms of acetonuria, the following facts were ascertained (partly from experiments on animals):—

4. If animals are fed with nutriment rich in nitrogen, an abundant acetonuria is developed, but the acetone disappears completely by feeding with substances as free as possible from nitrogen. (Amylacea and fat.)

5. Artificial checking of respiration carried even to the point of asphyxia, by poisoning with carbonic oxide, does not produce acetonæmia when slightly nitrogenous food is given. (Experiment on dogs.)

6. Neither in the fæces nor in the contents of the stomach of dyspeptic children can acetone be demonstrated.

Digitized by Google

7. By lactic acid fermentation so small quantities of acetone are produced that it is not to be regarded as the source of acetonuria.

From these facts it may be concluded, that acetonuria has its source in a peculiar disintegration of albuminoid substances.

Regarding the relationship of the development of acetone to rachitis and concomitant nervous symptoms, essentially negative results were obtained. It was found that—

8. Children affected with rachitis and subject to laryngospastic attacks show either no acetonuria or acetone appears in slight traces only in the urine.

9. Long-continued bountiful feeding with acetone or acetic ether (CH_3 CO CH_2 COO C_2 H_5) caused in young dogs neither eclamptic attacks nor rachitis. The animals remained perfectly healthy, and no affection of the kidneys followed.

10. The abnormal nutritive metamorphosis producing rachitis is, therefore, in no direct connection with the disintegration of albumin causing acetonuria.

A STUDY OF SOME OF THE BACTERIA FOUND IN THE DEJECTA OF INFANTS AFFLICTED WITH SUMMER DIARRHŒA.

UNE ÉTUDE DES BACTÉRIES TROUVÉES DANS LES DÉJÉCTIONS D'ENFANTS AFFLIGÉS DE LA DIARRHÉE D'ÉTÉ.

UNTERSUCHUNG EINIGER DER BAKTERIEN IN DEN ENTLEERUNGEN VON KINDERN, WELCHE AM BRECHDURCHFALL LEIDEN.

BY WILLIAM D. BOOKER, M. D.,
Baltimore, Md.

The term summer diarrhœa is used in this article to include conditions usually classified as functional or non-inflammatory diarrhœa, gastro-enteric catarrh, dysentery and cholera infantum. The transition from one of these conditions to the other is often so gradual and their symptoms are so often blended that it is not permissible, or even possible, to draw sharp lines of distinction between them.

The great increase of diarrhœal affections in children with the increased heat of summer was observed first by American physicians, and a thorough report upon the subject was made by Benjamin Rush in 1789.* For a long time this disease was supposed to be confined to America, but since the middle of the present century it has been recognized in England, Germany, France and other countries. During the last twenty-five years the European contributions to the literature of cholera infantum are not less than those from American sources.†

Of the causes which lead to the greater frequency and severity of these affections in summer, the effect of heat has been naturally considered to be the most important, but in what manner this influence is exerted is still a subject of discussion.

That microörganisms are in some way concerned in the causation of the group of affections embraced under the name summer complaint or summer diarrhœa of children, is a view which has often been suggested and which, of late years, has gained favor. The increasing popularity of this mycotic theory is based more upon the advance

---

* B. Rush. "Medical Inquiries and Observations," Philadelphia, 1789. The article "Cholera Infantum" bears the date 1773.

† A full bibliography of cholera infantum, up to the year 1882, is to be found in the Index Catalogue of the Library of the Surgeon-general's Office, United States Army, Vol. III, p. 148, et seq.

Digitized by Google

in our knowledge in general concerning the relation between bacteria and infectious diseases, than upon any actual discoveries of specific pathogenic organisms in the diseases now under consideration.

In the absence of any such actual discoveries the following facts have been urged, with more or less plausibility, in favor of the mycotic theory. Under favoring external conditions the summer diarrhœa of children occurs as an epidemic. It is not possible to explain at least a large number of the cases by the anatomical lesions found after death, which are often entirely disproportionate to the severity of the symptoms during life, and when present may often be regarded as secondary. It is generally admitted that abnormal processes of fermentation and of decomposition in the gastric and intestinal contents play an important rôle in the etiology and symptomatology of these affections, and these processes outside of the body have been proven to depend upon the presence of various low vegetable organisms. The influence of heat is no less apparent in favoring the development of these processes than in favoring the development of the summer diarrhœa of children.

In view of these circumstances, and others of a similar purport might be adduced, it is not unnatural that many investigators have directed their attention to the microscopic study of the dejecta, in the hope of finding there microörganisms which might be regarded as the cause of the disease. Although these researches brought to light the fact that the diarrhœal stools are swarming with a large number of various forms of bacteria and of fungi, no positive conclusions could be drawn from this fact, inasmuch as the normal stools also contain countless bacteria. Indeed, many observers went further than this, and drew the unwarrantable inference that bacteria cannot be concerned in the causation of diarrhœal affections, because the normal stools as well as the diarrhœal stools contain an enormous number of various bacteria. This inference is unwarrantable, inasmuch as the mere microscopical examination of the stools affords only imperfect information as to the morphological characters, and no information at all as to the biological characters of the organisms present, and it may well be that in diarrhœal stools organisms with specific pathogenic properties are present which do not exist in healthy stools.

Evidently the only way in which to attack with any hope of success the problem as to the significance of the microörganisms in the diarrhœal stools of children is to isolate so far as possible the various species according to the modern bacteriological methods, which we owe to Koch. The organism thus isolated must be studied morphologically and biologically, more especially with reference to their fermentative and pathogenic properties. Furthermore, such organisms must be compared with those found in healthy stools, and the frequency of their presence in diarrhœal stools and their relation to different varieties of summer diarrhœa of children must, if possible, be established.

Before the work thus outlined can be satisfactorily prosecuted, it is necessary to become familiar with the bacteria found normally in the stools. It is from this point of view that Bienstock, Stahl, and especially Escherich, have made their valuable investigations of the bacteria in normal fæces.* Previous observers confined their

* Bienstock. *Fortschritte d. Medicin*, 1883, p. 609 (preliminary communication), and *Zeitschrift f. klin. Med.*, Bd. VIII, p. 1. Stahl. Verhandl. d. Congresses f. Innere Medicin 3d Congress, p. 193, 1884. Escherich. *Fortschritte d. Medicin*, 1885, Nos. 16 and 17, and "Die Darmbacterien des Säuglings," Stuttgart, 1886.

Bienstock was the first systematically to isolate and study by the bacteriological methods the bacteria of the fæces. Stahl isolated twenty varieties, concerning which only a brief notice was published. His work, which was undertaken under Koch's direction and which promised much, was cut short by his death.

Individual species of bacteria have been isolated from the fæces by Brieger, Koch, Finkler and Prior, Miller, Kuisl, Weisser, and others.

Digitized by Google

examinations to the microscopical study of the stools. This method of examination is indispensable, but for reasons which have already been mentioned it has hitherto shed little or no light upon the problem before us. Hence it is that we pass over without further comment the painstaking microscopical examinations of the stools made by Hausmann, Szydlowski, Woodward, Nothnagel, Uffelmann, Baginsky and others.

As the bacteria in the fæces of adults living upon a mixed diet differ greatly in number and in kind from those in the healthy fæces of milk-fed infants, and as our interest is solely with the latter, we pass at once to the consideration of the bacteria present in the stools of healthy sucklings. We owe to Escherich the fundamental investigation of this subject. His researches are of great value and have been made according to the most approved bacteriological methods. These researches have laid the foundation for a fruitful study of the bacteria in the pathological stools of infants.

Escherich found, as had also previous observers, that our present bacteriological methods do not enable us to isolate in the form of pure cultures all of the bacteria existing in the fæces. There is therefore a discrepancy between the results of microscopical and those of bacteriological examination of the fæces, the latter method showing a smaller variety of organisms than the former. For this reason, if for no other, it is plain that the bacteriological cannot displace the microscopical examination. The two methods must be used in combination with each other. Efforts to obtain culture media more suitable than those in common use for the cultivation of a larger number of fecal bacteria have not hitherto been successful.

Escherich proved the correctness of the common belief that the meconium of the newborn infant is entirely free from microörganisms. After a variable period bacteria make their appearance in the meconium, and usually by the second day after birth they are present in large number. The chief mode of ingress of bacteria to the intestine is through the air, saliva and food which are swallowed, but inasmuch as bacteria are sometimes found in the meconium taken from the rectum three to seven hours after birth, Escherich believes that they may enter per anum. The bacteria found in meconium stools are fewer in number but greater in variety than those in the subsequent milk fæces.

While bacteria are present in enormous number in the fæces of healthy milk-fed infants, it was found that two species of bacteria are constantly present, and that of these one species so greatly preponderates that it is sometimes found almost as a pure culture. The bacteria constantly present are the bacterium lactis aerogenes and the bacterium coli commune. These are designated obligatory milk-fæces bacteria, in distinction from the inconstant bacteria, which are called facultative or potential milk-fæces bacteria. The bacterium lactis aerogenes is present greatly in excess of the colon bacterium in the upper part of the small intestine, but in the lower part of the small intestine and throughout the colon it diminishes in number, and in the healthy stools only comparatively few individuals of this species are found. On the other hand, the bacteria coli commune* becomes more and more numerous toward the end of the intestine and vastly exceeds in number all other varieties of bacteria in the stools.

The obligatory milk-fæces bacteria are capable of growing with the production of fermentation, without oxygen. The bacterium lactis aerogenes causes lactic acid fermentation of milk sugar, with the development of CO_2 and H, and there is every reason to believe that it is this organism which causes this species of fermentation, which has been proven to occur normally in the infant's intestine. The colon bacterium, which is found also in the meconium and in fæces from a mixed diet, probably causes

* Escherich thinks it possible that under this name several closely allied species are included, as he found some differences in the appearances of the colonies on gelatine plates.

Digitized by Google

some species of fermentation or of decomposition in the secretions of the large intestine, perhaps in the mucus.

The potential or inconstant milk-fæces bacteria are for the most part aerobic, and so far as they develop at all in the intestinal canal, probably grow in the peripheral layer, which contains a small amount of oxygen. The inconstant bacteria are, as a rule, somewhat more numerous in the fæces of infants fed with cow's milk than in the stools of sucklings.

Two facts of the greatest importance are brought to light by Escherich's investigations, viz.: the remarkable and unexpected simplicity and uniformity of the bacterial vegetation in the healthy fæces of milk-fed infants, and the variation in this vegetation which occurs with a change in the quality of the diet.

At first glance it is not easy to understand why, of the manifold varieties of bacteria which gain access to the infant's intestine, only two should develop there constantly and in large number, but this difficulty is less considerable when we consider that an organism in order to supply itself with the necessary oxygen and food in the intestine must be capable of causing anaerobic fermentation of the intestinal contents. In harmony with the simple and uniform food of the milk-fed infant, we find a corresponding uniformity in their intestinal bacteria.

It is not possible to foresee to what clinical and therapeutic uses may be put the knowledge of the constant characters of the bacterial vegetation in the normal milk fæces. It seems a justifiable inference that any marked variation in the quality of this bacterial vegetation is an expression of some disturbance in the alimentary tract.

So long as the normal fæces were believed to contain a chaotic mass of all sorts of bacteria, it seemed a useless and unpromising task to make any especial study of the likewise chaotic mass of bacteria in diarrhœal stools. This point of view, however, is now changed. With definite information concerning the more important and constant bacterial species in the normal stools, it has become a matter of the utmost interest to learn what new species of bacteria appear in diarrhœal stools, and what changes occur in the normal bacterial vegetation.

Such knowledge may prove of value in many ways. Hitherto the study of the fecal bacteria in pathological cases has had especially for its object the hope of discovering some specific forms which might be regarded as the essential cause of the disease. This is, of course, a most important object of research, and one which I have kept in view in my investigations. There is, however, another point of view hardly less important and which is based upon the fact that the kind of bacteria found in the fæces vary with the intestinal contents which serve as food for the bacteria. Thus, we find sharply defined differences in the bacterial vegetations characterizing milk fæces, meconium, meat fæces, fæces from a mixed diet, etc. In cases of summer diarrhœa there are abnormal changes in the contents of the stomach and of the intestine, in consequence of morbid secretions, peristalsis and fermentations, and corresponding to these abnormal intestinal contents we may expect to find abnormal vegetations of bacteria, and it is reasonable to suppose that some definite relations may be discovered between certain forms of bacteria and certain definite changes in the intestinal contents. Such a discovery might be of diagnostic and, perhaps, of therapeutic value.

It is evident that we must be very cautious in assuming that any causative relation exists between strange forms of bacteria in the fæces and the existing disease. Various interpretations of such a coincidence are possible. First: it may be that the new forms of bacteria are to be regarded simply as the necessary accompaniment of the altered intestinal contents, and do not influence in any way the disease. Even upon this supposition of their harmless saprophytic nature the study of these bacteria may prove of diagnostic and therapeutic value, for reasons which have already been mentioned. In the second place, the new forms of bacteria, while not the primary cause of the disease, may, by

Digitized by Google

their presence and growth in the intestine, cause a continuance and aggravation of the disease. For instance, we can readily suppose that a gastric or intestinal catarrh, or some abnormality in peristalsis or secretion, induced primarily by error in diet, or by heat, or by some constitutional cause, or by unhygienic surroundings, may be kept up and aggravated by the presence of microörganisms whose continued existence in the intestine is first rendered possible by some abnormality which would otherwise be transitory. In the third place, the bacteria may begin their work outside of the body, by developing in the milk or other food taken by the infant and causing abnormal products of fermentation or decomposition, possibly poisonous ptomaines. In the fourth place, one or more of the species of bacteria found in the stools of infants affected with summer diarrhœa and not found in the normal stools may be the essential and specific cause of the disease in the same sense that the typhoid bacillus is of typhoid fever, or the anthrax bacillus of anthrax. It must be admitted that without further proof of any given case the presumption is against this last supposition.

In view of the very peculiar qualities which bacteria must possess in order to adapt themselves to the conditions of growth in the healthy infant's intestine, and in view of the preoccupation of the field by other bacteria, it is not likely that bacteria which enter the normal intestine will be able to displace those normally existing there. That preparation of the soil which we vaguely call predisposition doubtless plays a most important rôle in the class of diseases now under consideration. Given a favorable soil, such as that resulting from gastric or intestinal catarrh, or from abnormalities in peristalsis or secretion, then bacteria which would fail to gain lodgment in the healthy intestine may grow, and by their presence become the most serious factor in the disease.

The foregoing considerations have led me to undertake the biological investigation of the bacteria found in the dejecta of infants affected with summer diarrhœa.

METHODS PURSUED IN THIS INVESTIGATION.

(*a*) For collecting material: A small, slightly pointed glass tube, sterilized in the flame of a spirit lamp, was introduced into the rectum, and immediately after withdrawing it the contents were emptied into a sterilized test tube plugged with sterilized cotton wadding. As only the contents of the tube were put in the test tube, and the room in which the work was carried on was comparatively free from germs (as shown by the small number of colonies, often none, growing upon agar plates exposed for a considerable time), the purity of the cultures may be reasonably assured. Agar plates were made at once, and all colonies showing the slightest difference were planted in agar tubes. The cultures were then transferred to the pathological laboratory of the Johns Hopkins University, where the further isolation and study of the bacteria were carried on under the general supervison of Prof. Welch and Dr. Meade Bolton, the assistant in the Bacteriological Department.

(*b*) For the study of the morphological and biological characters of the bacteria the modern methods introduced by Koch were employed. The usual solid and fluid nutritive media, such as nutrient gelatine, nutrient agar, steamed potatoes and bouillon were used. As a rule, in place of the old plate method, Esmarch tubes were employed to isolate different species of bacteria, to test the purity of cultures and to observe the characters of the colonies. The investigation of the fermentative properties of the organisms was carried on chiefly with milk, as will be described hereafter.

(*c*) For testing pathogenic properties: The animals used were guinea pigs, young kittens, rabbits, white mice and rats. The experiments consisted in: (1) feeding pure cultures in milk which had been previously sterilized one hour for three successive days in the steam sterilizer. It was fed to mice in small sterilized glass dishes, and small pieces of bread were crumbled in it when the mice would not take the milk culture by itself; 3–8 cc. of the milk was given daily until death occurred, or for five to eight days,

Digitized by Google

when no effect was produced. A similar quantity was fed to the guinea pigs and kittens, through a sterilized pipette. The animals otherwise were fed as usual during the experiments. (2) Hypodermatic and intra-venous injections: The injections were made with a sterilized Koch's syringe. The fluid used for injection was sterilized water or bouillon impregnated with the bacteria and injected at once. (3) In other cases bouillon was inoculated and allowed to stand for several weeks, and then sterilized according to Tyndal's method of fractional sterilization. Hypodermatic injections were made in rats and mice, at the root of the tail; in guinea pigs and kittens about the middle of the abdomen. The intra-venous injections were made in rats in the jugular vein, and in rabbits in the outer vein of the ear.

(*d*) To test if the bacteria were pathogenic in very small quantities, a pocket was made in the skin with sterilized scissors, after trimming off the hair, and by means of a straight platinum needle dipped into a fresh culture a very minute quantity of the culture was taken up and introduced into the pocket under the skin.

Autopsies were made as soon after death as possible, and cultures and cover slip preparations made from different cavities and organs of the body. Before opening the abdomen the hair was singed or cut off, and the skin washed with sublimate. In making cultures from the organs of the body, the needle was introduced through an opening made with heated scissors, so that it could only come in contact with the interior of the organ.

Condition of the children from whom the cultures were obtained.—From 1500 to 2000 children affected with summer diarrhœa are sent every summer from Baltimore city to the Thomas Wilson Sanitarium, ten miles out in the country. Since the opening of the Sanitarium, in 1884, only one case of cholera infantum, according to the classical description, has been brought to it, though a modified form of the disease is not uncommon.* The great majority of the children have acute or chronic gastro-enteric catarrh, and simple diarrhœa, and about one per cent. have dysentery.

The children from whom the fæces were taken for examination were chosen with special reference to the severer forms of these affections, with one perfectly healthy child for control, and one just beginning with a mild diarrhœa, in all, seventeen children.

DESCRIPTION OF THE INDIVIDUAL CASES.

CASE I.—Three months old; fed exclusively on breast milk; has decided stupor, and is extremely emaciated; evacuations frequent, watery, and yellow or greenish in color.

CASE II.—Eleven months old; chiefly cow's milk diet; has high fever, stupor, and very frequent vomiting and purging; evacuations watery.

CASE III.—Nine months old; chiefly cow's milk diet; is emaciated, has slight fever, drowsiness, frequent vomiting and purging; evacuations watery.

CASE IV.—Eight months old; chiefly breast milk diet; sick only a few days; has stupor, vomiting and purging; evacuations rice colored, watery, and mixed with considerable fecal matter.

CASE V.—Eight months old; cow's milk diet; has stupor and persistent vomiting immediately after anything is taken into the stomach; evacuations are not frequent, and are semi-fluid and of yellow color.

CASE VI.—Eleven months old; chiefly cow's milk diet; has frequent vomiting and purging and decided stupor.

CASE VII.—Six months old; condensed milk diet; is emaciated; has drowsiness, frequent vomiting and purging; evacuations green and watery.

CASE VIII.—Five months old; breast milk diet; frequent vomiting and purging.

* True cholera infantum and the modified form of the disease will be classed together in this article as cholera infantum.

Digitized by Google

CASE IX.—Ten months old; chiefly breast milk diet; slightly emaciated; has vomiting and purging; evacuations watery.

CASE X.—Eight months old; cow's milk diet; extremely emaciated; evacuations white and watery, with small quantity of fecal matter.

CASE XI.—Six months old; cow's milk diet; extremely emaciated; evacuations greenish fluid with mucus and white lumps mixed through it.

CASE XII.—Six months old; cow's milk diet; extremely emaciated; has sallow complexion; evacuations greenish and watery.

CASE XIII.—Eight months old; milk diet; is emaciated, restless and has frequent vomiting and purging. Evacuations watery.

CASE XIV.—Eight months old; breast milk diet; the rectum is covered with a croupous deposit; evacuations very frequent and containing a large quantity of pus, sometimes mixed with blood.

CASE XV.—Eight months old; cow's milk diet; emaciated; evacuations frequent and composed of bloody mucus or pus, with a small quantity of fecal matter. The dysentery is secondary to catarrhal enteritis.

CASE XVI.—Seven months old; breast and cow's milk diet; evacuations semi-fluid and have a whitish-yellow color; commencing with a mild diarrhœa; no previous digestive disturbance.

CASE XVII.—Four months old; breast milk diet; has one evacuation daily; the fæces are formed and have a light brown color; has never had any digestive disturbance.

Summary.—The children were from three to eleven months old. Twelve were fed exclusively upon milk and the others chiefly upon milk. Case II had been sick for four or five days with a mild diarrhœa, and true cholera infantum only 24 hours. Cases I to VII inclusive, excepting Case II, had been affected with a modified form of cholera infantum from three days to two or three weeks. In these the nervous symptoms were especially prominent. Cases VIII to XIII inclusive were cases of chronic gastro-enteric catarrh. Case XIV had primary dysentery (croupous), which was confined chiefly to the rectum. Case XV had secondary dysentery. Case XVI, previously healthy, was just beginning with a mild diarrhœa. Case XVII was perfectly healthy.

BACTERIA SEPARATED.

Eighteen different varieties of bacteria have been isolated. The differentiation was made by the morphology of the organisms, by the characters of the growth upon various nutritive media, particularly gelatine, agar and potato, by the action on milk and by the effects of inoculating animals. All the varieties belong to the bacilli except one, which is a coccus. Three varieties of bacilli liquefy gelatine, the others do not liquefy it. The greatest number of varieties were found in the cases of cholera infantum; a larger number in gastro-enteric catarrh than in dysentery; and the smallest number in the healthy child; only one variety being found in it. Eight different varieties, the largest number isolated in any single case, were found in each of two cases. Table I * shows the distribution of the bacteria.

The difference in the number of varieties found in the fæces does not necessarily indicate a similar difference in the intestinal cavity. In the diarrhœal fæces, which come from the upper as well as the lower intestine, and are discharged at such short intervals that no considerable delay can occur in any part of the canal, the conditions are more favorable for obtaining representations from all portions of the canal,

* The table is not yet complete, as not all the cultures from Cases IV, VI, VII, XI have been completely investigated, and, owing to the present imperfect means for differentiating bacteria the identity of some varieties is not positive; this is especially the case with bacillus C.

Digitized by Google

than is the case in the dysenteric discharges, which come chiefly from the lower intestine, and in the healthy fæces which are voided once in 24 hours, and are probably retained in the colon sufficiently long to cause the death of many bacteria which cannot exist for a long time in the large intestine. Nor is it claimed that the cultures obtained represent absolutely all the varieties of bacteria contained in the fæces, as the bacteria may not be equally distributed through the fæces, and their colony growth, especially upon agar, to which we are limited in this climate during the greater portion of the summer for plate cultures, is not always distinctive enough to be recognized. Moreover, not all the bacteria present in the fæces will grow in our ordinary culture media.

TABLE I.

SHOWING THE DISTRIBUTION OF THE BACTERIA.

	Cholera Infantum.							Catarrhal Enteritis.						Dysentery.		Beginning Diarrhœa.	Healthy.
Bacteria	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII
Bacillus A	+	+		+		+											
" B	+	+	+	+	+				+	+	+	+	+				
" C	+	+	+	+	+	+	+	+	+	+	+	+	+			+	+
" D	+	+	+														
" E			+											+			
" F	+		+														
" H		+													+		
" K	+														+		
" L																+	
" M		+			+	+		+				+					
" N		+															
" O										+							
" P															+		
" Q													+				
" R								+									
" S	+	+								+							
" T				+													
Micrococcus V	+		+	+						+	+		+				

CONSTANCY OF THE BACTERIA.

The bacterium coli commune, or varieties of bacteria so closely resembling Escherich's description of it that positive separation has not yet been successful, was found in all the cases except the two of dysentery. It was present in smaller numbers in the more serious cases, especially in cholera infantum, but was found apparently as a pure culture in the fæces of the perfectly healthy child.

One variety was found nearly constantly, and in large quantities in cholera infantum, and not in the dysenteric or healthy fæces. It resembles the description of the b. lactis aerogenes, but the resemblance is not sufficient to be regarded as an identity. A liquefying bacillus, possessing marked pathogenic properties, was found in four cases of cholera infantum, and not in other cases. The variety of micrococcus was found in three cases of cholera infantum and three cases of catarrhal enteritis, and was not found in the dysenteric or healthy fæces. The bacteria found in dysentery were also found in cholera infantum and catarrhal enteritis, but many varieties appeared in the latter which were not found in dysentery.

A rough comparison was made between the healthy and pathological fæces, by studying cover-slip preparations and making cultures from the smallest quantity that could be taken up from the fæces by a straight platinum needle, and then estimating the difference in the number of the colonies. This estimate can only be approximative, as the solid fæces are more adhesive than the liquid fæces, and a larger quantity is taken up by the needle when introduced the same depth. Cover-slip preparations from

Digitized by Google

the fæces of the perfectly healthy child showed almost a pure culture of a bacillus identical with Escherich's drawing and description of the b. coli commune (Die Darmbakterien des Säuglings). Table II, Fig. 1. The bacilli were in immense numbers and the fæces appeared to be composed almost entirely of them. There were, besides these, a few short bacilli joined in twos and resembling Escherich's drawing of the b. lactis aerogenes. No other forms were seen. From the cultures made from the fæces only the form resembling the b. coli commune was obtained. Cover-slip preparations from a child just beginning with a mild diarrhœa showed the b. coli commune and two or three other forms, but the number of bacilli in this case were not so great as in the perfectly healthy child. The cover-slip preparations from the diarrhœal fæces varied in different cases; in some the bacteria were in immense quantity, in others they were not so numerous, but in all the preparations more than two different forms could be recognized. In the plate cultures made it was difficult to distinguish any difference in the number of colonies growing from the healthy and pathological fæces; but always a greater variety of colonies could be seen in the cultures from the diarrhœal fæces.

DESCRIPTION OF THE INDIVIDUAL SPECIES OF BACTERIA.

Bacillus A.—Found in cholera infantum.

Morphology.—Varies somewhat in different stages of growth. In fresh cultures the bacilli are narrow, with rounded ends and varying in length, sometimes growing into long rods or dividing into twos. Fig. 1, Table II. The average size in agar culture twenty-four hours old is about three to four μ long and seven μ wide. In older cultures the bacilli are shorter and smaller. The bacilli show active motion in hanging-drop preparations.

Gelatine Growth.—They liquefy gelatine rapidly at ordinary temperatures; the colonies grow in twenty-four hours to a size large enough to be seen by the naked eye; under the microscope they appear nearly colorless; after reaching a certain size liquefaction begins. The gelatine in stick culture is completely liquefied in three or four days.

Agar Growth.—In stick cultures the surface is covered in a few days with a nearly colorless skim; in the depth is a luxuriant and delicate stalk corresponding to the line of insertion of the platinum needle. The colony growth is characteristic; the colonies are bluish to the naked eye, and have an indistinct halo around them which shades off imperceptibly into the surrounding agar; slightly magnified, the colonies are light brown and the borders indistinct. The surface of the colony has a delicate, wavy appearance.

Potato Culture.—The growth is luxuriant, a dirty brown color, raised slightly above the surface and has well-defined borders.

Blood Serum.—In twenty-four hours a small, white patch is noticed; this gives place, in a few days, to a white semi-fluid sediment around the edge of the tube; in six weeks about one-third of the stick culture is liquefied.

Action on Milk.—Milk is coagulated into a gelatinous mass with alkaline reaction. In a few days the milk is separated into a clear fluid upper stratum and a lower thick gelatinous stratum; as the culture grows older the fluid stratum increases at the expense of the lower thick stratum. In some cultures in which milk was acid when inoculated the reaction afterward became alkaline, but a sufficient number of experiments have not yet been made to be conclusive upon this point.

Pathogenic properties tested by experiments upon lower animals:—

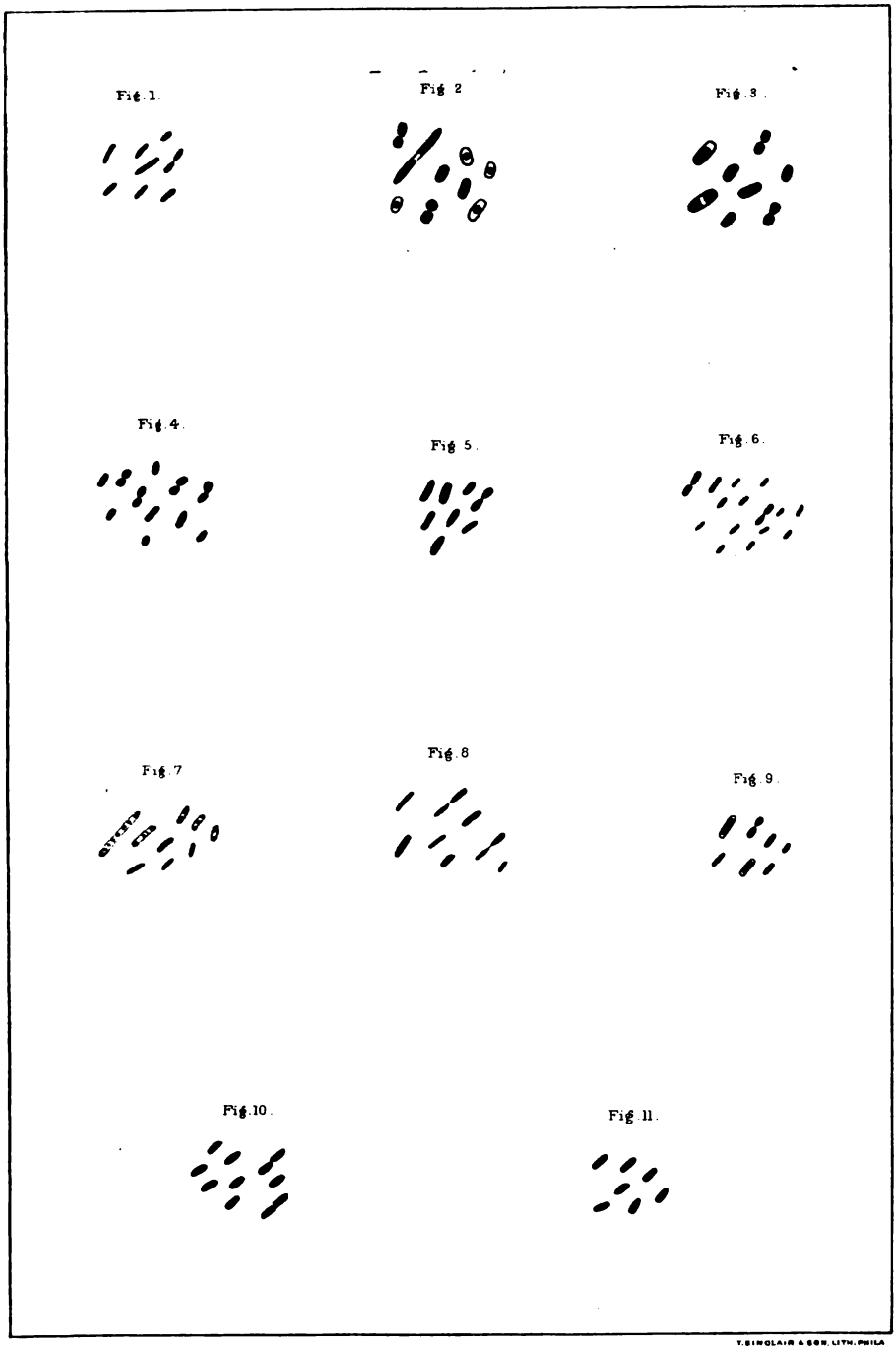
(*a*) Feeding with milk cultures which were from one to six weeks old :—

Experiment 1.—Three mice and three young guinea pigs.

Results.—All died; death occurring in from one to eight days. Autopsies were made in each case as soon after death as possible. No abnormal changes were noticed in any case, except a certain degree of emaciation.

Digitized by Google

TABLE II.



EXPLANATION.—The drawings were all made with Zeiss' homogeneous immersion $\frac{1}{12}$, ocular 3. Magnifying about 650 times.

Fig. 1, Bacillus A, from an agar plate culture 24 hours old. Fig. 2, Bacillus B, from a gelatine plate culture 8 days old. Fig. 3, Bacillus B, from a potato culture 4 days old. Fig. 4, Bacillus C, from a potato culture 4 days old. Fig. 5, Bacillus M, from a potato culture 4 days old. Fig. 6, Bacillus D, from an agar culture 24 hours old. Fig. 7, Bacillus F, from a potato culture 24 hours old. Fig. 8, Bacillus H, from a potato culture 24 hours old. Fig. 9, Bacillus E, from a potato culture 24 hours old. Fig. 10, Bacillus L, from a potato culture 4 days old. Fig. 11, Bacillus K, from a gelatine plate

Digitized by Google

Digitized by Google

Cover-slip preparations showed, in the stomach and intestines, among other bacteria, a bacillus identical in microscopic appearance with the original, and usually in large numbers. From the blood of the heart, spleen, liver, kidneys and peritoneal fluid the bacilli were sometimes found, but not always.

Esmarch tubes from the stomach, intestines and kidneys gave chiefly liquefying colonies. Cover-slip preparations and potato cultures from the colonies were identical with the original. As a rule they were in greater numbers in the small than the large intestines. In the kidneys they were generally pure and in large numbers. From the heart, spleen and liver negative results, except in one case from the heart and one from the liver pure liquefying colonies were obtained.

Only one of the animals, a mouse, lived longer than three days. On the fourth day it was noticed to have frequent discharges, which were soft, of a whitish yellow color and contained some mucus. Esmarch tubes, made from the discharges, had the appearance of being almost pure of the original, and cover-slip preparations from the liquefying colonies showed the same. The diarrhœa continued four days, when the animal died, being very much emaciated. There was no diarrhœa in the other animals.

Control.—Two guinea pigs of the same age as those used were fed, one upon the customary food alone, and the other upon this and sterilized milk. There was no difference between these two, and both continued healthy. Mice were fed with sterilized milk without being affected.

Experiment 2.—A young kitten was fed with milk cultures in the same manner as the guinea pigs, without results; it was then given croton oil until diarrhœa was produced, and fed with the culture without results.

Experiment 3.—Mouse. Milk culture, fourteen days old, sterilized by the *interrupted* method, was given six days without results. The animal was then fed on the infected unsterilized milk without being affected, except a slight diarrhœa which lasted two days. In this experiment the milk was slightly acid when it was inoculated, and the casein was not liquefied to the same extent as happened when it was alkaline at the time of inoculation.

(*b*) Intra-venous injections.

Experiment 1.—Three rats. Bouillon culture, 18 days old and sterilized by the interrupted method. Sterilization was proved by the failure to obtain cultures from it. .5 cc. was injected into a jugular vein of each rat. Results: all died. Death occurring in two and a half to five hours. Autopsies show no changes, except that the blood vessels on the posterior part of the brain appeared congested.

Control.—A rat was injected in a similar manner, with the same quantity of pure sterilized bouillon, without being affected.

Experiment 2.—Rat. A small quantity of an eight-days' agar culture was put into one cc. of distilled water, and .5 cc. of this injected at once into a jugular vein; no results.

Experiment 3.—Rabbit. Milk, treated in the same manner as in Experiment 1, was filtered in a sterilized filter, and two cc. of it injected into the ear vein without results.

(*c*) Hypodermatic injections.

Experiment 1.—Three kittens. One cc. of freshly inoculated bouillon was injected into each kitten.

Results.—Two recovered after being somewhat dull and stupid for several days. The third died on the fifth day.

Autopsy.—The organs of the body appeared healthy. There was suppuration for a space of two centimetres around the point of inoculation. Cover-slip preparations from this point showed a large number of bacilli identical with the original,

Digitized by Google

and Esmarch tubes from it and the peritoneal cavity gave pure cultures of the original.

Experiment 2.—Mouse. .3 cc. of liquefied gelatine culture injected.

Result.—Death in twenty-four hours.

Autopsy.—On the back, just above the point of inoculation, the skin was puffed up with air about the size of a hazel nut. The skin around the point of inoculation was red and appeared to be inflamed. Nothing else abnormal noticed. Cover-slip preparation and Esmarch tubes from the point of inoculation, liver, spleen kidney, peritoneal fluid and heart appeared to be pure cultures of the original.

Experiment 3.—Two rats. .5 cc. of liquefied gelatine culture injected into each rat.

Results.—In one the skin on the posterior half of the back was gradually separated by a process of necrosis. Complete separation, leaving the under surface bare, was effected in two weeks. The edges were clean cut and there was no suppuration. In the other rat a similar but much smaller separation of the skin took place on the side of the thigh and about one inch from the point of inoculation.

Experiment 4.—Mouse. A straight platinum needle dipped into a fresh potato culture was inserted under the skin.

Result.—Death on the sixth day.

Autopsy, immediately after death.—The organs of the body appeared healthy. Cover-slip preparations and cultures from the spleen, heart, liver and peritoneal fluid gave negative results.

SUMMARY.

Bacillus A was found in four cases, one of which was in the stage of stupor of acute cholera infantum, and the others were more chronic cases of a modified form of cholera infantum in which the nervous symptoms largely predominated. It was not among the most numerous bacteria found in the discharge in either case, and owing to this fact it may have been overlooked in other cases. It produces a gelatinous coagulation in the milk, which is afterward more or less completely liquefied, and changed into a light brown color. It leaves the oil globules unaffected. Its pathogenic properties appear to reside in the power of producing injurious products in albuminous compounds, and its action seems to vary according to the manner it is introduced into the blood. Milk cultures fed to mice and guinea pigs proved fatal, while the sterilized milk cultures had no effect when fed. When introduced directly into the blood a small quantity of sterilized bouillon cultures proved rapidly fatal, while the unsterilized fresh cultures injected into the veins of rats had no effect. In all the animals to which it was given more or less drowsiness or stupor was produced. Only one had pronounced diarrhœa, and one a mild diarrhœa. Besides being found in the elementary canal of all the animals that died after being fed upon the culture, it was also found in some cases in other organs, especially the kidneys.

Bacillus B.—Found nearly constantly in cholera infantum and gastro-enteric catarrh.

Morphology.—Short, thick bacilli, with slightly rounded ends, often joined in pairs. The average size is 2–3 μ long and 1 μ wide. Thick, long filaments were often seen. Some of the bacilli and nearly all the long filaments contained a clear, glistening spot, which had every appearance of a spore, but cultures containing these apparent spores were not more resistant to heat than were others. In cultures eight days old the bacilli are longer than they are in fresh cultures, and many of them show when stained with gentian violet a deep violet centre with purple or clear poles. The bacilli often show capsules. The bacilli show no motion of their own in hanging-drop preparations.

Gelatine cultures.—The colonies develop rapidly; the superficial ones are raised

Digitized by Google

and spread out; they are generally round with even borders, though often irregular in shape and white to the naked eye. When seen under the microscope they are loose and granular, and have a yellowish-brown centre with a white border; by the third day this becomes a nearly uniform light brown color, and a few days later a dark central zone displays itself with a light brown peripheral zone. Older cultures show a number of concentric rings of different shades of brown. The deep colonies remain small and have a yellow or brown color. In stick cultures the surface growth is raised, white, and extends about half over the surface; in the depth it has a solid and uniform stalk without any marked end swelling.

Agar cultures.—The colonies develop rapidly, and in forty-eight hours reach a considerable size. They are white to the naked eye, and are round and dome-shaped. Under the microscope they have a light-brown color and are not compact. Neighboring colonies run together without any mark of separation at the point of junction. The deep colonies are small and dark, and have a granular appearance.

Potato cultures.—Cream or pale yellow color, with a raised and nodulated surface, well defined and notched borders and sloping edge. Gas bubbles appear over the surface in two or three days. There is but slight moisture about the growth at first. But slight, if any, difference can be noticed between the cultures on old and new potatoes.

Action on milk.—Milk is coagulated with acid reaction, and sometimes with evolution of gas.

On blood serum a small whitish patch appears in a short time, but instead of continuing to grow as a solid growth, it gives place to a whitish semi-fluid substance seen around the sides of the tube.

Pathogenic properties tested by experiments upon lower animals:—

(*a*) Hypodermatic injections.

Experiment 1.—Mouse. .5 cc. of a fresh bouillon culture injected.

Result.—Death in forty-eight hours.

Autopsy, at once.—Skin around the point of inoculation was red and appeared to be inflamed. Organs of the chest and abdominal cavities appeared healthy. Cover-slip preparations from the point of inoculation, peritoneal cavity, spleen and right auricle, showed a short, thick bacillus identical with the original. Cultures were obtained from the point of inoculation and from the peritoneal fluid.

Experiment 2.—Guinea pig, injected with two cc. of fresh bouillon culture.

Result.—Death in twenty-four hours.

Autopsy.—Slight œdema and redness of the skin around the point of inoculation. The peritoneal cavity contained about ten cc. of a grayish-white viscid fluid which began to escape as soon as the cavity was opened. There was general peritonitis and the stomach and intestines were congested. The other organs of the abdominal cavity and those of the thoracic cavity appeared healthy. Cover-slip preparations from the peritoneal fluid showed an enormous quantity of pus cells and a pure culture of the bacillus; some of the bacilli were around the pus cells and some appeared to be in the cells. Cover-slip preparations from the right auricle also showed a pure culture of the original. Cultures from the peritoneal fluid, spleen and right auricle gave pure cultures of the original.

Experiment 3.—Rat and mouse. Five-tenths to one cc. of bouillon culture, obtained from the right auricle of the guinea pig, was injected into each.

Results.—The rat died in forty-eight hours and the mouse on the fourth day.

Autopsy of the rat showed a small inflamed spot at the seat of inoculation, and the body somewhat emaciated. The organs of the body appeared healthy. Cover-slip preparations and cultures made from the heart, spleen, kidney, peritoneal fluid and point of inoculation only succeeded from the spleen, peritoneal fluid and point of inoculation; in these the original bacillus was obtained pure.

Digitized by Google

Autopsy of the mouse.—The body was very much emaciated and the skin was separated, by sloughing, from the abdomen and right thigh. The lymphatics of the mesentery were white and distended and the receptaculum chyli was very large and filled with a white fluid. The organs of the body appeared healthy. Cover-slip preparations from the abscess and receptaculum showed a large number of bacilli identical with the original.

Experiment 4.—Rat injected with .5 cc. and a kitten with one cc. of fresh buillon culture, with negative results.

Experiment 5.—Two mice. A straight platinum needle was dipped into a fresh potato culture and inserted under the skin.

Results.—One mouse was unaffected; the other died on the sixth day.

Autopsy showed nothing abnormal, and cover-slip preparations and cultures were negative.

(*b*) Feeding with milk cultures.

Experiment 1.—Three mice.

Results.—Death in two to four days.

Autopsies.—No pathological lesions were noticed in any case. Cover-slip preparations from the stomach and duodenum showed a larger proportion of short, thick bacilli, resembling the original, than the lower part of the ileum. In some preparations the cover-slip preparations from the stomach and duodenum looked like almost pure cultures of the original. A bacillus identical with the original was also found in the cover-slip preparations from the heart, spleen, liver and kidney; in these it was generally the only bacillus found, and pure cultures were obtained from these organs. Esmarch tubes made from the contents of the stomach and intestine gave a large proportion of the colonies in every way resembling the original, and this proportion appeared to be greater in the upper than in the lower intestine.

Experiment 2.—Mouse, young guinea pig and kitten. Negative results. Artificial diarrhœa was produced in the kitten, with croton oil, and the milk culture again fed to it with negative results.

Summary of Bacillus B.—Found nearly constantly in cholera infantum and catarrhal enteritis, and generally the predominating form. It appeared in larger quantities in the more serious cases. It was not found in the dysenteric or healthy fæces. It resembles the description of the b. lactis aerogenes, but the resemblance does not appear sufficient to constitute an identity and, in the absence of a culture of the latter for comparison, it is considered a distinct variety, for the following reasons: Bacillus B is uniformly larger, its ends are not so sharply rounded, and in all culture media long, thick filaments are seen, and many of the bacilli have the protoplasm gathered in the centre, leaving the poles clear. There is some difference in their colony growth on gelatine, and in gelatine stick cultures bacillus B does not show the nail-form growth with marked end swelling in the depth. In potato cultures the b. lactis aerogenes shows a difference between old and new potatoes, while bacillus B does not show any difference.

Bacillus B possesses decided pathogenic properties, which was shown both by hypodermatic injections and feeding with milk cultures.

Bacillus C.—Found nearly constantly in the stools of cholera infantum, catarrhal enteritis and the healthy suckling. It appeared in diminishing quantity the more serious the case of diarrhœa. It answers so fully to the description of the b. coli commune that the two are regarded as identical. The difficulty which Escherich met with in being always able to consider his culture of the b. coli commune as pure, and yet unable to separate it into more than one variety, was experienced by myself with the cultures of this variety obtained from different children. Cultures of bacillus *C* taken from different children would sometimes, on the same medium, appear identi-

Digitized by Google

cal, and at other times, with all the conditions for the two cultures as nearly alike as it was possible to have them, each would appear sufficiently different to constitute distinct varieties if the difference had remained constant. It is highly probable, as Escherich suggested, that instead of this being one variety it may prove to be a group of varieties, or a family of closely allied organisms.

Bacillus D.—Found in cholera infantum and chronic gastro-enteric catarrh.

Morphology.—Small, narrow bacilli, varying in length and thickness at different stages of growth. In the fæces, and in fresh blood serum cultures they are narrow and have very small, round, clear spaces throughout the body of the cells, which give a granular appearance when seen with Zeiss homogeneous immersion objective one-twelfth and eye piece I. In fresh gelatine cultures the cover-slip preparation has the appearance of being mixed; large rods with vacuoles and staining deeply, and small, narrow rods which do not stain so deeply being seen; but repeated plating failed to separate them into two varieties. The bacilli show no motion of their own in hanging-drop preparations.

Agar stick growth.—A small, round, white patch on the surface, with a thick stalk in the depth.

Gelatine stick growth.—A colorless, slightly raised patch, with irregular borders on the surface, and a delicate stalk in the depth.

Gelatine colony growth.—The surface colonies on the second day, small, white and beginning to spread; on the third day they have nearly reached their full growth, and are white or bluish white to the naked eye. Microscopically, at first they have a dead white appearance, and as they grow older the central part changes to a light yellow color, and is slightly raised; the borders are irregular and notched and remain white. The old colonies have a uniform light yellow color. The deep colonies remain small and have a uniform light yellow color and granular appearance.

Agar colony growth.—Small, pale yellow colonies, not characteristic.

Potato growth.—Characteristic. A bright golden yellow color, raised and dry surface, and well-defined border. There is no difference in the growth on old and new potatoes.

Action on milk.—No change is produced for three or four days; after this time it becomes more decidedly alkaline, but never coagulates. In six to eight weeks the milk has a brown, transparent appearance.

Pathogenic properties:—

(*a*) Hypodermatic injections.

Experiment 1.—Five mice, two rats and two guinea pigs. Buillon cultures used; .5 cc. injected into the mice, one cc. into the rats, and two cc. into the guinea pigs. Results.—The rats and three mice were unaffected, and two mice and the guinea pigs died, death occurring in 24 hours to four days. Autopsies.—Nothing abnormal was noticed beyond a limited inflammation at the point of inoculation, which was suppurative in the guinea pigs. Cover-slip preparations and cultures from the point of inoculation, peritoneal fluid, spleen and heart in the mice gave a pure culture of a bacillus identical with the original, and the same from the peritoneal fluid and point of inoculation in the guinea pigs.

Experiment 2.—Two mice. A straight platinum needle was dipped into the pure cultures obtained from the mice that died, and inserted under the skin, with negative results.

(*b*) Feeding with milk cultures. Four mice and two kittens.

Results.—Three mice and the kittens were unaffected, and one mouse died on the seventh day.

Autopsy.—Body emaciated; nothing else abnormal was noticed. Cover-slip preparations from the peritoneal cavity, stomach, intestine, heart and liver were

Digitized by Google

mostly negative. Esmarch tubes from the stomach, duodenum and ileum gave in the second tubes about 100 colonies from each. The great majority of the colonies were identical with the original. Potato cultures made from the colonies were also identical with the original.

Summary.—Bacillus D was found in only three cases of cholera infantum, and was not found in large numbers in any case.

Its pathogenic properties are not strong, as more than half the animals in which it was tested were unaffected by it.

Bacillus E.—Found in cholera infantum, gastro-enteric catarrh and dysentery; it was not found in many cases.

Morphology.—In fresh gelatine cultures they are short, small bacilli, with rounded ends, many joined in twos and some in long filaments. In potato and milk cultures they are longer and some have small, clear spaces in the ends.

Potato cultures have straw or yellow color, raised surface and the borders sometimes defined and sometimes not well defined.

Gelatine plate culture.—The surface colonies are white and not much spread out; under the microscope they have a uniform lemon color. The deep colonies remain small and have a light yellow border with a brown centre. Gelatine stick culture has a slightly raised, smooth and nearly colorless patch on the surface, and a luxuriant stalk with fringed border in the depth.

In agar the colonies are bluish to the naked eye and uniform grayish-white under the microscope. The stick culture has a smooth, white, raised patch on the surface, and a thick stalk in the depth.

Only negative results were obtained in the experiments made upon lower animals with this variety, and milk is apparently unaffected by it.

Bacillus F.—Found in only one case of cholera infantum.

Morphology.—In fresh cultures they are small, short bacilli, which do not stain deeply with a few longer and thicker forms which take a deeper staining; in old cultures small, clear spaces are seen throughout the body of the cell. In some cultures, especially milk, very long filaments are seen.

Potato growth.—A moist, dirty brown and sometimes glossy surface, but slightly raised, with borders generally defined.

Gelatine growth in stick culture.—A dry, light brown patch on the surface, with a very delicate growth in the depth. The growth is very slow and never very extensive on the surface. The colonies also grow very slowly and seldom reach a large size. Microscopically, the surface colonies are white, glistening and coarsely granular; as they grow older they show a light yellow centre surrounded by darker concentric rings; the deeper colonies remain small and are yellow and granular.

Agar growth in stick culture.—Small, moist, glass-colored patch, slightly raised and lobulated surface, with well-developed stalk in the depth.

Colony.—The surface colonies are large and round, and have a ropy consistency. Microscopically, they are yellow and have a wavy surface.

It renders milk more alkaline without coagulation. Old milk cultures have a brown, transparent appearance.

Pathogenic Properties:—

(*a*) Hypodermatic injections.

Experiment 1.—Two mice. .5 cc. of melted gelatine culture injected.

Results.—Death in 24 and 48 hours.

Autopsies showed nothing abnormal in either case. Cover-slip preparations and cultures from the point of inoculation gave an enormous quantity of bacilli identical with the original.

Digitized by Google

Experiment 2.—Two mice injected with .5 cc. of bouillon culture, with negative results.

(*b*) Intra-venous injection.

Experiment 1.—Rabbit. One cc. of bouillon culture injected into the outer vein of the ear, with negative results.

(*c*) Feeding with milk culture.—Four mice and one kitten. Negative results. Two of the mice were then given croton oil and again fed on the milk culture.

Results.—Death in 24 and 36 hours.

Autopsies.—No abnormal changes noticed. Cover-slip preparations and cultures from the intestines gave a large number of bacilli and colonies which closely resembled the original. Potato cultures made from the colonies were identical with the original.

Bacillus H.—Found in one case of cholera infantum and one case of dysentery.

Morphology.—In fresh agar cultures they are small bacilli with rounded ends. In fresh potato cultures they are larger and longer, and the ends are more pointed.

Potato growth.—Bright yellow-colored growth, but slightly raised, with dry surface and defined borders.

Gelatine growth.—The stick cultures have a small, colorless patch, but slightly raised above the surface, and a well-developed growth in the depth. In plate cultures the surface colonies are bluish and spread out. Microscopically, they show a white, irregular and notched border, with a yellow, slightly raised centre.

Agar growth.—The stick cultures have a smooth, nearly colorless and but slightly raised patch on the surface, and a thick stalk in the depth. In plate cultures the colonies are round and white, with flat surface. Microscopically, they have a uniform brown color and are not compact.

Action on milk.—Coagulates milk with reaction.

In the few experiments with this variety upon lower animals only negative results were obtained.

Bacillus K.—Found in one case of cholera infantum and one case of dysentery.

Morphology.—Short, small bacilli, varying in size. The growth on potato is not uniform. Sometimes several days elapse before anything is noticed; at other times the culture develops more rapidly and is yellow, defined, raised and dry.

In gelatine plate cultures the colonies are whitish, or nearly colorless, and but slightly spread out. Under the microscope they are uniform, smooth and have a straw color. The stick culture has a small colorless patch on the surface, with a delicate stalk in the depth.

In agar plate cultures the colonies are round and white. Under the microscope they have a uniform light yellow or straw color. In agar stick culture the surface is moist and glassy, with a delicate stalk in the depth.

It renders milk acid, and coagulates it only after standing in the thermostat for five or six days. Pathogenic properties not tested.

Bacillus L.—Found in one case of mild diarrhœa.

Morphology.—Short, thick bacilli; many in the form of the figure eight; a few long filaments sometimes seen. It resembles somewhat the b. lactis aerogenes, but differs from it in important particulars.

In gelatine plate cultures the colonies are usually not spread out, but are thick, with a flat surface, and white to the naked eye. Microscopically they have a uniform light yellow color, or a flat, straw colored, central zone, which is raised above a white border.

On potato the culture is lemon colored, dry and not well defined. It renders milk acid, and coagulates it only when kept in the thermostat at 38° C. for five or six days. Pathogenic properties not tested.

Digitized by Google

Bacillus M.—Found in cholera infantum and gastro-enteric catarrh.

Morphology.—Solid, thick bacilli, about two μ long and one μ wide. Some are joined in twos and some are in long filaments. The colony growth in gelatine is changeable. The surface colonies at times are spread out, white or bluish white, and often showing a concentric arrangement to the naked eye. Microscopically they have the appearance of a large yellow colony placed in a larger white colony with only the irregular border of the latter showing, or they show a concentric arrangement, some of the rings being lobulated and some fringe-like or homogeneous. At other times the colonies are nearly colorless and show under the microscope, at first, a uniform greenish-yellow color with fine concentric markings. As the colonies grow older they show a yellow central zone with fine bar-like markings, and an outer white zone. The deep colonies are small and yellow.

On potato, the growth has a golden-yellow color with a moist, glistening surface, and well-defined borders.

Milk is rendered acid, but is not coagulated. As far as tested it showed no pathogenic properties.

Bacillus M resembles the b. coli commune in many respects, but differs sufficiently to be considered a separate variety. It is larger, and never shows the protoplasm gathered into the centre of the cell with the poles clear. On potato, it has a brighter yellow color, and the colony growth in gelatine, though often resembling, is never identical with the b. coli commune.

Bacillus N.—Found in one case of cholera infantum.

Morphology.—Exceedingly small and short bacilli.

Potato growth.—A faint straw or light yellow-colored growth, with a dry and slightly raised surface and defined border; in some cultures nothing is seen upon the potato.

Gelatine plate cultures.—The colonies are small and glassy or colorless, and are taken up entire with the needle.

Agar growth.—In stick cultures the surface has a small raised patch with a rough surface, and a well developed growth in the depth. The surface growth is drawn in ropes with the needle. In plate cultures the colonies are small. The superficial colonies are bluish to the naked eye; microscopically, a lemon-colored centre which gradually tapers to a white border; the deep colonies are uniform light yellow and remain very small. As far as tested it showed no pathogenic properties.

Bacillus O.—Found in one case of catarrhal enteritis.

Morphology.—Small, short bacilli, many joined in pairs. The potato cultures are dry, yellow, raised and defined, sometimes having a bright yellow color, at others a dark yellow. In gelatine plate cultures the colonies are spread out and have a grayish-white centre with a bluish outer zone. Microscopically they have a yellow centre with a white border. In gelatine stick culture the surface has a small, colorless, slightly raised patch with smooth surface. In the depth is a luxuriant stalk with fringe border. In agar plate culture the colonies are large, round and white. Microscopically they have a light yellow color, and gradually taper from the centre to the border.

It coagulates milk rapidly into a solid, firm clot with acid reaction. Pathogenic properties not tested.

Bacillus P.—Found in one case of dysentery.

Morphology.—Small bacilli with rounded ends, some in long filaments. Potato cultures are yellow, with dry raised surface and well defined borders.

Gelatine plate cultures.—The superficial colonies are spread out and bluish-white. Under the microscope they have a light yellow centre with a white border. As the colonies grow older they have a uniform light yellow color. In agar plate cultures the colonies are large, round and white. Under the microscope they have a light brown

Digitized by Google

centre with a white rim. It coagulates milk more slowly than bacillus *O*, and the coagulation is not as solid or as firm. It renders milk strongly acid. Pathogenic properties not tested.

Bacillus Q.—Found in one case of catarrhal enteritis.

It resembles bacillus B in many respects, but is not identical with it. It is a short, thick bacillus, having a similar form to bacillus B, but does not stain clean like the latter; its borders show fine, fuzzy processes, and the intermediate spaces between the bacilli are stained, showing a peculiar network.

The colony growth in gelatine resembles that of bacillus B, only it is darker and more of a grayish-brown color under the microscope. The potato culture resembles bacillus B, but is more luxuriant and juicy. The most marked difference is in its action on milk. Bacillus Q renders milk acid, and coagulates it only when kept in the thermostat, at 38° C., for five or six days.

Bacillus R.—Found in one case of catarrhal enteritis.

It resembles bacillus Q in its action on milk but resembles bacillus B more closely in other respects. In morphology bacillus R is shorter, smaller, and more rounded ends, often having the appearance of diplococci.

In its growth upon potato and gelatine it resembles bacillus B very closely; the only difference observable is that the colonies are more compact in bacillus R.

The pathogenic properties of bacilli Q and R were not tested.

Bacillus S.—Found in one case of cholera infantum and one case of catarrhal enteritis.

Morphology.—Small bacilli, varying in length, some joined at the ends, forming long chains. On potato the culture is dirty yellow, slightly raised and has well-defined borders.

It liquefies gelatine more slowly than bacillus A, and coagulates milk with an acid reaction.

In agar plate cultures the colonies are blue to the naked eye; under the microscope the colonies are indistinct and whitish.

Pathogenic properties were not tested.

Bacillus T.—Found in one case of cholera infantum.

Morphology.—Plump, oval bacilli.

On potato it has a pretty pink growth with well-defined borders, and the surface is raised, in places, into large gas bubbles. On old potato culture the growth eats into the potato and is lower than the surrounding border. It liquefies gelatine rapidly and coagulates milk with acid reaction. Pathogenic properties were not tested.

Micrococcus V.—Found in cholera infantum and catarrhal enteritis.

Morphology.—Small round cocci, some joined together, forming diplococci.

Nothing is seen, in potato cultures, on the surface of the potato

Gelatine cultures.—Both the plate and stick cultures develop very slowly. In stick cultures a small dry patch is seen on the surface, and limited to the point of inoculation after a considerable time; the growth in the depth is more flourishing. In plate cultures the colonies cannot be seen with the unaided eye before the seventh or eighth day. They are round and have a bluish appearance; under the microscope a uniform lemon or light brown color. The colonies never reach a large size. In agar the growth is also slow. In stick culture the surface growth is limited to a small patch at the point of inoculation. The growth in the depth is not extensive. In plate cultures the colonies remain small and have a bluish appearance. Microscopically, they have a light yellow, granular appearance. It produces no apparent change in milk, and, as far as tested, has shown no pathogenic properties.

Digitized by Google

GENERAL CONSIDERATIONS.

Regarding the work in its present condition as too incomplete to admit of positive conclusions, the following considerations are given as showing the tendency of the results thus far obtained:—

The bacterium coli commune does not disappear in the diarrhœal fæces of infants, but appears to diminish in number, according to the severity of the disease. It, or a variety or varieties closely resembling it, appears to be constantly present, but not as a pure culture or in the same largely predominating form as in the healthy milk fæces.*

It was not found in the dysenteric discharges.

No variety of bacteria has been found which bears the relation of constant or obligatory bacterium to the diarrhœal and dysenteric discharges that the colon bacterium bears to the healthy milk fæces.

One variety was found nearly constantly and generally in the predominating form in cholera infantum and catarrhal enteritis, but was not found in the dysenteric or healthy fæces. This variety resembles the description of the b. lactis aerogenes, but differs from it in certain particulars, and in the absence of a positive culture for comparison, has been regarded as distinct. The identity of the two, however, is rendered more probable by the fact that the b. lactis aerogenes is a constant or obligatory bacterium, and the predominating form in the healthy upper intestine of sucklings, while it gradually diminishes in number or altogether disappears in the lower intestine and fæces; and it is easy to conceive that with the frequent and rapid passage of the diarrhœal discharges through the intestine, a large number of the b. lactis aerogenes might be contained in them and in a suitable condition for obtaining cultures. Should the separation into a distinct variety be confirmed by proper comparisons, then we have a new form possessing many of the characteristics of the b. lactis aerogenes, with marked pathogenic properties, nearly constant in cholera infantum and catarrhal enteritis, which has not been found in the contents of the intestine of the healthy suckling, or their fæces. Should the two forms prove identical, then we have the b. lactis aerogenes occurring more frequently and in much larger quantity in the diarrhœal than in the healthy stools.

The number of varieties of bacteria in the diarrhœal fæces exceeds that of the dysenteric and healthy fæces.

The actual number of individual bacteria in the healthy fæces is as great as in that of the diarrhœal fæces.

The number of varieties of bacteria as well as the number of individual bacteria is less in the dysenteric than in the diarrhœal fæces.

The bacteria found in the dysenteric fæces were also found in the diarrhœal, but a number of varieties were found in the diarrhœal fæces which were not found in the dysenteric.

The bacteria differ from the inconstant varieties found by Escherich in the contents of the healthy intestine of sucklings and their fæces.

The number of varieties of micrococci, liquefying and chromoginic bacteria was less than that found by Escherich in the healthy suckling.

Considerable difference was manifest in the effects produced upon lower animals by the different varieties of bacteria with which experiments were made.

Two varieties appeared to possess marked pathogenic properties; in others the pathogenic properties were less marked and some appeared entirely non-pathogenic.

A liquefying bacillus which was found frequently but not constantly in cholera

* It is probable that some of the cultures which have been considered as identical with the b. coli com. would prove, with better means for the differentiation of bacteria than we have at present, to be different varieties.

Digitized by Google

infantum, and not in other forms of summer diarrhœa or healthy fæces, effected changes in albuminous compounds which proved rapidly fatal when injected in small quantity into the veins of rats, and milk cultures of the same bacillus generally resulted in death when fed to mice and guinea pigs.

Diarrhœa was not a prominent symptom in the animals in which the experiments were made. A pronounced diarrhœa was noticed in only one case.

It was shown by experiment that none of the bacteria were capable of multiplying in ordinary hydrant water, and cultures could not be obtained from the water in any case forty-eight hours after it had been inoculated, and in the great majority twenty-four hours afterward.

All the varieties of bacteria found are capable of thriving in milk. Some varieties produce coagulation of the milk with acid reaction, some render milk acid without coagulation, some render milk alkaline without coagulation, some have no apparent effect upon milk, and one variety coagulates milk with alkaline reaction.

INFANTILE MARASMUS.

MARASME DES ENFANTS.

KINDERMARASMUS.

BY I. N. LOVE, M. D.,

St. Louis, Mo.

In presenting a paper for your consideration, with many misgivings I select the subject of infantile marasmus. I am aware that the subjects of abdominal and other forms of surgery are more alluring, and such as this are, as a rule, unattractive, yet we must remember that nothing in the form of disease is trivial, for a human life is always involved, and all that influences and affects life for good or ill is of the greatest import.

A series of interesting cases met with in private practice during the past few years, compared with other cases occurring in hospital and dispensary practice, have impressed upon my mind the importance of this condition, and the means of antagonizing it.

The term marasmus, like malaria, is a misnomer, and expresses but little as regards the pathology of the disease; it declares simply that our patient is wasting away, repair on the part of the tissues having surrendered partially or completely to decay.

A condition of "marasmus," wasting or consumption occurs in all forms of exhausting diseases; but the name is only applied in cases of wasting accompanied with fever or symptoms pointing to any well-defined disease.

It is more frequently met with among the young and the aged, but whether infantile or senile, it is usually dependent upon similar causes and conditions. Among infants we meet cases which can clearly be referred to congenital syphilis, which at once takes them off the list of marasmus cases and places them under the specific classification. Others, again, have been so classified when they would probably have been more correctly diagnosticated as tuberculosis, tabes mesenterica, etc. Care in eliciting the family history and examining the cases will generally avoid these errors of diagnosis.

Many cases of so-called marasmus, if closely investigated, will present a history and general indications of intestinal catarrh.

Digitized by Google

Niemeyer, in writing upon the subject of chronic intestinal catarrh of children, refers to the fact that the imperfect diagnosis of "marasmus" is frequently assigned to such cases, and he is undoubtedly correct.

Eliminating all cases clearly belonging to other classifications, there remains those cases of wasting or general atrophy in which no fever or local lesion can be discovered. Pronounced pictures they are, too, after a prolonged period of progression; muscles shrunken and flabby, osseous prominences everywhere visible, with the pale, shriveled, dry skin hanging in broad folds and wrinkles about them, like a pair of loose and baggy trowsers upon calfless legs; face withered, wrinkled and worn, suggesting the miniature daguerreotype of some emaciated, toothless hag. The most pronounced features in the case being loss of flesh, loss of strength, loss of color, the complexion being of a dull leaden color.

Having excluded all cases of wasting dependent upon tangible conditions, such as tuberculosis, congenital syphilis, intestinal or gastric catarrh, etc., I shall devote my attention to the consideration of the cases which can properly be called marasmus.

They present all the symptoms above referred to, and in marked degree we have inactivity of the secretory glands.

In life there is dryness of everything—skin, alimentary canal and the emunctory organs in general, and after death, upon examination, we find further evidences of lack of fluidity or proper moisture of the tissues, confirming the thought that there has been a lack of secretion and excretion, exosmosis and endosmosis.

Primarily, then, I take the position that inactivity of the glandular system is at fault. In the very outstart of every infantile career we have more or less inactivity of the glands; the liver, with other glands, is larger (being more engorged) at birth relatively than at any later period in life. Attention to the proper establishment of the equilibrium of the circulatory, secretory and excretory system of the infant is of vital importance.

Given this torpid, glandular condition, coupled with improper or insufficient food, and other hygeinic errors, we have the factors favorable to the furnishing of a full-fledged case of typical marasmus. The five digestive juices upon which depends the proper preparation of pabulum for prompt appropriation on the part of the absorbents, are the products of parts of the secretory glands and the proper elimination of effete matter; the ashes of combustion, if you please, depends upon the zealous work of the excretory glands.

To illustrate my position, I herewith report, in a concentrated form, the notes of one of a series of cases under my care during the past year.

A. D., born August 1st, 1886, of healthy, wealthy parents, who have been under my observation constantly for over ten years (three other strong, hearty, robust children having been previously born), no hereditary taint whatsoever. At birth well formed, fairly well developed; the labor was, in common parlance, a dry one, but there were no complications, and nothing to indicate but what the child would be as healthy as his predecessors.

After a few days, bowels being slow in moving, olive oil was ordered, and nothing more was heard from the child until it was two months old. At this time aid was sought, for the reason that the child was constipated, uncomfortable and evidently not thriving. Inquiry developed the fact that from birth there had been habitual constipation, but little urination and continual restlessness and discomfort. The mouth and tongue were dry, the skin inactive, dirty, and yellow looking, the child smaller than at birth, with shrunken and flabby limbs, distended, overfilled and protruding abdomen, with the blue and close crowded veins standing out like whip cords over its surface.

There was evidently lack of proper secretion, excretion and assimilation; the baby was starving, though apparently furnished with sufficient and proper nourishment by

Digitized by Google

the mother. I at once ordered one grain of calomel and twenty grains of sugar of milk triturated thoroughly for a full half hour, and divided into twenty powders, one powder to be given every two hours, dry on the tongue, and followed at frequent intervals with liberal quantities of water. After twenty-four hours had passed, the bowels began to move freely, the aid of several warm water injections being given, and enormous quantities of hard, undigested, cheesy masses were passed, followed for several days by numerous large, loose, offensive dejections. More than likely, on account of this great accumulation, an acute intestinal catarrh would have soon been developed. During this time, when the activity of the glandular system was becoming aroused and the outlook better, the mother was taken very seriously ill with malarial fever, and it was soon apparent that a substitute was demanded. A strong, full-habited wet nurse (with a baby of the same age as our little starving patient, about three times as large and almost hoggishly fat) was secured, and to her credit, she refused to serve unless permitted to bring her child with her, promising to artificially feed him, and reserve her breasts for our patient. At this juncture the family removed some distance from the city and beyond my observation, until about six months had elapsed, when I was summoned and found my little patient in a condition every way aggravated. Investigation developed the fact that the motherly instinct of the wet nurse had prompted her to permit her own lusty boy to empty her breasts before giving them to the little starveling under her care. Not to go too much into detail, suffice it to say, that inability to secure a proper wet nurse soon necessitated artificial feeding. Various foods in the market were tried, without avail; a fermentative dyspepsia and gastro-intestinal catarrh presented, and the beginning of the end seemed near. All milk and malty foods were now relinquished by the stomach, and a raw meat liquid food, ten drops in a teaspoonful of water and two drops of brandy, were given every hour, and the child ordered to be given a bath every two hours, in either warm, fully-digested milk, warm cod-liver oil or warm water, with a teaspoonful of alcohol to the pint.

The internal medication was the infinitesimal dose of calomel triturate (previously referred to) every two hours, given for the purpose of stimulating secretion and excretion, antagonizing fermentation, antisepsis in the rendering inert of the ptomaines and other poisonous products of decomposition in the alimentary canal. This course was followed uninterruptedly, except by the gradual increase of the food, with gradual improvement for one week. Artificially digested milk was then cautiously added to the diet list and the amount of the liquid raw meat food doubled. From this time on, the progress toward perfect nutrition, growth and development was more and more rapid, and within one month he was becoming a well-nourished baby and possessed of a ravenous appetite, taking goodly quantities of water, and his excretory organs doing good service.

The one-twentieth grain of calomel was continued three times daily for two months, and after that resumed whenever indicated. The nutritious baths, with gentle massage and friction, were diminished in frequency, but not thoroughness, to three times daily, and later were given only morning and night.

From the observation and study of a series of twelve cases (the case which I have presented being typical of the twelve), where well defined causes of innutrition, such as syphilis, tuberculosis, etc., did not enter, I feel that I am justified in deducing the following :—

1. Infantile marasmus, so called, is dependent primarily upon torpidity and inactivity of the glandular system, and aggravated by unsuitable, over-abundant, insufficient food and unsanitary surroundings.

2. That which is of first importance in the treatment is the arousement of secretion and excretion, and the most valuable remedy we have for this purpose is minute doses of calomel, given in conjunction with as much water as can conveniently be adminis-

Digitized by Google

tered, the two agents, calomel and water, both being ardent accelerators of glandular action, stimulators of the secretion of the digestive juices, true aiders and abettors of digestion, and decided openers of the dammed up organs of diuresis, and awakeners of the dormant organs of defecation, cleansers of the vital sewerage system.

3. In the matter of diet, the mother's milk is best, and some other mother's milk next best.

Whether mother's milk or artificial food be given, the quantity and quality should be most carefully guarded.

In many instances the liquid raw meat foods in small quantities, well diluted and frequently given, will be of great service. All artificial food should be pre-digested.

4. In extreme cases the administration of soluble foods in the form of baths and by gentle friction will be of value, and in all cases gentle massage and frequent bathing (sometimes adding diffusible stimulants to the water) are of great service, much of the water being directly absorbed by the hungry and thirsty tissues.

TREATMENT OF SCROFULA, BY SEA BATHS, IN WINTER, AT CANNES, ALPES MARITIMES, FRANCE.

TRAITEMENT DU SCROFULE PAR LES BAINS DE MER, EN HIVER, À CANNES, ALPES MARITIMES, FRANCE.

BEHANDLUNG DER SCROPHULOSE DURCH SEEBÄDER IM WINTER, IN CANNES, SEEALPEN, FRANKREICH.

BY DOCTOR DE VALCOURT,

In Charge of the Civil Hospital and of the Maritime Hospital for Children at Cannes.

Sea baths have power in giving health and strength to children in general, and are of particular efficacy in overcoming and curing scrofulous manifestations in those predisposed thereto. This fact has been recognized by physicians, and during the last few years special establishments, hospitals for children thus afflicted, have been founded in European countries along the sea coast. The sea baths in the north offer many advantages, but present serious obstacles. The tide in many places prevents a fixed hour for baths. Often the surf is too strong for puny and delicate children, particularly on a pebbly beach; many shores have a sand so fine that the least wind blows it in clouds, and causes, or aggravates, those ophthalmic troubles to which scrofulous children are predisposed. Finally, the temperature of the sea, also that of the air, the fogs and the absence of sun, are so many obstacles which render sea bathing in the north of Europe uncomfortable, if not impossible, to these unfortunates. These unfavorable conditions do not exist on the shores of Riviera, and notably at Cannes. There, no tide or current, rarely any surf, a beach gently inclining, porphyric sand too heavy to be lifted by the wind and on which the children can walk so easily as to dispense, for the most part, with attendants in the bath. The temperature of the water is never so low as to hinder the short baths, *hydrotheric marine*, which constitute the tonic treatment of scrofula. Finally, the light and the heat of the solar rays, even in winter, produce a healthy reaction after the immersion.

Because of the presence of these hygienic conditions, Cannes has received every winter, from the different northern countries, children afflicted with coxalgia, Pott's disease, scoliosis or other bone diseases; children puny or delicate, young girls with

Digitized by Google

whom menstruation is delayed or difficult, and even rheumatic sufferers. Then, when these acute manifestations may have disappeared, and their feeble constitutions are to be strengthened, we prescribe for them, not only the sea baths, but also exercise on the beach and in the boats on the water. Those who are not able to take the baths by immersion, take warm sea baths or bathe in cold sea water at their houses.

As to the sea bath given warm, we desire, but as yet without entire success, that the baths should be heated instantaneously by a current of steam, as is done at the mineral baths at Schlangenbad, so as to avoid the partial decomposition of the salt water by the prolonged heating.

Until 1882, the northern children frequenting the beach at Cannes belonged principally to families in easy financial circumstances, these only being able to pay the expense of dwelling on the sea coast. At this time Monsieur Jean Dollfus generously founded a marine hospital for scrofulous children, to the end that those of the poorer classes might participate in the benefits.

Children were sent us from Mulhausen, Paris or Geneva. Those who were subjects of pulmonary tuberculosis were not admitted, for the sea baths are not satisfactory to them, and we would not risk the connection of children simply scrofulous with those affected with phthisis. The children arrive about the first of October and remain until the first of June.

The baths are administered to them on their arrival and are continued as long as possible. In 1886–7, for example, the autumnal season lasted until December 23d. The children like the baths so that they would voluntarily continue them all the winter. The temperature of the sea was 65° in October and gradually descended to 52°, the minimum at Cannes.

It is, therefore, not because of the low temperature of the sea water, but rather that of the atmosphere, that we ceased the bathing in the open sea during the winter. Yet we know many persons who continue bathing during the entire winter, and it is quite possible that we will arrive at this point with some of our children.

We have never yet found a case of bronchitis, or like affection, which could be attributed to cold baths; on the contrary, the amelioration of the health of those who were specially sick was manifest and prompt. The baths were renewed on the 17th of February, when the temperature of the sea was 58°, and were rarely interrupted by atmospheric perturbations until the close of the season at the end of May. Some of our children had running abscesses, but that did not hinder them following the bath treatment. The suppuration increased slightly under the influence of the first baths, the edges of the sores reddened slightly, but that once passed and a rapid improvement was the rule. In some exceptional cases we covered the sores with a protective during the bath, and on coming out bathed the parts with soft water. Some of our sick were attacked by Pott's disease or scoliose. We put on these Sayre's corsets, which were taken off during the bath.

We think that the sea baths must be rejected during the period of painful muscular contraction of coxalgia. It is necessary, then, that we content ourselves with the treatment of immobility, of continued extension and rest at the sea shore. But the acute symptoms of contraction once passed, we return to the use of the baths, cease the immobility, whether there be suppuration or not, this symptom having only a secondary signification and not retarding the final cure. I say even more: in those cases where there is suppuration, the treatment is easier, more certain; this prognostication is favorable at least among the children sent to us. The hospital Dollfus commenced in a small villa, much restricted for the number of children under treatment. To avoid the evil effects of bad air and odors, the matron had the idea to keep the windows open during the entire day. We were at first opposed to this, but experience proved that the children improved so much under this exposition to the fresh air, even during the most

Digitized by Google

rigorous days of our winter, that now, instead of combating, we are in favor of the system, and it forms part of our treatment. We have not had a single case of bronchitis among our children. The only case of acute sickness which we had to treat was our cook, who contracted a pleurisy from imprudently sleeping in a damp room.

We would not prescribe open windows to a new comer unused thereto; to do this would incur a needless responsibility; but if the children arrive at Cannes in October or November, while the season is yet warm, they will soon be prepared by their sea baths against any atmospheric changes, and this will be an advantage in securing a general good result to their health.

One can easily compare the benefits of this hygienic régime with that of the poor children penned up in the vicious air of a hospital and in the immediate neighborhood of, if not in contact with, other children affected with contagious diseases.

The results obtained are extremely remarkable. I could give a long list of observations, but it will suffice to refer to the many of our foreign confrères who have visited the hospital Dollfus and seen for themselves.

The establishment has just been transferred and enlarged. M. Dollfus has purchased a hotel on the Square Brougham, admirably situated on the beach. It is arranged to receive forty-five children. Fifteen beds have been reserved for children coming from Geneva, through an arrangement made by the charitable committee of that city.

We can but wish that these establishments should be multiplied, for they will never be enough to receive and care for all the children thus unfortunately and sadly afflicted, but who, we will hope, can thus be saved to life and restored to health.

Digitized by Google

ORTHOPÆDIC SURGERY.

THE TREATMENT OF CONGENITAL CLUB FOOT.

TRAITEMENT DU PIED BOT CONGENITAL.

DIE BEHANDLUNG DES ANGEBORENEN KLUMPFUSSES.

BY MR. WILLIAM E. BALKWILL,

London, Eng.

In the few remarks I have the pleasure of addressing to you there is a twofold object. As you know, my subject is that of the cure of club foot. The text is an old one, but still worth consideration: to this end I wish to sketch our method in England, and so free the way for any discussion that may to our President and yourselves seem valuable. It is said that to make an old tree again bear fruit, you must dig about and dress the ground; that, I feel, is our position with regard to Orthopædic Surgery, a subject which is but too often relegated to the waste-paper basket. In spite of slight differences, our mutual ideas are the same, simply and only the surest and most perfect way to alleviate deformity of the feet. To this end I ask you to kindly follow me closely, as the subject involves more than superficial thought. In stating the practice I have been in the habit of using—and, I am proud to say, never without success—I ask your opinion, so that I may take across the water a message from our American, Canadian and other brethren. This may well come from the home of specialties.

To describe club foot is not necessary, so I confine myself to treatment alone. We will take, if you please, a case of simple primary congenital talipes equino-varus. The child is brought to you at some two or three weeks old, and you have to decide when to operate, for operate you must; no extension apparatus is of permanent good. Well, the time to be fixed is to be judged by the health, strength and size of the child. I have operated at fourteen days and postponed until three or four months older.

My invariable practice has been—first, to be sure of a really good assistant; secondly, to know that my tenotomes are perfect; and, thirdly, to be careful that a flexible splint or two, some lint, bandages, and strapping are at hand. Then, with the little patient perfectly prostrate on the side, I take a sharp-pointed knife with a large belly, and, feeling for the internal malleolus, insert the tenotome in the mesial line until I feel the bone; slightly withdrawing it, I slope the knife until it enters the tendon sheath. Then altogether withdrawing the tenotome, its place is taken by a blunt-pointed instrument, which is inserted with its cutting edge toward the malleolus. The assistant, meanwhile, has been careful not to move the foot at all. With—and I wish you to note this, gentlemen—careful ease you will find the knife slip into the opened sheath, and then your assistant stretches, not too quickly. A snap of the posterior tibial tendon; take your knife away partly, get the assistant to again turn the foot back, again stretch it, and the flexor longus digitorum follows. At once place a pledget of lint and strapping. Get your assistant to hold it, feel carefully for the tibialis anticus, which is, in varus, as a rule, just to the inner side of a pucker of skin; divide, and place the foot in

Digitized by Google

the same position as prior to the operation. By this you avoid any possible trouble, and the quiet stretching afterward gives, in my opinion, a stronger foot, with less chance of relapse. After the anterior tibial tendon has been divided, feel for the plantar fascia. In a primary congenital case it is very seldom division is necessary. If there be tension, divide at once, but leave the Achilles tendon until, by means of the flexible splint and bandaging two or three times weekly for three weeks, you are able to proceed further to the second stage. This means division of tendo-Achillis. My best results have arisen when, instead of extending fully at once and keeping the foot in that position, I have taken time over it, and, in fact, used the foot as Isaac Walton did his frog—no force, no undue pressure, but a careful guidance. With this, gentlemen—though the treatment may occupy longer time, and not so much either—with this and good nursing you may always prognosticate a good end.

Subsequent treatment depends a great deal on the necessities of the case. Some require instruments such as I have to show you. Others require well-made boots, with stiffening to go even as high as the calf. Given the chance of treatment at an early age, little is requisite. In London we have the character of always using so-called irons. Our cases are gathered from all over the world, have been mostly neglected by their parents after the doctor has said good-bye, and in these relapsing ones all of us must apply and devise various supports of divers kinds. Do not be terrified when a conscience-stricken mother says: "I will not let my child wear irons." I cannot counsel irons for all; that must be a clinical point.

These various supports that I have here for valgus, varus, etc., I shall be happy to show any of you, or, with our President's permission, hand them round. They are but models. As I said before, my object in speaking to you was more to bring forward a greater esteem for orthopædism, grasping, as it does, the extremes of all surgery.

I thank the members of this Section most heartily for their patient hearing.

SECTION OF CONTRACTURED TISSUES ESSENTIAL BEFORE MECHANICAL TREATMENT CAN BE EFFECTUAL.

SECTION ESSENTIELLE DES TISSUS CONTRACTURÉS AVANT D'EFFECTUER LE TRAITEMENT MECANIQUE.

DURCHSCHNEIDUNG DER VERKÜRZTEN GEWEBE WESENTLICH, EHE MECHANISCHE BEHANDLUNG WIRKSAM SEIN KANN.

BY LEWIS A. SAYRE, M.D.,

Of New York.

Distortions or deformities which are the result of *contractured* tissue can only be removed by forcible rupture of the same, or by cutting before traction is applied; but similar deformities resulting from simply contracted tissue may frequently be rectified by manipulation and constant traction properly applied, without section of the tissue.

A *contracted* tissue is one that is simply shortened, but which can be elongated by careful, continuous and judiciously applied traction and manipulation, and, therefore, does not require to be divided.

A *contractured* tissue is one which has undergone some change of structure in the fibrillæ of the muscles, and which cannot be elongated or stretched unless the tissues are severed or torn, and, therefore, section in such cases is absolutely necessary. Mr.

Digitized by Google

Little, of London, describes this condition as one of "structural shortening;" but I prefer to call it *contracture*, to designate it from the simply *contracted* muscle. Upon the recognition of which class of shortened tissue we are dealing with, will depend the selection of the means most proper for the treatment and consequent removal of the resulting deformity. And as the two classes demand somewhat different treatment, it is well to have some positive rule to aid us in our diagnosis.

How are we to know whether the tissue is *contracted* or *contractured?* The following simple rule, which I have very carefully observed in many hundreds of cases for the past twenty years, without a failure in a single instance of its proving to be correct, leads me to feel almost justified in laying it down as a law of universal application in all cases of *contractured* tissues.

If, in any case of club-foot or other deformity from muscular contraction, we stretch the shortened parts to their utmost tension by manual force or mechanical aids, and when the parts are thus stretched, we suddenly add to the tension by pressing with the thumb or finger on the part thus stretched, or by pinching the stretched tissue between the thumb and finger, and if, by either of these acts, we produce a *reflex* spasm or sudden shivering of the whole body, that muscle tendon or tissue thus yielding this *reflex spasm* is *contractured* and cannot be elongated *without severing* of its fibres.

If, on the contrary, when the test is applied, as above described, and no *reflex* irritation or muscular spasm is produced, it is evident that the parts are simply *contracted* and can be further elongated by persistent constant traction and proper manipulation, and, therefore, do not require division.

This is a very important rule to observe in practice, as it will save the surgeon a great deal of valuable time, the patient months of useless and unnecessary torture, and will always yield the most satisfactory results.

To attempt to stretch a *contractured* tissue is to subject the patient to a great amount of unnecessary pain, and at the same time run the chance of producing serious disturbance of the nervous system, as the involuntary contraction or "reflex spasm" of the muscular system is produced in a less degree, may be, every time the *contractured* tissue is stretched, and we are all aware of the disastrous results which sometimes follow long-continued irritation of the nervous system, both from "reflex" and other causes.

If a ship is fastened to the dock by a cable stronger than the engine can possibly break, she cannot leave her moorings until some one has severed the hawser or else loosened it from its attachment.

The same with any deformity, the result of *contracture* of any tissue. The deformity cannot be removed until the *contractured* tissue is divided.

In all such cases it is infinitely safer to make such division by *subcutaneous section* than by manual or mechanical force, as a force sufficient to tear these dense tissues could not readily be released before damage to other and more yielding tissues might be done.

By dividing subcutaneously all the shortened or contractured tissues, and hermetically closing the wound made by the tenotome, and *immediately* placing the parts in their normal positions, and retaining them there, the space between the severed ends of the tendon, muscle or fascia becomes filled with bloody serum or lymph, which being protected from atmospheric influences, becomes organized, and makes the tissue thus divided as much longer than it was before, as the distance between the severed ends.

Should the skin be *contractured*, as is frequently the case, it must also be divided freely and the wound stretched until the parts are brought into their normal position, allowing the gap to become filled with a clot of blood, *which is to be left in situ*, and being dressed antiseptically, it soon becomes organized and fills the space, without the formation of any pus, as has been frequently proved by Dr. Schede, of Hamburg.

Of course the parts must be kept in their normal position, and at absolute rest until

Digitized by Google

the new tissue has become thoroughly organized, which generally takes about twelve to fourteen days.

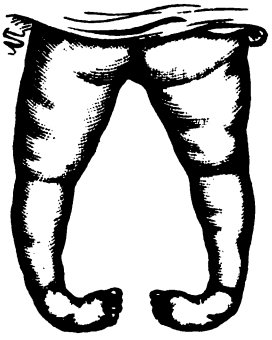
After this has taken place, massage, frictions, active and passive motions, with the use of electricity to develop the weakened muscles, together with such mechanical appliances as may be required in each particular case, will be demanded and must be thoroughly practiced in some cases for many months before the cure will be complete.

The following cases are very good illustrations of the principles here expressed:—

CASE I.—Congenital double varo-equinus. Eversion shoes with strong traction applied when child was eight weeks old, and continued for 14 months. No improvement. Subcutaneous section of contractured tissues and immediate replacement, followed by rapid recovery.

Helen A. G., aged sixteen months, daughter of George H. G., of Bergen Point, N. J., was brought to me on January 14th, 1887, with congenital double varo-equinus, as seen in Fig. 1, from drawing by R. H. Sayre. The mother states that the feet were in the same position they are at present when the child was born. When two months old was taken to a prominent orthopædist of the city, who had made for her the extension shoes she now has on, and who has had charge of the case for the past 14 months. Finding no improvement, they brought the child to me on January 14th, 1887, but I

Fig. 1.



refused to take charge of it without consultation with the former attendant. On the following day I received a very kind note from the Doctor, stating his inability to be present, but requesting me to take charge of the case.

The condition at the time was as seen in Fig. 1. Both heel cords were very strongly *contractured*, also the tissues in the sole of the feet, especially on the left side; both tibiæ curved out and knees bent in. Child walked with feet at right angles to front axis of body. Reflex spasm very marked on point pressure of both tendo-Achillis and plantar fascia of each foot, thus showing *contracture* of these tissues, which had not yielded in any perceptible degree to fourteen months of faithful stretching.

January 17th. Tendo-Achillis and plantar fascia of both feet subcutaneously divided by Dr. R. H. Sayre. Feet put in right-angle position and retained by footboard and adhesive plaster.

February 1st. Removed dressings for first time; wounds healed; outside of foot still lower than inside border. The feet were easily forced by the hands into their normal position, and retained there by plaster-of-Paris bandage. The feet being held in nearly natural position until the plaster "set."

February 10th. Removed plaster-of-Paris boots; feet improving. Massaged feet

Digitized by Google

and legs and reapplied the plaster boots, inserting a hook in the plaster bandage on the outside of each leg, about the middle of the calf. When the plaster was "set" a strong India-rubber band, such as are slipped over letters, was attached to each of the hooks, and passing behind the legs, tended by its contraction to evert the feet. These bands can easily be increased in number, or made of thicker and stronger rubber, as the case requires. When the plaster was "set" and the India-rubber attached, she could walk quite well with the toes everted.

March 1st, 1887. New plaster-of-Paris boots applied. Position of feet improving.

May 1st, 1887. Various plaster boots have been applied from time to time, as it is necessary to change them frequently in a rapidly growing child.

Is now quite well, as seen in Fig. 2.

Fig. 2.

Fig 3.

The feet can now be retained in a very good position when the boots are removed. The boots, which had been made on the feet, over a very nicely fitting stocking, were cut through stocking and plaster in front, from top to bottom, and opened wide enough to slip off the feet. The feet and legs were then well rubbed—massaged, electricity applied for five minutes, and then the plaster boots were reapplied and secured by a roller bandage, and the India-rubber bands attached as before.

The mother was instructed to remove the boots daily, and after giving similar treatment to reapply them. July 1st, 1887, very greatly improved, as seen in Fig. 2, from photo; can walk well in bare feet, with both feet flat on ground. Ordered ordinary laced boots, with high counters to sustain the ankles, in which she walks very well.

CASE II.—Equino-varus, from post-diphtheritic paralysis, tibials of left foot, followed by *contracture* of tendo-Achillis and plantar fascia.

Strong traction applied daily for fourteen months without improvement. Section of contractured tissues and *immediate* replacement to normal form resulted in perfect restoration of natural position and limited voluntary motion inside of four weeks.

Julius G., aged eight years, son of Sigmund G., of E. 116th street, was brought to me May 28th, 1887, with equino-varus of left foot, as seen in Fig. 3, photo, from Dr. R. H.

Digitized by Google

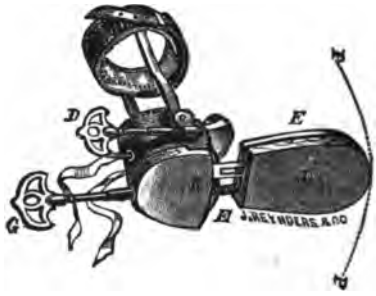
Sayre. He had had scarlet fever and diphtheria when three years old, which left him with more or less deafness of both ears and paralysis of posterior and anterior tibial muscles of left foot.

His father says that he brought the boy to me in the fall of 1885, and that I advised an operation at the time; but that I was sick at the time, and unable to perform the operation personally; he sought other advice.

The surgeon consulted did not think an operation necessary, but advised massage, electricity and the application of "Shaffer's Extension Club-foot Shoe" (see Fig. 4) for a few minutes at a time, twice a day.

This treatment was carried out very faithfully for more than a year, without, as the father states, any improvement. His present condition is seen in Fig. 3. Severe pain accompanied with strong reflex spasm was produced by point pressure on the plantar fascia when stretched, and the same result followed point pressure on pinching of the tendo-Achillis when stretched, thus proving both to be in a state of *contracture*, and requiring section before they could be relieved. As the most faithful efforts to stretch the parts with Shaffer's extension shoe had been made for more than a year without any apparent improvement, the father readily assented to the operation. I at once administered the chloroform and my son, Dr. L. H. Sayre, divided subcutaneously the plantar-fascia and tendo-Achillis of the left foot. Immediately closing the wound, and restor-

FIG. 4.



ing the foot to its normal position, he retained it there by my usual form of dressing, with footboard, adhesive plaster and bandage. A few hours after the operation he had to go to Harlem (116th street) to remove the bandage, on account of the pain under the great toe. On removing the bandage a small blister was discovered under the great toe, from the pressure of a splinter in the wadding. This was removed and the foot redressed as at first.*

This dressing remained undisturbed for 14 days, without the slightest inconvenience to the patient, and without any change in the position of the foot or bandage.

On removing the bandages on the fourteenth day the wounds were found entirely healed, without any pus, and the effused material between the severed ends of the tendon was so firmly organized that the patient could voluntarily extend the foot against a quite firm resisting pressure.

* After an operation for club-foot and dressing it in the manner I have described, if the patient complains of any pain after a few hours, do not give him an anodyne to allay it, but immediately remove all the dressing and ascertain the cause of the pain; by removing it before you replace the bandages, you will save your patient from all pain, and avoid any danger of sloughing from undue pressure at any point, or of abscess from injury by foreign body, as in this instance.

This is a very important rule which I wish to enforce strongly.

Digitized by Google

A nicely fitting stocking was put on, and the foot being held at a right angle with the leg, and perfectly straight, was encased in a plaster-of-Paris bandage from the toes to near the knee. The leg being slightly shorter than the other, an oval pad of the plaster-of-Paris was placed under the foot to elongate the limb, and when the plaster was set the patient was able to walk remarkably well, the oval sole under the foot almost compensating for the stiffened ankle joint. He walked upon this plaster support for one month, when he was fitted with a nice pair of laced boots with a very high counter and elongated heel and sole, in which he walks very well, and no deformity, as shown in Fig. 5 (from photo.).

CASE III.—Talipes equino-valgus paralytica; subsequent contracture of tendo-Achillis, and plantar fascia.

Treated daily for two years and a half by a strong screw extension apparatus without any improvement, but producing great disturbance of the nervous system. Subcutaneous section of the *contractured* tissues and immediate replacement of parts to

Fig 5.

normal position resulted in perfect recovery of form and partial voluntary motion, in two months. The only deformity remaining being the shortness of the limb.

G., aged six years, 743 Broadway; always healthy until three years of age, when she was confined to bed for a week with severe indigestion and a very high fever (probably spinal meningitis). When she attempted to get up it was discovered that she had lost the power of moving either lower extremity. She recovered partial power of the left leg in about two months, and could flex the right thigh slightly, but could not extend the leg upon the thigh. The right heel began to draw up and the foot to evert; was treated for six months by electricity and massage, but made no improvement. A brace was applied, which she wore for two months, but it caused so much pain that it was abandoned. She was then placed under the charge of a specialist, an orthopædic surgeon, who applied his peculiar club-foot shoe, with a powerful screw worked by ratchet and key, for the purpose of stretching the tendo-Achillis and plantar fascia.

Digitized by Google

The child was taken to the surgeon's office daily during the winter, and every other day during the summer (when they were at their country seat, out of town) for two years and a half, for the purpose of having his personal adjustment of the apparatus. Another special shoe was worn during the night. As there was no improvement in her condition, her uncle, Dr. Purron sent her to me on the 27th of June, 1887. I sent her back with a request to bring her former attending surgeon, if they wished me to examine the child. Dr. Purron came with the child himself, on the 29th, and requested me to take charge of the child, as her former surgeon was absent from the city.

On removing the apparatus I found the limb two and three-quarter inches shorter than the other, with extreme equino-valgus, with contracture of tendo-Achillis and plantar fascia, both giving reflex spasm on point pressure when stretched.

As the most faithful efforts had been made to stretch the parts for two years and a half, without, as the Doctor stated, the slightest improvement, he readily assented to my operating.

Assisted by Dr. Purron and my son, Dr. L. H. Sayre, I subcutaneously divided the tendo-Achillis and plantar fascia, and dressed in my usual way, on June 29th, 1887.

The tendon was unusually hard and very difficult to cut. I have noticed this fact in many cases where they have been subjected for a long time to extreme traction.

Fig 6.

Does this severe traction produce inflammation and condensation of the tissues?

June 30th. Perfectly comfortable; slept well all night; foot in good position; went to the country.

July 30th. To-day dressed the foot for the first time since operation. Bandages and footboard exactly as when applied fourteen days ago. Aunt says the child is very much less nervous, and sleeps well.

Wounds perfectly healed. Tendo-Achillis firmly united, and can extend the foot voluntarily.

Put on stocking, placed foot at right angles and applied plaster-of-Paris bandages, with extra roll of plaster under foot, to equalize the length of legs, as shown in Fig. 6.

July 13th. Walks well on plaster boot, the rounded and elongated sole almost compensating for the stiffened ankle joint.

Aug. 12th, 1887. Has been running with the other children for the past month, and is in perfect health, as seen in Fig. 7.

Removed the plaster boot, and had her measured for a pair of laced boots. The one on the left foot to be elongated and rounded, similar to the plaster-of-Paris-boot.

Digitized by Google

A photograph of her in her shoes was taken at this time by Dr. R. H. Sayre (see Fig. 8).

The two following cases have already been published in the *New England Medical Monthly* of November 1886, but as they very strongly illustrate the principles I have endeavored to inculcate in this paper I have concluded to add them to my report.

CASE I.—Some three years since, Dr. T. Gaillard Thomas brought a gentleman to my office with his little son, about six years old, who was suffering from congenital equino-varus of both feet in a very marked degree.

He had been subjected to various treatment since early infancy, and for the past twelve months had been visited daily at his house by a prominent orthopædic surgeon of this city, who had applied for a few minutes to each foot daily, during all this time,

Fig 7.

Fig 8.

a shoe of his own construction, which was capable of making *very powerful* traction upon the contractured parts; but as the father and Dr. Thomas both stated, without having produced the slightest change in the deformity.

But the effect of the torture had made such an impression on the child's nervous system that he was almost ungovernable and would submit to the treatment no longer. Upon stretching his foot with one hand, and pressing with the fore-finger of the other on the tense plantar fascia, an instantaneous spasm of almost all the muscles of the body was produced, and the child screamed in agony. The same result was produced when the same test was applied to the plantar fascia of the other foot, and also when applied to the tendo-Achillis of either side.

Digitized by Google

Dr. Thomas was convinced, after this examination, of the folly of attempting to stretch such tissues, and advised the father to place the child under my care.

The same afternoon I went to his house in the upper part of the city, and assisted by my son and Dr. Develin, who administered the chloroform, and in the presence of Dr. Walker, Dr. Thomas' assistant, I divided subcutaneously the plantar fascia, tibialis anticus, and tendo-Achillis of either side, and *immediately* restored the feet to their normal position, and dressed them in my usual way while he was still under the anæsthetic; but this plan of dressing is so fully described in my "Manual of Club-Foot," and in my lectures on "Orthopædic Surgery," that there is no occasion to repeat it here. I visited him on the following day, and to my surprise he put out his hand and greeted me with a cordial welcome, instead of screaming, as the mother said he had always done before when any doctor came to see him. The father said that he behaved so differently from what he had ever done before, and seemed so happy, that he acted like a "reconstructed" boy.

The dressings were removed for the first time on the fourteenth day, when all the wounds were perfectly healed, without a drop of pus, and the Achillis tendon of either side was so firmly united that he could voluntarily extend either foot even when quite firm resistance was made against the bottom of the same. He was measured for a new pair of shoes, as his feet were so much longer that he could not wear his old ones, and the footboard and adhesive plaster re-applied for another week, as a means of protection until the organization should become more firm. Three weeks from the time of the operation he walked very well, with both heels upon the floor, in an ordinary shoe, simply requiring Hudson's elastic attachment to aid in flexing the feet.

These he wore for nearly a year, when, by the daily use of electricity, massage and manipulation, his cold and atrophied legs had become so well developed that he could run and play with the other children, without any artificial support whatever, and has remained well until the present time, with constant improvement in the development of his lower extremities.

CASE II.—John S., of Brooklyn, aged nine, was brought to me on the 15th of July, 1886, with a very marked equino-varus of the right foot, with severe contraction of the plantar fascia, standing on the ball of the great toe, as seen in Figure 9.

The mother says that the child was perfectly natural at birth, but that when eleven weeks old it rolled over on its face in the bed and was nearly suffocated. Two days later it had convulsions, which were repeated at intervals for two days, during which time the bowels were obstinately constipated. After forty-eight hours a passage from the bowels was effected and the convulsions ceased. Some weeks afterward it was noticed that the child had lost full control of the muscles of the eyes, being unable to move the eyes in all directions as it formerly had done. This gradually disappeared, and when he began to sit up the mother noticed that the feet were strongly drawn backward, and the knees widely separated. When he began to stand he did so on his toes, the heels of both feet being strongly drawn upward. Wore braces of different kinds, without benefit, and when he was twenty-four months old was taken to a public institution in this city, where the surgeon divided the tendo-Achillis of either side, and succeeded in obtaining an excellent result on the left side, but the right one was very unsatisfactory.

He remained under treatment for two years after the operation, without any improvement in the right foot, and in February, 1885, he went to another institution in the city, where the surgeon applied an "extension shoe," similar to the one described in the previous case.

This shoe was personally applied by the surgeon once every day from February until the 1st of May, and from May until July it was applied twice a day by the parents, but, as they state, without producing the slightest change in the position of the foot, although

Digitized by Google

the traction power of the instrument was so great as to cause the child to suffer intense pain, which frequently lasted for some time after the "extension shoe" had been removed.

The parents therefore decided to try some different treatment, and brought him to me on the 15th of July, 1886. On extending the foot and making pressure with the finger on the plantar fascia an instantaneous *shivering* of all the muscles in the body was produced. The same result followed point pressure on the tendo-Achillis when stretched, only in a much more marked degree. This shivering or tremor of the whole body is what I mean by the *reflex spasm* produced by point pressure on stretched tissues, and

FIG. 9.

proves that those tissues are *contractured* and must necessarily be divided before any elongation can be produced.

As the case was one in which traction had been thoroughly tested by a perfectly competent surgeon, and without any improvement, according to the parents' account, I decided to cut him. I sent a request for the surgeon who had attended him to be present at the operation, and sent the boy to the photographer's, and Fig. 9, from the Moss Engraving Company, gives an exact representation of his deformity. At 3 P.M. of the same day, July 15th, assisted by my son, Dr. L. H. Sayre, Dr. J. Woodbury and Dr. E. Develin, who administered the chloroform, I divided subcutaneously the plantar fascia and the tendo-Achillis, and *immediately* brought the foot into its normal

Digitized by Google

position by considerable force, and dressed it in my usual way, as above described. Only a few drops of blood were lost in the operation, but the space between the severed ends of the divided tendo-Achillis was much greater than in any of my previous operations. Unfortunately the gentleman whom I had invited to see the operation was out of the city, and therefore could not be present.

No local or constitutional irritation of any kind followed the operation. The child slept soundly every night without opiates, and ate his meals with perfect regularity.

The bandages were never changed in the slightest degree until the 31st of July, just sixteen days after the operation, when they were removed for the first time, in the presence of my son, Dr. Develin and Dr. John F. Ridlon. The wounds were all healed, without the formation of a drop of pus; could flex and extend the foot, showing that the tendo-Achillis had united, and could stand erect with heel upon the floor. Left for his home in Brooklyn that afternoon.

Fig. 10.

August 6th. Boy at office; foot in perfect condition, but from too much exercise and the pressure of a tight-laced shoe there was a slight abrasion of the skin near the wound in the tendo-Achillis.

The mother stated that he was so happy in being able to walk that he got a number of boys in the back yard with some goats, and as some of the boys had seen the lassoing by "Buffalo Bill" in the "Wild West," then performing on Staten Island, they

Digitized by Google

attempted the same performance on the goats, and in this way he had hurt his heel, and finding a little moisture on his stocking she had brought him to see me. Fearing that too much motion might extend the irritation to the deeper and just newly-organized tissues, I put on his stocking and applied over it a plaster-of-Paris bandage, thus immobilizing the ankle joint. When the plaster was hard I cut a fenestra through the plaster and stocking on the abraded part and dusted it with iodoform.

He was able to walk without any trouble, but with no motion at the ankle.

He wore the plaster cast until the 16th of August, when I removed it and found the abrasion entirely healed, the foot in good position, and he was capable of flexing and extending it quite freely, and walked with his heel firmly on the floor.

Figure 10 was taken on that day, August 16th, just one month and a day after the operation, and illustrates practically the result of the treatment.

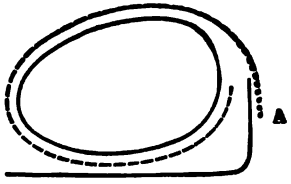
August 28th, walked across the Brooklyn bridge without assistance, a distance of half a mile or more, and can run up or down stairs without limping.

Foot in natural position; requires merely time, massage, electricity and exercise, to develop his atrophied muscles, to make the cure complete.

DISCUSSION.

Dr. A. B. Judson, New York.—The treatment of congenital club-foot should be divided into two stages by the arrival of the time when the child begins to try to stand. Before that time, in the period during which the child's weight does not interfere with corrective procedures, the varus should be reduced by whatever means may be at hand—a succession of carved-wood splints, tractable metal splints, adhesive plaster, plaster-of-Paris splints, the artificial muscles, or a combination of these methods, one with another, or with tenotomy, as the case may require. Whatever the plan adopted, by the time the child begins to put his weight on the foot, the varus should be so far reduced that the sole is presented squarely to the ground. Of course, if nothing further be done, the varus would return when walking begins, and would soon be as bad as ever, and what is now required in this, the second stage of

Fig. 11.



treatment, is a slight splint extending up the inner side of the foot and leg, and so furnished with buckles and webbing that pressure may be applied from within outward along the inner border of the foot and at the inner and upper part of the leg, and from without inward in the neighborhood of the ankle. This club-foot shoe or splint is more efficient if made without a movable joint at the ankle. It should be made of tractable steel, so that it may be bent from time to time, and thus take the lead as the foot improves in shape, and requires patient attention on the part of the surgeon to secure at all times an efficient and comfortable fit. Its efficiency may be greatly increased by the untwisting force of a strip of adhesive plaster and webbing wound around the foot and buckled to the upright portion of the foot piece, as shown in Fig. 11, which represents schematically a transverse vertical section of the foot and foot piece, the broken line representing the plaster, the dotted line the

Digitized by Google

webbing, and A the place for the buckle. The object of the splint is to give the foot a push toward the position of valgus, so that when the child steps the sole will present squarely to the ground or a little more than squarely. Every footfall will then be an impulse in the right direction, applied with the force of the rapidly increasing weight of the child.

Mr. EDMUND OWEN, London.—Mr. Balkwill has come forward as the representative of the old-fashioned treatment of club-foot, and as such he narrows the issue to the difference between that and the modern or American method. This being so, I am glad to say that I am, and have been for some years, as heartily devoted to the American plan. The old plan consisted in dividing first the tendon of the tibialis posticus, and, perhaps, also that of the flexor longus digitorum, and then in putting the foot up in only a slightly improved position; then, when it was supposed that plastic effusion had connected the divided ends of the tendon, to begin with Scarpa's shoe to correct the inversion. Later on, perhaps weeks afterward, when the inversion had been fairly dealt with, the tendon of Achilles was divided, and in a few days flexion of the foot was started with. This plan is now, even in England, giving place to the method of dividing all the tendons at one and the same time; and not only that, but bands of fascia and ligaments were also divided. I wish to call particular attention to the need of dividing the anterior part of the internal lateral ligament of the ankle joint, which was much concerned in keeping the front part of the tarsus flexed and inverted on the head of the astragalus. Scarpa's shoe, together with mechanical spinal supports, should be relegated to the limbo of extinct surgical instruments; the introduction of the plaster-of-Paris method of treatment had rendered them superfluous. There need be no fear of failure of union occurring in the divided tendon after putting the foot at once in the correct position; plastic effusion and blood in the sheath of the tendon quickly prepared the way for an efficient splicing of the tendon.

I cannot accept entirely all that Dr. Sayre has said as to the difference between contractured and contracted tendons. I do not deny its existence, in fact, but am not prepared to accept it, but commend in the highest terms Dr. Sayre's teaching and practice.

Dr. LEWIS A. SAYRE, New York.—The treatment of congenital talipes should be begun immediately after birth. If prompt treatment were the rule, section of tendons would rarely be called for.

Dr. E. R. LEWIS, Kansas City, Missouri.—The distinction made by Dr. Sayre between contracted and contractured tendons, if it proves to be a reliable guide, is an important diagnostic sign in considering the question of operative interference. If I mistake not, the late Prof. S. D. Gross taught, years ago that the administration of chloroform would lead to a correct decision of the question. I have used this method in torticollis and talipes with good results, but I shall resort to the method of Dr. Sayre, and feel amply repaid for my trip to Washington if it proves as certain as I think it will.

Dr. W. N. HINGSTON, Montreal.—I would like to ask Mr. Balkwill if, in stating he had operated on no child under fourteen months of age, he intended it to be understood that he discountenanced operations on children at an earlier period. My views on the question of the time of operating have undergone great modifications. Formerly, it was usual to delay operating till after the period of teething, at least, but the terse words of Dr. Sayre, written some years ago, that one is warranted in delaying an operation only till the tenotome can be procured, is an advice

Digitized by Google

which I have followed from the time they were uttered, with almost invariable satisfaction.

A very marked distinction is made by Dr. Sayre between contracted and contractured tendinous tissue. Does he apply the latter term to a tendon which he has already divided, and which, having united in a faulty manner, the question of re-division or stretching presents itself? How soon after division was the foot put into restraining apparatus?

Mr. BALKWILL.—I am in favor of early operations for club-foot. I have never operated earlier than fourteen days, and do not think anything is to be gained by operating earlier than that. I have also postponed the operation for three or four months, for what appeared to be sufficient reasons, and have secured as good results as could have been obtained by an earlier interference.

Dr. LEWIS A. SAYRE.—I place the foot, forcibly stretched to its position, in plaster immediately after section. If I see the case as an original one, I never have to re-divide; but frequently cases come to me from other practitioners who have failed, and then I do not hesitate to re-divide the tendon.

Dr. D. A. K. STEELE, Chicago.—I came to the Section a strong partisan of American surgery, but if Mr. Owen's views and methods prevail in England, I am as firm an adherent of English surgery. The results of our experience in Chicago have suggested the following rules: (1) Operate as early as the child is brought, provided the tendon is shortened. (2) Divide all tissues that prevent perfect restoration of the foot. (3) Retain the foot in a correct position by plaster-of-Paris dressing applied over cotton batting and carried above the flexed knee, to prevent rotation. (4) Leave the toes uncovered, in order to watch the circulation. (5) Let the entire procedure be clean, aseptic and antiseptic.

PRINCIPLES OF PROGRESS IN ORTHOPÆDIC SURGERY.

PRINCIPES DE PROGRÉS DANS LA CHIRURGIE ORTHOPÉDIQUE.

GRUNDSÄTZE DES FORTSCHRITTES IN DER ORTHOPÄDISCHEN CHIRURGIE.

BY MR. NOBLE SMITH,
London.

Progress in any science can take place only when correct principles are followed out, and I would urge that orthopædic surgery should be studied and practiced upon some such basis as that which I here suggest. I advance the following propositions:—

I. THAT IT IS ABSOLUTELY NECESSARY FOR THE SURGEON TO POSSESS MECHANICAL APTITUDE BEFORE HE CAN DEAL SUCCESSFULLY WITH THE CASES INCLUDED IN ORTHOPÆDIC SURGERY.

In the first place, the surgeon ought not to leave to the instrument maker the devising of new or the modification of existing instruments. This fact is much more appreciated in the United States of America than it is in England.

In the second place, even where no instrument is required, the appreciation of mechanical principles and the solving of mechanical difficulties form a very important part of the orthopædic surgeon's work.

The advantage of dealing with orthopædic surgery as a special branch is based upon

Digitized by Google

the mechanical nature of the treatment, and this "mechanical nature" not only includes the use of instruments, but the adaptation of mechanical principles to every plan of correcting deformities: First, in mechanically applying antagonistic forces to counteract the abnormalities, whether (*a*) by operation, (*b*) by mechanical appliances, (*c*) by muscular exercises, (*d*) postures, or (*e*) by the combination of two or more of these forces.

To enlarge somewhat upon these points:—

(*a*) *In counteracting abnormalities by operation*, a very familiar instance may be given in the appreciation of the necessity of reserving operation upon the tendo-Achillis in cases of talipes varus until the inversion of the foot has been overcome, *i. e.*, the appreciation of the mechanical value of the fixation of the heel in the treatment of the inversion of the foot.

(*b*) *In the application of mechanical appliances.* It is very desirable that the surgeon should possess the ability himself to adjust all instruments to the patients and make alterations in them as the case proceeds, and this obviously requires mechanical aptitude.

(*c*) *By muscular exercises.* These should also be applied upon mechanical principles. An instance of fault in this respect is the manner in which exercises have been devised for the treatment of lateral curvature. It has been suggested, and is a not uncommon practice (at least in England), to recommend that the patient should exercise upon a trapeze or horizontal double bar, with the arm upon the side of the dorsal *concavity* raised higher than the other arm, with the view of drawing out the concave part of the spine. The surgeon who applies mechanical principles to this subject will see that as the muscles connecting the arm with the back are attached to the apices of the *spinous processes* of the vertebræ, the chief effect of acting with increased force upon these muscles will be to increase the rotation of the bones by the leverage exerted upon their spinous processes, whereas, bringing the arm into action upon the *convex* side from before backward, will tend to draw the vertebræ, by the same muscles upon this side, out of their rotated position toward a normal one. This is only a very general description of the theory involved, many particulars of detail having to be observed to carry out this system with effect.

(*d*) *By posture.* Here, again, it is obvious that mechanical principles are generally involved. To give an instance, I may refer to the fact that although it has for long been recognized that flexion of the spine increases the rotation, yet it has not been usually taught that enforced extension (not longitudinal extension, but the reverse of flexion) may be made a valuable method of removing this condition.

II. THAT THE SURGEON SHOULD AIM AT SIMPLIFYING MECHANICAL APPLIANCES.

This principle, it might be thought, "goes without saying;" but if we study the figures of machines to correct deformity as portrayed in mechanician's catalogues of the present day, another opinion must be arrived at, namely, that a large field is open in this direction.

A simple metal splint, adaptable by the surgeon, might often take the place of many-jointed Scarpa's shoes and elaborately made apparatus for straightening fingers.

A gutta-percha mould or plaster bandage will often prove a more effectual method of retention or support than the more expensive appliances.

III. THAT JUDGMENT AS TO COMPARATIVE ADVANTAGES OF OPERATION AND MECHANICAL PRESSURE SHOULD BE CAREFULLY AND FAIRLY GAUGED.

Upon this point I will take as an example the subject of osteotomy for knock-knees. The brilliancy of the operation, the value of which in some cases I fully appreciate and observe, seems to have unhinged the discretion of some surgeons of the day. They

Digitized by Google

have adopted this means as a sure and certain plan of straightening the limbs, and have dismissed as useless an older method which they have not the skill or patience to conduct.

It is not a matter of opinion, but of fact, that the former treatment is not so absolutely certain in its ultimate effects, while the latter, when judiciously applied, will lead to excellent results in many cases.

IV. THAT NATURAL FORCES SHOULD BE USED AS MUCH AS POSSIBLE TO ACT REMEDIALLY, both by (*a*) exercises, and also (*b*) by directing the natural movements of the body when applying instruments.

With regard to the first (*a*) nothing need be added, but in explanation of the latter (*b*) it may be stated that when instrumental treatment is required (apart from the immediate treatment after operation and the fixation of inflamed joints), the apparatus should be so constructed and applied that the actions of the body are utilized as much as possible to perform the necessary correction, and as little dependence as possible placed upon statical force.

As an example, the support of weak joints may be referred to where, instead of fixing or firmly encompassing such joints by firm bandages, a not too tightly fitting apparatus, which prevents irregular movements, while permitting freedom to those which are normal, will tend to cause the natural movements to bring about a cure without perpetuating the existing weakness.

An elastic bandage or lacing knee cap is often thought to be a simpler contrivance for these cases than a steel support. Whereas, the former conduces rather to a perpetuation of the evil, while the latter, if skillfully constructed, should act in the manner described above (of course, there are some cases in which the elastic bandage or knee-cap is necessary).

V. THAT THE SURGEON SHOULD CONSIDER THE PECULIARITIES OF EACH INDIVIDUAL CASE, AND NOT DEPEND UPON CONVENTIONAL MECHANICAL MACHINES.

VI. THAT IN DEVISING MECHANICAL APPARATUS, WE SHOULD TAKE INTO CONSIDERATION THE VARIOUS MOVEMENTS OF THE BODY, AND NOT ATTEMPT TO ADAPT A MACHINE AS IF IT WERE BEING APPLIED TO AN IMMOVABLE STRUCTURE.

Probably the cases in which it is most important that this principle should guide us, is in the use of apparatus for the spine. Even in cases of diseased bones, where fixation is our chief object, consideration for the usual movements of the trunk is necessary in first arranging such supports, and it is only by degrees that carious spines can be comfortably brought into a posture of complete repose. In lateral curvature spinal instruments are only justifiable when they permit the action of spinal muscles and allow movement to the trunk.

The progress of orthopædic surgery must, I think, be based upon some such principles as those which I have sketched.

In conclusion, I would add a few salient points which, I think, we should bear in mind when determining as to the use of mechanical appliances :—

1. Necessity.
2. Efficiency.
3. Lightness, in compliance with the necessary strength.
4. Simplicity.
5. Construction admitting of alteration by the surgeon himself.
6. Construction being such that while fulfilling the above requirements, instruments should be as inexpensive as possible, both as regards first cost and small liability to require repair and alteration.

Digitized by Google

THE TREATMENT OF LATERAL CURVATURE OF THE SPINE.

TRAITEMENT DE LA COURBATURE LATÉRALE DE L'ÉPINE.

DIE BEHANDLUNG DER SEITLICHEN VERKRÜMMUNG DER WIRBELSÄULE.

BY DR. JAMES KNIGHT,
New York.

Viewing the human form in the erect position, we observe that the entire weight of the body is borne upon a very limited base, the outline of which is an ovate circle, formed by the two feet placed together. The inner border of the outline of each foot presents a concavity directed inward, which is the lateral counterpart of the vertical curve called the arch of the foot. The only other plantigrade animal walking erect is the bear, whose foot is lacking in the elastic arch and the curvilinear inner border. Man, therefore, has much the advantage of the bear, inasmuch as the elastic arch of the human foot and coördinated muscular action render possible a great variety of attitudes and motions, with the maintenance of equilibrium. But this extraordinary and unequally distributed demand on muscular power may be attended by an impairment of force on the part of a muscle or group of muscles, thus producing an impairment of equilibrium.

This view leads to an appreciation of the readiness with which a loss of equilibrium of the spinal column is induced in predisposed persons, who habitually adopt strained positions of the body, and especially in females who sit for a long time using the right hand and arm in knitting, crocheting and doing fine needlework, or who rest a burden on the left hip while using the right arm, as is the habit of young girls who carry a babe while dusting the room. This leads me to the observation that liability to lateral curvature depends in a large degree on the station in life occupied by the individual. We have the independent *élite* who perform no manual labor. We also have families of the middle class, in which the greater part of the labor is done by hired help, while the daughters of the family are often found performing hand-sewing and at times sweeping with a heavy broom, which requires the shoulder to be advanced, and sometimes pressing ruffled dresses, fine laces, etc., with a heavy iron, which requires severe muscular exercise of the right arm and shoulder. It is this female labor that presents a most important factor in tendency to lateral curvature of the spine, and that curvature to the right side. The wealthy do not labor thus, and the indigent laboring female invigorates her muscles by more general exercise in the ordinary pursuits of her life. Hence it is that the largest number of cases of lateral curvature of the spine is to be found in the middle class of society, in girls from twelve to fourteen years of age.

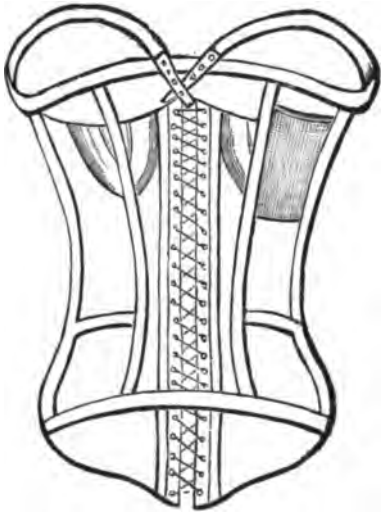
In considering the subject of treatment it is necessary to restore that uniformity of development which is found in the normal child, and the earlier this is attempted the better. The tendency of the body is to be depressed by the force of gravitation, which is imperfectly opposed by the muscular power of the trunk. It is, therefore, harmful, rather than beneficial, to place weights on the head, as is often done in the management of these cases. The immediate object of treatment should be to encourage the extension of the muscles, and this is best done, not by limiting our attention to the muscles which are believed to be affected, but rather by placing the whole body in such an attitude that the muscles will be stretched and the deformity reduced. Such attitudes should be assumed in frequent exercises, and the improvement thus gained should be maintained by a brace worn in the intervals of exercise. It has been observed that patients who apply for treatment are often in a poor general condition, with loss of appetite and a haggard appearance. Their condition forces them to come

Digitized by Google

for advice, and as they attend punctually to the treatment they are relieved. The health of the patient must be carefully attended to during the progress of treatment.

The parallel bars furnish the best form of exercise, and these should be used twice a day for five minutes, having the brace loosened or taken off during the exercise, and afterward laced tightly. The patient being relieved of all clothing that would in the least restrict muscular exercise or freedom, the bars should be elevated so as to only, at first, permit the person to sustain the upright position upon the extreme ends of the toes, the body and lower limbs being, as nearly as possible, suspended by the vertical support of the arms upon the bars. This position having been attained, the patient should then be directed to flex the leg to a right angle with the body, and to keep up an alternate extension and flexion of the body and legs until fatigue gives notice of the necessity of rest, when a suitable support of equal elevating force should be applied to the body while in a suspended position.

The brace (figured below) consists of upright bars, but the sides and back consist of extending bars from below the hips to the axillæ, and shaped to conform to the body



in a normal state. They are conjoined by riveting them to a belt bar, more than half encircling the body for the upper circle and the same for the hips. In a line from the hips upward (for an ordinary person) a four-inch plate, about four inches square, for the right shoulder, and two cross strips of steel about two inches lower down, thus forming a waist, and you have the steel frame ready for a brace. The steel is three-eighths of an inch in width and not one-eighth of an inch in thickness, which is heavy enough if the metal is fine. It is covered with twilled muslin, the steel having a padding of blanket, which is increased in thickness under the arms. There are straps to pass over the shoulders, and all the bars have an outer covering of leather. The lacing in front is attached to the two bars which extend from below the hips to the armpits, and made to fit the body properly immediately in front of the lacing.

The braces are intended to maintain what has been gained by extension upon the bars; hence, the necessity of lacing up carefully after exercising, in order not to lose what has been gained by the practice on this valuable means of relief. Tight lacing in other circumstances has proved a serious matter, but lacing as here directed is differ-

Digitized by Google

ent, because the upright bars prevent any injury which lacing under ordinary conditions might inflict. The bars press outward over the irregular surface so as to protect the rest of the body.

During a period of twenty-three years 2072 cases of lateral curvature have been treated in the Hospital of the New York Society for the Relief of the Ruptured and Crippled. Of these, 1667, or 80.5 per cent., were females and 405, or 19.5 per cent. were males. 187, or 9 per cent. were over twenty-one years of age, 976, or 47 per cent., were between fourteen and twenty-one years, and 909, or 44 per cent., were under fourteen years. Many of the latter class were obscure in their origin, some being congenital and others the result of pulmonary disease. Of the whole number, 1723, or 83 per cent., were of the projecting right shoulder, and 349, or 17 per cent., were to the left.

THE AMERICAN HIP-SPLINT.

ÉCLISSE AMÉRICAINE DU FÉMUR.

DIE AMERIKANISCHE HÜFTSCHIENE.

BY DR. A. B. JUDSON,
New York.

In the present Congress, the first held in America, it will not be thought inappropriate to devote a short paper, chiefly historical in its character, to the American splint for the treatment of hip disease.

This apparatus was first described by Dr. Henry G. Davis and Dr. Lewis A. Sayre, in the April number of the *American Medical Monthly*, published in 1860. These two surgeons wrote independently, but by a curious coincidence they both described a new splint which was recognized as an important invention, not only in this country, but especially in England and France, where it was known as the American splint. Under this name it has been described and discussed by Edwards[1], Barwell[2], Holmes[3], Marsh[4], Adams[5], Thomas[6], Bouvier[7], Le Fort[7], Velpeau[7], Verneuil[7] Giraldes[7], Armand[8], E. Boeckel[9], Hennequin[10], Monod[11] and Philipeaux[12], and doubtless by other eminent European surgeons.

It will be interesting to inquire whether the name American has been rightly given to this apparatus? As first described, in 1860, it has two important features: (1) A perineal strap or ischiatic crutch-head, for the purpose of keeping the weight of the body from resting on the affected limb, the patient being thus enabled to engage actively in ordinary pursuits while wearing the splint and (2) adhesive plaster applied with the view of making traction on the limb.

---

[1] *Edinburgh Medical Journal*, June, 1861, p. 1118.
[2] *Lancet*, Nov. 7th, 1863, p. 530. "Diseases of the Joints," 2d Edition, London, 1881, p. 467.
[3] "Diseases of Infancy and Childhood," 2d Edition, London, 1869, p. 432.
[4] *British Medical Journal*, July 28th, 1877, p. 98.
[5] *British Medical Journal*, Jan. 5th, 1878, p. 10.
[6] "Review of the Treatment of Hip Disease," Liverpool, 1878. Preface.
[7] *Bull. de la Soc. de chir.* 1866, pp. 122, 126, 147, 154.
[8] *Thèse de Paris*, 1878, p. 37.
[9] *Bull. gén de Thérapeutique*, 1875, p. 451.
[10] *Arch. gén.*, Jan., 1869, p. 62.
[11] *Arch. gén.*, June, 1878, pp. 704–712.
[12] "De la Coxalgie," Paris, 1867, pp. 261, 262, 284.

Digitized by Google

In regard to these two features, ischiatic support and traction by the use of adhesive plaster, the first was not an American invention, nor was it a novelty. Support of this kind had been used for a long time in the construction of artificial limbs, and even in the treatment of hip disease the possibility of so supporting the body had occurred to M. Ferdinand Martin, a wood-cut of whose splint is found in Bonnet's "Treatise on Diseases of the Joints," published in 1853.

But when we come to consider the other remarkable feature of this splint, we recognize a real advance in mechanical surgery, and one which may rightly be called American. The use of adhesive plaster for prehension of the limb, in the treatment of fracture of the long bones, was an American invention, and the transfer of this device from the treatment of fractures to that of hip disease was first effected in the new splint. For many years it had been a common practice in the treatment of hip disease to make traction with the long splint for fracture of the femur, prehension of the limb being made by a gaiter, or fillet or handkerchief placed around the ankle. These instruments of torture were supplanted in the new hip splint by the absolutely comfortable and convenient adhesive plasters. Thus we see that the new splint was a combination of an old device, ischiatic support, with an American invention, traction by adhesive plaster, and as the happy combination was made in America, it is not strange that the courteous attitude of European surgeons toward the surgery of a comparatively new country, led them to call the new method the American method, and the new splint the American splint.

Following the history of the hip splint in this country for the past twenty-seven years, one is amazed at the great number of the so-called improvements that have been made upon it. The most important has been a perfecting of that part of the apparatus which provides for ischiatic support of the body in standing and walking. The first splint did not extend to the ground, but depended on the integrity of the plaster adhesion for keeping the weight of the body from resting on the inflamed joint. Dr. Edmund Andrews,[1] of Chicago, and Dr. C. Fayette Taylor,[2] of New York, proposed and perfected an extension of the splint to the ground, and thus left but little to be desired as an ischiatic crutch. Aside from this great improvement no essential changes have been made. Experience and increasing light have shown that certain things which it was thought that the splint accomplished are mechanically beyond its reach, and that some things supposed to be desirable and even necessary to proper mechanical treatment are of no importance whatever. The two things which the splint does today, and which it has done ever since the improvement above mentioned, the two functions of the splint, so to speak, are (1) to make the affected limb a pendent member, resembling in this respect the arm, when the patient is erect,[3] which it does as an ischiatic crutch, and (2) to apply traction to the distal member of the joint, which it does by its rack and pinion and adhesive plaster. Traction protects the joint from the traumatism of motion, muscular or otherwise, and the ischiatic support protects it from the traumatisms of standing and walking, while the patient runs about and follows the ordinary pursuits of life for the months and years necessary to bring about a recovery with restoration of ability and symmetry, so far as may be.

I will close by briefly referring to two points of practical utility. The first is in regard to an early diagnosis, which is especially of great importance, inasmuch as there is reason to believe that if treatment can be begun sufficiently early the focus of osteitis

---

[1] *Chicago Med. Examiner*, Dec. 1860, pp. 753, 754.

[2] *Medical Record*, Sept. 1st, 1867, pp. 289–291.

[3] It is interesting in this connection to recall the words of M. Hennequin: "Mais le corps humain peut-il conserver pendant des mois entiers l'attitude verticale, touchant le sol par un pied seulement? Évidemment non; c'est au-dessus de ses forces."—*Arch. gén.*, Jan., 1869, p. 64.

Digitized by Google

in the cancellous tissue may be resolved before the other structures of the joint are involved. Reason for this belief is found in the fact that disease of the joints is comparatively rare in the upper extremity, where a focus, being in a pendent member, may undergo resolution, protected, as it is by the nature of the case, from the traumatisms which assail the lower extremity in standing and walking.

Now, if the lower extremity can be made pendent, as can easily be done by the use of the hip splint, in the very incipiency of articular osteitis of the hip, before the articular contours are changed and before the circumarticular muscles are seriously involved, we may look for resolution of the osteitic focus and recovery without lameness or impairment of motion.

To assist in making an early diagnosis in a doubtful case a careful study should be made of those limitations in the motions of the joints which become apparent only when the extremes of normal motion are approached. This may be done in various ways. I have found two methods easy in practice and certain in their revelations. The first method applies to rotation, which is a direction in which limitation of motion first takes place. Let the patient lie supine with the feet slightly apart. With the hand placed lightly on the knee of the unsuspected limb a rocking or oscillating motion is given to the whole limb, outward and inward rotation following each other, while the toe sweeps through an arc of nearly 180°, the inner border of the foot striking the table, and the outer border nearly reaching that level. This occurs in the well limb. A similar manipulation of the suspected limb may reveal a slight limitation of rotation, the result of hip disease. The other simple procedure relates to flexion. Let the patient, still on the table, sit up and kiss the knee. By flexing the neck and back and drawing the limb up with the hands this can easily be done with the unaffected limb, while the attempt to do it with the suspected limb may reveal a slight limitation of flexion indicative of hip disease.

Another diagnostic sign, too little thought of perhaps, but of importance in the very early stage, has recently been referred to by Dr. A. J. Steele,[1] of St. Louis, Missouri, as "a brawny thickening about the joint in front of the capsule or behind the trochanter." There will in some cases be found a condensation of the soft tissues, due apparently to the vicinity of osteitis, not visible, perhaps, but recognized by palpation or pinching with the thumb and finger, and then often not detected, excepting by comparing the two sides. It will be found that a smaller pinch of the skin and underlying tissue can be made on the well than on the affected side. These tests are to be used, of course, in connection with other diagnostic helps and with due regard to other conditions which have the power to produce similar phenomena. Properly used they may betray the presence of hip disease in a patient as yet entirely free from pain and lameness.

The other practical point which I would emphasize relates to the position of the limb. Adduction is most to be dreaded. It causes tilting of the pelvis and apparent shortening, which, although technically *apparent*, produces more disability and deformity than the shortening which is called real. It is due, as a general thing, to the fact that the patient uses the well limb more than the affected one in walking, putting the former forward in less time than the latter, and unconsciously keeping the affected limb off the ground more than half of the time, and drawing up and adducting it in order to make it less of an impediment. To remedy and prevent this, the patient, during and after treatment, should be drilled in rhythmical walking, which compels the affected limb (protected by the splint during treatment) to do its full share of the work of locomotion, and leads the patient unconsciously to thrust the affected limb down and to abduct it so that it may be in the best position to receive the weight of the body, and do its half of the work of progression. It is gratifying to witness a recovery in which

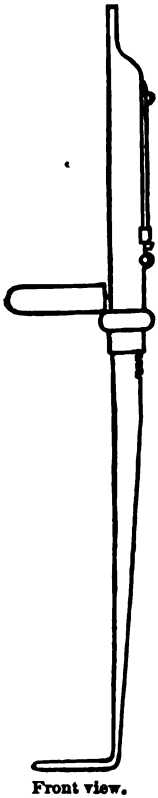
[1] *Transactions* Missouri State Medical Association, 1887, p. 102.

Digitized by Google

real shortening is more than counterbalanced by apparent lengthening. Although this may be the case when the patient is discharged, the abduction, which is so favorable a feature, is likely to disappear and give place to adduction, with its disability and deformity, if the gait is allowed to become habitually irregular.

Figures 1, 2[1] and 3[2] will give an idea of modifications made in the hip splint by the writer.

FIG. 1.

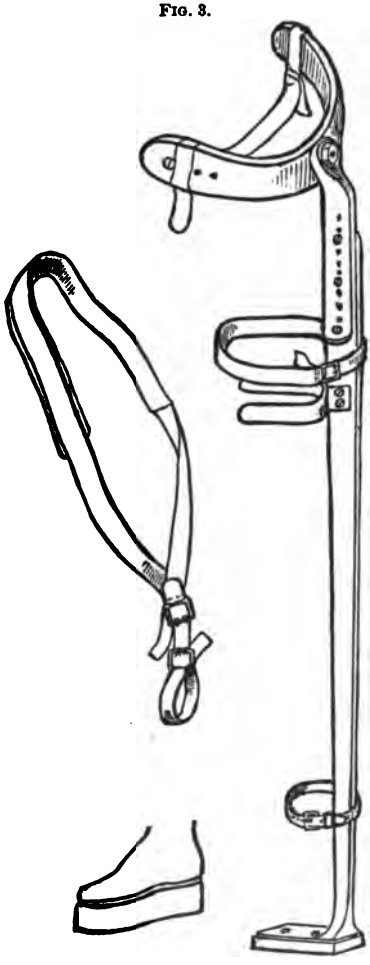


Front view.

FIG. 2.

Side view.

FIG. 3.



In closing, I would deprecate a tendency to complicate the mechanics of the hip splint. If its true functions, which are few in number and simple, and the limitations of its usefulness, are duly recognized, it will be found a most useful and convenient appliance.

---

[1] *New York Medical Journal*, January 24th, 1885, pp. 111, 112.
[2] *Medical Record*, June 25th, 1887, pp. 721, 722.

Digitized by Google

ON CONTRACTED KNEE JOINT AND AN APPLIANCE FOR RAPID REDUCTION OF SUBLUXATED TIBIA.

SUR LA CONTRACTION DE L'ARTICULATION DU GENOU ET UN REMÈDE POUR RÉDUIRE RAPIDEMENT LA SUBLUXATION DU TIBIA.

ÜBER DAS CONTRAHIRTE KNIEGELENK UND EINEN APPARAT ZUR SCHNELLEN REDUCTION DER SUBLUXATION DER TIBIA.

BY DR. E. H. BRADFORD,

Boston, Mass.

Contracted knee joints can be straightened in several ways.

1. By gradual mechanical means.
2. By osteotomy or wedge-shaped excision.
3. By force suddenly applied (brisement forcé).

These methods have their respective merits, each suited to a certain class of cases.

The latter is a well-recognized procedure with comparatively little risks in proper cases.

The chief dangers in forcible straightening are—

1. Danger of exciting a severe arthritis.
2. Fracture of the bones forming the joint.
3. Rupture of the popliteal artery.

The first of these can be avoided by the exercise of judgment in selecting proper cases; the other two by care in the application of force and in the amount used.

It is advisable that the greatest amount of force should be applied in such a way that if an excess is used damage to the bone and artery will not take place. The first rupture of the adhesions glueing the joint together is the part of the procedure which demands most force, and it is safer that this be done by forcible flexion rather than forcible extension.

Forcible flexion is best done in the following way :—

The patient, anæsthetized, is placed on the floor, or low table; the surgeon, stooping over the patient, holds the affected limb with both hands placed under the popliteal space, the thumb pressing on the patella. The patient's ankle is firmly held between the surgeon's knees. It will be found that by bending the knees the surgeon's whole weight can be thrown upon the end of the patient's limb (*i. e.*, the long arm of the lever). The surgeon is able to adjust carefully the amount of force that he will use, by regulating the amount of pressure he exerts. He can be guided, in a measure, by the sensation felt in his hands grasping the patient's knee. After adhesions have been thoroughly broken down in this direction, the patient is to be turned on his face and a cushion or hard pillow placed under the patella; forcible extension is then to be used, with careful and gradually increasing force, the surgeon holding the patient's ankle in his hands and an assistant holding the knee.

Tenotomy of the ham-string tendons is advised, but it will not be found to be of great assistance as a rule, as the chief obstacle to correction is in the ligaments and fasciæ. After the limb is straight, it is desirable that it be kept straight, as a tendency for recontraction remains. The most ready way is to fix the limb in a plaster-of-Paris bandage. The limb is covered with a layer of sheet wadding with an extra amount of thickness over the patella. The gypsum roller bandages are applied in the ordinary way. The limb is to be held in the corrected position while the bandage is becoming hard, which should not require, with properly prepared bandages, longer than five minutes. If necessary, the plaster can be reinforced by a strong strip of steel, secured by a bandage on the outside of the plaster bandage and removed after the plaster is thoroughly

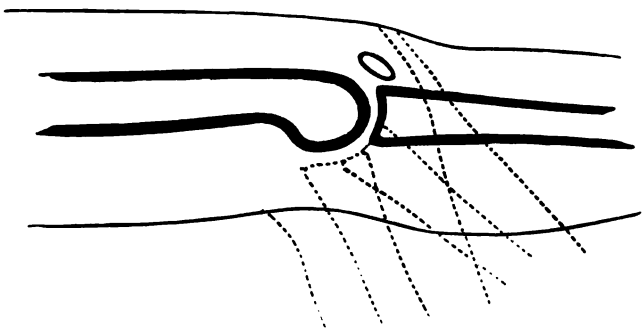
Digitized by Google

hard. The easiest way to hold the limb in a proper position while the plaster is becoming hard, is for the surgeon to seize the foot with his hand, pressing upward, while an assistant pulls in an opposite direction downward on a strip of bandage placed over the thigh just above the knee. This loop of bandage is to be removed when the plaster has become hard.

It is necessary that the plaster should hold the leg firmly and should extend as high in the thigh as possible. In addition to, or in place of, the plaster-of-Paris bandage, a Thomas knee splint can be used.

Forcible manual straightening meets little difficulty in the simpler cases, but in the more obstinate ones resistance is encountered beyond a certain point, and an attempt to completely straighten the limb will leave a more or less complete subluxation of the tibia. This is due in part to the spasm of the ham-string muscles, which has pulled the tibia backward, but chiefly to the fact that owing to adhesions the flexed tibia is unable to slide forward over the condyles of the femur, as happens in normal extension. Raising the leg simply crowds the anterior edge of the tibia into the condyles. To obviate this the head of the tibia should be pressed forward and upward to the same degree that the leg is raised. (Diagrams I and II.)

DIAGRAM I.



Normal motion (flexion and extension) at knee-joint.

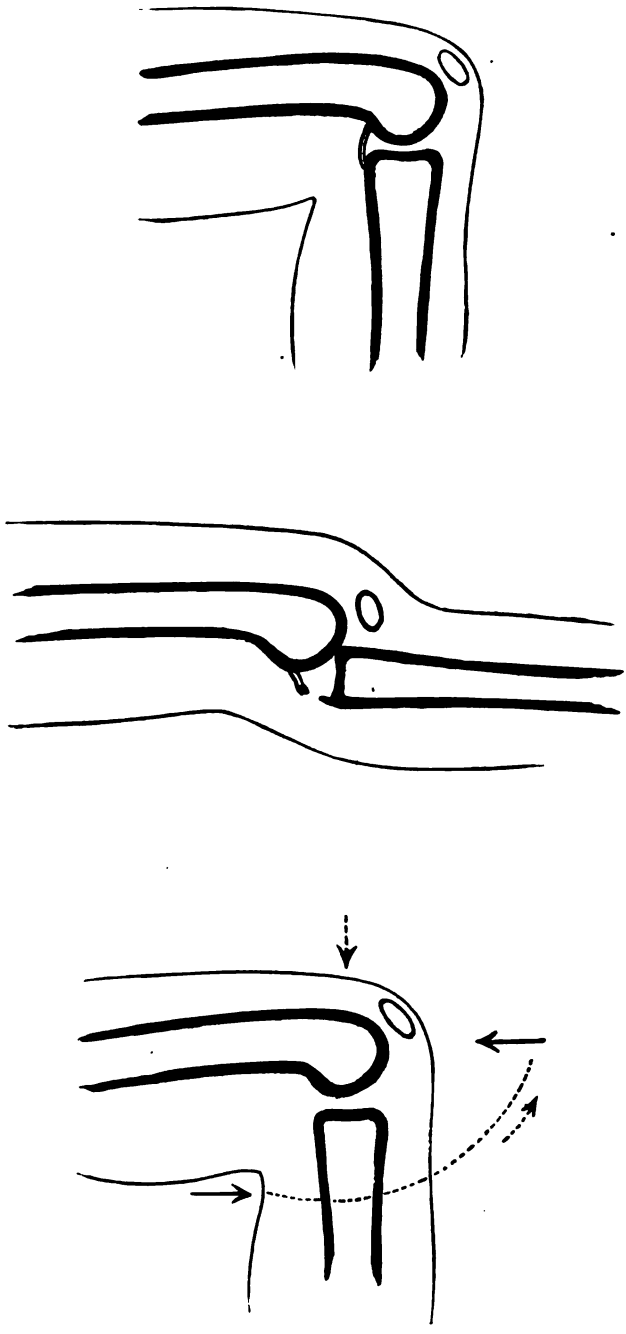
Several means have been used to effect this. The writer has found the method illustrated in the accompanying diagrams of use in the most obstinate cases. (Diagrams III, IV and V.)

Pressure forward on the head of the tibia is exerted by turning the handle A; this, by means of a screw force, pushes the plate C forward, working through the band B. The calf muscles protect the artery and nerve from injurious pressure. Counter-pressure is secured by means of leather straps D and E, which are passed respectively over the knee and leg, protected by a thick layer of saddler's felt. Several straps will be needed at the knee to prevent loss of counter-pressure, as the limb is made straighter. Another strap, F, under the leg, secures the lower part of the leg. The side bars, bands and plate of the apparatus should be of strong steel.

The apparatus is put on the limb in a flexed position (after rupturing adhesions by forcible flexion if that is needed), the head of the tibia is pushed forward as far as is advisable, and, by means of the end of the appliance, which serves as a handle, the leg is extended; the pressure forward of the head of the tibia can be increased, if necessary, and the place of counter-pressure regulated, if necessary, by loosening such of the straps D D as extension of the limb may tighten too much.

Digitized by Google

DIAGRAM II.



Illustrating extension in contracted knee-joint.

Digitized by Google

When the limb is partly corrected, strong downward pressure on the femur will be needed. This can be secured by means of a strap (Diagram V), which, passing over the knee (protected by padding) is secured below to the floor or to a strong rod, H, on which the operator or his assistant can place his foot, regulating the amount of pressure.

Experiments on the cadaver which were conducted at the Harvard Medical School, through the courtesy of Drs. C. B. Porter and T. Dwight, showed that by means of this appliance the tibia could readily be pushed forward to any desired extent. On normal joints, the tibia can be pushed forward to a considerable distance with-

DIAGRAM III.

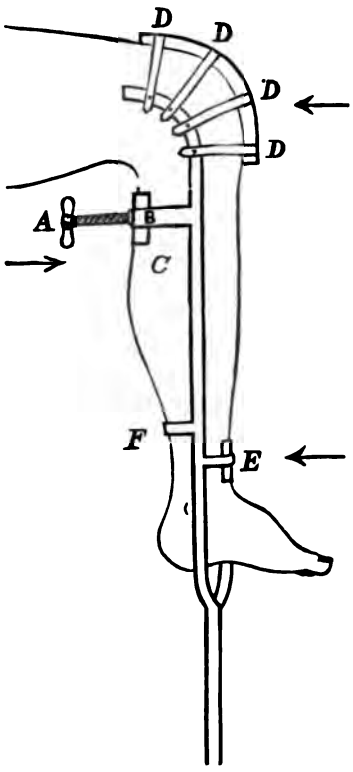
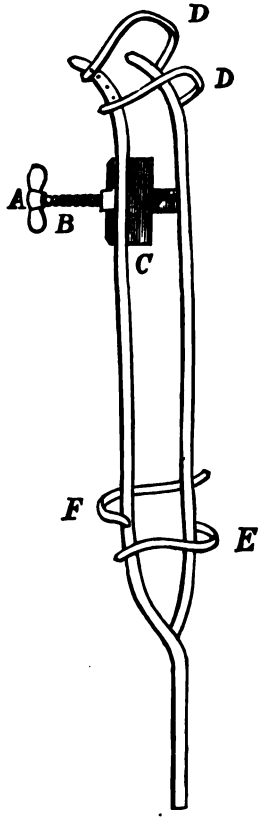


DIAGRAM IV.



out rupturing the ligaments. The first to rupture was the posterior band of the crucial ligament. The tibia could be pushed forward over an inch without rupture of the anterior band of the ligament or the posterior capsular ligament.

In the cases V and VI, on which the appliance was used, force to this extent was not required, and the subluxation was corrected without great difficulty.

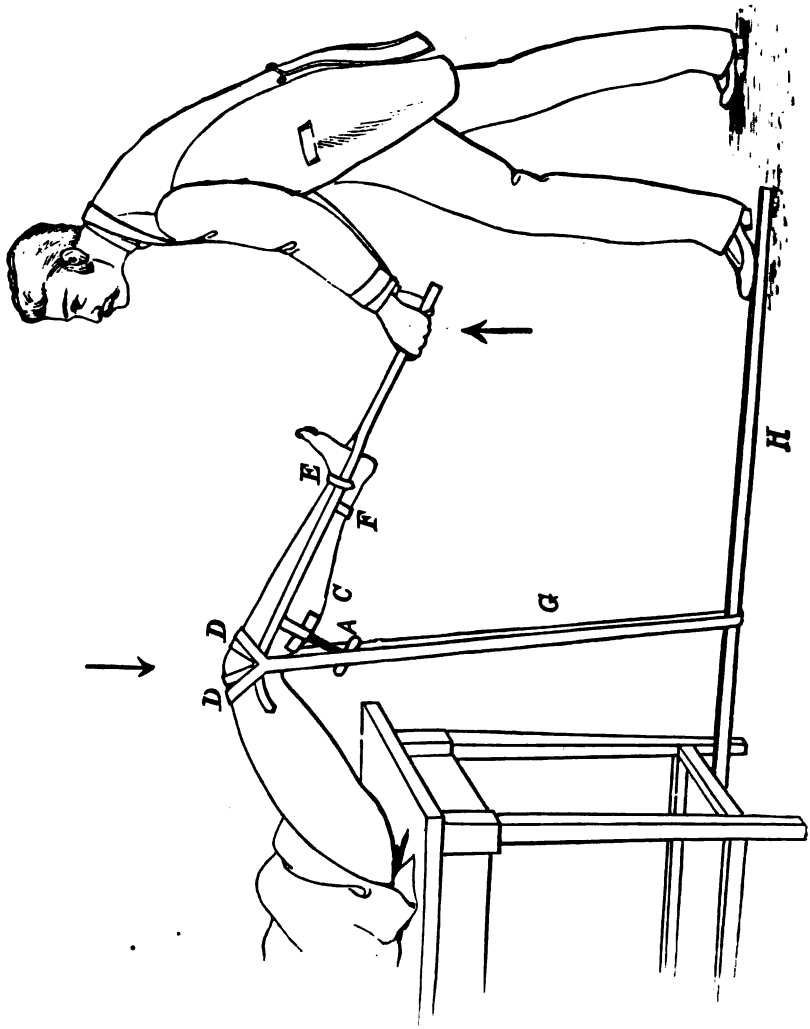
Clinical experience has shown that in osteoclasis the skin can stand, without danger, all the pressure needed in this procedure. In the case where the appliance was used no injury to the skin took place.

Some difficulty was encountered in keeping the straps D D in position, and they

Digitized by Google

required some adjustment and watching; and it appeared that they might exert injurious pressure in cases with any existing inflammation of the synovial membrane, which would contraindicate the use of the appliance. In old contracted cases this trouble would not be encountered. The patella can be protected by proper arrangement of padding and adjustment of the strap.

DIAGRAM V.



The patient, during the operation, should be, of course, completely anæsthetized, and the appliance should not be applied so as to exert pressure an unnecessarily long time. Some time, however, will be needed for properly stretching the ligaments, but, as a rule, a pressure of not more than a few minutes will be needed for complete reduction of the tibia.

Digitized by Google

The apparatus will be of use in a comparatively small number of cases, but when it is used, its efficiency will be found of value.

The limb should be fixed after correction, as has already been described.

The fixation bandage should be worn for a few weeks, and after this, for several months, a proper mechanical support is advisable. The time required for the use of crutches and support varies with different patients.

In the following cases of right-angled contraction at the knee joint, occurring in adults, forcible correction was performed. Cases occurring in children are not here mentioned, except Case I.

CASE I.—Boy of fourteen, patient at Carney Hospital. Rectangular contraction following old tumor albus many years before. Ending in a cure by anchylosis.

Tenotomy. Forcible straightening; separation of the lower epiphysis of the femur; immediate fixation in plaster bandage in a nearly straight position. Patient heard from two years afterward, as having a useful leg.

CASE II.—Man, twenty-one years of age. Some contraction at the knee following old tumor albus; apparently nearly well. Patient at St. Margaret's Infirmary. Forcible straightening; fixation in ham splint and stiff bandage. Fixation bandage removed too soon. Not much improved.

CASE III.—Lady, twenty-five years of age. Rectangular deformity at knee following puerperal rheumatic arthritis one year before. No motion at joint noticeable without an anæsthetic. No subluxation. Forcible correction, attended by a loud report and a sensation of tearing of connective tissue; immediate fixation of the limb in plaster bandages. Complete correction of the limb. Patient heard from four years after the operation; walks without cane, with a slight limp; limb perfectly useful, but without motion at the knee.

CASE IV.—Lady, aged forty-five. Rectangular contraction at the knee, of one year's standing, said to follow slight injury. Tenotomy. Forcible straightening; immediate fixation. Result—a straight limb, with some motion. Patient heard from three years after operation; limb said to be useful.

CASE V.—Woman, twenty-one years old. Former tumor albus; contracted knee since she was five years old. Forcible correction (without mechanical appliance); immediate fixation. Nearly straight limb, but without correction of the subluxation of the tibia; useful limb.

CASE VI.—Woman, twenty-four. Former tumor albus; right-angled contraction at the knee-joint for eighteen years. Marked backward subluxation of the tibia. Patient unable to step at all on the limb. Forcible correction with the help of mechanical appliance; correction of the luxation; contraction corrected; immediate fixation. Marked genu-valgum followed, due to the lengthening of the internal condyle. This relieved by the McEwen operation. Straight limb. The limb was then in a relatively normal position and free from subluxation. Three months afterward she was discharged from the hospital. At that time she was able to bear nearly full weight upon the limb. She wore, however, a retention stiff bandage, and used one crutch in walking. At the present time (a year later) she is able to walk without crutch or cane, but usually uses a cane.

BIBLIOGRAPHY.

Bauer. "Two cases of brisement forcé; successful." *Med. and Surg. Rep.*, Phila., 1858–9, I, 173, 189.

Bauer. "False anchylosis and contracture; tenotomy and forced extension. Recovery." *St. Louis Med. and Surg. Journal*, 1870, N. S., VII, 200.

Detmold. "Forcible breaking up of adhesions in anchylosis of knee joint." *N. Y. Med. Times*, 1852, II, 207.

Hancock. "Angular anchylosis of knee joint; forcible extension." *Lancet*, 1853, I, 111.

Digitized by Google

Hauner. "Ankylosis genu sinistri; brisement forcé." *Oesterreich. Ztschr. f. Kinderh.* Wien, 1856–7, II, 421.

Henry. "Forcible flexion of ankylosed knees." *Lancet*, 1859, II, 411.

Heyfelder, J. F. "Ueber die forcirte Extension bei Kniecont." *Oesterreich. Ztsch. f. prakt. Heilk.*, Wien, 1857, III, 493.

Heyfelder (O.). "Heilung . . . durch Exten. forcée." *Deutsch Klin.*, Berlin, 1851, III, 422.

Holl. "Zerreissung der Kniekehlengefässe und Nerven bei Streckung einer Contractur, etc." *Arch. f. Klin Chir.*, Berlin, 1878, XXII, 374.

Holston. "Straightening . . . by forcible extension." *Am. Med. Gaz.*, N. Y., 1858, IX, 581.

Homans. "False anchylosis; forcible movements; hemorrhage; ligation of femoral artery. Recov." *Boston Med. and Surg. Jour.*, 1876, XCV, 478.

Lloyd. "Laceration of an anchylosed knee." *Med. Times and Gaz.*, London, 1858, XVII, 61.

MacDonnell. "Contraction; rapid extension (several cases)." *Med. Chron.*, Montreal, 1858, V, 98 *et seq.*

Nagel. "Rheumatische Kniecontract. geheilt durch das Brisement forcé." *Deutsche Klinik*, Berlin, 1856, VIII, 436.

Salzer. "Ueber Zerreissung der Art. poplitea, etc., in Folge gewaltsamer Streckung der Kniegelenkcontractur." *Wien. Med. Wochenschr.*, 1884, XXXIV, 216, 246.

Smyly. "Forcible flexion of the knee for anchylosis in the straight position." *Dublin Q. J. M. Sc.*, 1866, XLI, 18.

Stabel. "Anchylosis in beiden Kniegelenken durch einmalige gewaltsame Streckung, etc., etc., geheilt." *Deutsche Klinik*, Berlin, 1860, XII, 367.

Stillmann. "Suggestions on the management of brisement forcé." *J. Am. M. Ass.*, Chicago, 1884, II, 203.

Wenz. "Gewaltsame Streckung, etc." *Ztschr. f. Wundärzte und Geburtsh*, Stuttg., 1864, XVI, 18.

Wiskemann. "Zur Streckung und Mechanik der Kniegelenkscontract., etc." *Berl. Klin. Wochenschrift*, 1882, XIX, 365.

Whitehead. "Notes of some cases of recent hip-joint disease, including two cases of excision and one of brisement forcé, and remarks on some others." *Prov. M. J.*, Leicester, 1887, VI, 152; also *N. Y. Med. Rec.*, 1886, XXX, 679.

Marsh, Howard. "Dis. of Joints."

Stillman. "Mechanical treatment of knee-joint disease." *N. Y. M. J.*, XLV, 37.

Lampagnani. "Delle ankilosi." *Arch. di ortop*, Milano, 1886, III, 97

Oberst. "Vier Fälle von akuten Eiterungen nach Brisement forcé." *Centralbl. f. Chir.*, Leips., 1885, XII, 361.

Gomez Pamo. "Dos casos de extension forzada de la rodilla." *An. d. ciruj.*, Madrid, 1883, II, 135.

Deakin. "On buttoning, or the forcible manipulation of stiff or weak joint." *Proc.* N. W. Prov. and Oudh. Branch Brit. Med. Ass., Allahabad, 1883, II, 381.

Black. "A possible danger in the forcible treatment of ankylosis." *Kansas City M. Rec.*, 1884, I, 251.

Knauf. "Die operative Behandlung der Ankylosen des Kniegelenkes." Jena, 1884, Pohle, 8°.

Barwell. "Ankylosis after chronic joint disease." *Lancet*, 1884, II, 814.

La Grange. "Traitement de l'ankylose du genou." Paris, 1883.

Sayre. "Amputation below knee joint in preference to *brisement forcé* in certain cases of deformity and ankylosis—two cases." *J. Am. M. Ass.*, 1883, I, 325.

Louvrier. "Nouv. traitement; mort par une cause étrang.; autres accidents." *Gaz. d'hôp.*, Paris 1840, 2. s., II, 66.

Philipeaux. "De la valeur et desindication de la rupture, etc." *Gaz. hebd. de Méd.*, Paris, 1865, 2. s., II, 67.

Bauer. "Clinical Lecture on contraction of knee joint and false anchylosis." *Am. M. Gaz.*, N. Y., 1858, IX, 193.

Bauer. "Case of contraction of right knee joint; unsuccessful grad. extension and *brisement forcé*; sloughing; paralysis; repeated *brisement* after myotomy; failure; amputation and recovery." *Phila. Med. and S. Rep.*, 1861–2, N. S., VII, 37.

Digitized by Google

Bauer. "False anchyl.; unsuccessful attempts at forcible extension; amputation." *N. Y. Med. Rec.*, 1867–8, II, 64.

Stanley. "False anchylosis of knee joint . . . forcible extension." *Lancet*, 1853, I, 538.

Cartade. "Etude sur le traitement de l'ankylose incomplète du genou par la rupture." Paris, 1873, 4°.

Chaboux. "De la rupture de l'ankylose, etc." Paris, 1879, 4°.

Dubinsky. "Die sogenannte Streckung . . bei Contracturen und Ankylosen des Kniegelenkes mit vorzugsweiser Berücksichtigung der in der Dorpater chir. Klin. zur Behandl. gekommenen Kniegelenkverkrümmungen." 8°, 1870.

Langenbeck. "Die gewaltsame Streckung der Kniecontracturen, etc." 8°, Hannover, 1858.

Morsdorf. "Ueber die Methode, durch gewaltsame Streckung, etc." 8°, Leipzig, 1867.

Uhlig. "De contracturis artic. genu . . per celerem ac violentem extentionem." 8°, Leipzig, 1853.

Vergne. "Du traitement . . par le redressement brusque." 4°, Paris, 1875.

Adams. "On the selection of cases for forcible movement in the treatment of stiff joints, and the method of procedure." *Brit. M. J.*, 1882, II, 666.

V. Bruning. "Ueber Kniecontractur und deren Streckung." *Pest. Med.-chir. Presse*, Budapest, 1881, XVII, 88 and 109.

Hay. "Case of stiff knee cured by forcible flexion." *Phila. Med. Times*, 1880–1, XI, 457.

Bauer. "Angular contraction with fibrous anchylosis of arm." *St. Louis Clin. Rec.*, 1879–80, VI, 238.

Bauer. "Joint diseases; old practices and recent views." *St. Louis Clin. Rec.*, 1878, V, 205.

Gamgee. "Treatment of ankylosis of knee." *Brit. M. J.*, 1878, II, 689.

Jones. "Tenotomy for contracted knee joint and forcible extension; the propriety of excision, etc." *Lancet*, 1879, I, 764.

Frank (P.). "De contractura et ancylosi artic. genu et coxæ, iisdemgen B. Langenbeckii methodo violenta extensione sanandis." 4°, Berolini, 1843, also *Med. T. and Gaz.*, London, 1853, N. S. VII, 59, 134, 473.

Adams. "On Restoration of motion . . . by forcible extension." *Med. Mirror*, London, 1865, II, 193, 257.

Küchler. "Ueber die gewaltsame Aufhebung der erworbenen Contracturen der Gelenke durch Händegewalt, ihre Anzeigen und die Verhütung von Zufällen." *Deutsche Klinik*, Berlin, 1859, XI, 377, 388.

Ollier. "Ankylose." *Dict. Enc. de sc. Med.*, Paris, 1866, V, 183.

Bauer and Whaley. "Cases of Contraction, etc." *Am. M. Times*, N. Y., 1861, II, 209.

Brodhurst. "On Forcible Extension, etc." *Med. T. and. Gaz.*, London, 1858, XVI, 550, 597, and 649.

Hawkins. "Forcible flexion in, etc." *Lancet*, 1859, II, 537.

Kade. "Ueber Ankylosenbrechung." *St. Petsbg. M. Ztsch.*, 1862, II, 1, 33.

Laffan. "Forcible rupture, etc." *Lancet*, 1872, II, 882.

Digitized by Google

Digitized by Google

SECTION XI—OPHTHALMOLOGY.

OFFICERS.

PRESIDENT: PROF. JULIAN J. CHISOLM, Baltimore, Md.

VICE-PRESIDENTS.

Prof. C. H. ABADIE, M. D., Paris, France.
W. A. BRAILEY, ESQ., London, England.
Prof. A. W. CALHOUN, M. D., Atlanta, Ga.
Prof. L. DE WECKER, M. D., Paris, France.
Prof. J. HIRSCHBERG, Berlin, Germany.
Prof. P. D. KEYSER, M. D., Philadelphia, Pa.
Prof. E. LANDOLT, M. D., Paris, France.
Prof. N. MANOLESCU, Bucharest, Roumania.
Prof. L. MAUTHNER, M. D., Vienna, Austria.
P. H. MULES, ESQ., Manchester, England.
H. POWER, ESQ., F.R.C.S., London, England.
Prof. D. S. REYNOLDS, M. D., Louisville, Ky.
J. TWEEDY, ESQ., London, England.
Prof. E. WARLEMONT, M. D., Brussels, Belgium.
Prof. E. WILLIAMS, M.D., Cincinnati, Ohio.

SECRETARIES.

Prof. ADOLF ALT, M. D., St. Louis, Mo.
Dr. ROBERT L. RANDOLPH, Baltimore, Md.
Dr. J. A. WHITE, Richmond, Va.

COUNCIL.

Dr. S. C. Ayres, Cincinnati, Ohio.
Dr. B. J. Baldwin, Montgomery, Ala.
Dr. Ole Bull, Christianna, Sweden.
Dr. F. Buller, Montreal, Canada.
Dr. L. Conner, Detroit, Mich.
J. Richardson Cross, Esq., Bristol, England.
Dr. H. Culbertson, Zanesville, Ohio.
Dr. H. Ferrer, San Francisco, Cal.
Dr. Galezowski, Paris, France.
Dr. Gayet, Lyons, France.
H. B. Hewetson, Esq., Leeds, England.
Dr. A. G. Heyl, Philadelphia, Pa.
Dr. F. C. Hotz, Chicago, Ill.
Dr. W. V. Marmion, Washington, D. C.
Dr. A. Mooren, Düsseldorf, Germany.
Dr. J. Samelsohn, Cologne, Germany.
Dr. W. H. Sanders, Mobile, Ala.
Dr. A. G. Sinclair, Memphis, Tenn.
Dr. E. Smith, Detroit, Mich.
Dr. G. T. Stevens, New York.
Dr. J. L. Thompson, Indianapolis, Ind.

Digitized by Google

FIRST DAY.

OPENING ADDRESS OF THE PRESIDENT,

JULIAN J. CHISOLM, M. D.,

Of Baltimore, Md.

GENTLEMEN AND COLLEAGUES.—My first duty is to express my very high appreciation of the honor conferred upon me to preside over so important a Section as that of Ophthalmology, and then to extend to you a hearty welcome to Washington.

That man proposes, but is not always permitted to carry out his good designs, is strikingly illustrated in the absence from this chair of our good and beloved colleague, Dr. Elkanah Williams, who by universal consent was selected to organize this Department of the International Medical Congress. His thorough training, large experience, and fund of knowledge well fitted him for so important a trust. It is much to be regretted that ill health has forced him to relinquish what every one deemed a very fitting honor. We regard Dr. Williams with pride as the pioneer among specialists in the United States.

It is curious to contemplate what radical changes in professional opinion a very few years produce. Thirty-five years ago, when Dr. Williams, after a careful preparation, announced that his practice would be restricted to the treatment of eye and ear diseases, it needed courage to face the universal condemnation and denunciation of the medical profession. At that time, in the United States, the practice of ophthalmic surgery, the first-born of the specialties, was in the hands of itinerants of very unsavory reputation; and a regular physician, however honest his purpose to prosecute a special line of study and practice, was so ostracized for his imprudence and independence that he had to seek again shelter in the body of the medical profession.

How totally changed is all this to-day, when it is acknowledged that progress is only attained in medicine through special studies. No organ of the living body is now deemed so small as not to need the laborious work of well-trained investigators, who may by diligent application find out some of the many hidden wonders of creation. To-day, this Ninth International Medical Congress is to a large extent an assemblage of honored specialists, the brightest lights of the medical profession. These self-sacrificing members of a profession having for its motto "Charity," travel thousands of miles, at great personal expense, for the sole purpose of interchanging thought, disseminating information, and for intercommunion of friendship. All are enthused with one impulse—an ardent desire to find out how disease can be controlled, pain relieved, and life with health sustained.

We will devote our days of reunion to the study of the Human Eye, in medicine a world within itself, and, so far, too vast to be thoroughly known, notwithstanding the exclusive and devoted study of numbers of the most original and brilliant investigators. There are many things in relation to eye diseases which we would most gladly know, and which we expect eventually to fathom. The presence here of so many dis-

Digitized by Google

tinguished masters in ophthalmology sustains us in the belief that our present deliberations will have their weight in the much desired advancement of our branch of the medical profession.

Our programme shows that there will be no lack of material for discussion, and the well-known names of the writers of the articles are a guarantee that the subjects will be effectively presented. There is no disease of the eye so thoroughly mastered that further information is not desired. Operations that are done daily, and with individual experience that extends into the thousands, are far from perfect. The fact that each one has his own method indicates instability and doubt. Eye surgery, although the most advanced of all the departments of medicine, because the longest studied as the pioneer of the specialities, is still full of obscurities. The most frequent of all eye operations, that for strabismus, has not yet fixed rules for its performance. When to operate and how to operate so as to insure retained and restored vision is not yet determined.

So is it also in cataract operations. The desire to see again is so paramount with the blind that the ophthalmic surgeon cannot remain content until he can show 100 per cent. of successes, after painless operations. The why and the wherefore of that terrible disease, glaucoma, is by no means clearly defined. While much has been discovered, a great deal more remains concealed.

Microbes in the development of eye inflammations is a new study, already startling in its revelations and full of promise in the very near future.

Errors of refraction in itself is so large a field for investigation that it threatens to subdivide the practice of eye surgery. We are not surprised to find, when we study the rich nerve supply belonging to the eye, and recognize the intimate relations of distant parts of the animal economy through the reflexes, that irregularities in eye construction and the effort induced in seeing clearly, cause disturbances in neighboring organs. That the eyes should pain when the vision is faulty is readily understood, but the general practitioner does not readily grasp the fact that from eye irregularities should proceed a class of symptoms often mistaken by them for cerebral, gastric, hepatic, spinal or neuralgic diseases. When, after the most varied and continued treatment, no permanent benefit has come to the patient, the adjustment of glasses for waning sight causes all of these bodily troubles to disappear as if by magic. So mysterious are these intercommunications and nerve connections, that some investigators believe that their observations will sustain them in avowing that many cases of epilepsy and chorea, usually considered as independent diseases, are of ocular origin.

These and many other interesting subjects appear on the programme for discussion, and will receive careful attention.

It is very important that a good record of our proceedings should be preserved for the *Transactions*. To insure this, it is very much to be desired that those who favor the Section with their opinions should put the same in writing while still fresh in memory, and present a copy to the secretaries before the close of the day's work. This course, if universally adopted, will facilitate very much the accuracy of the minutes.

I enter upon the serious duty of presiding over this distinguished body with many misgivings. Knowing too well my deficiencies, I rely with full confidence on your kindness and indulgence.

Once more I bid you welcome, as friends and co-workers in the Section of Ophthalmology.

Digitized by Google

EYE TROUBLES IN THEIR RELATION TO OCCIPITAL DISEASE.

LES AFFECTIONS DE L'OEIL DANS LEUR RAPPORT AVEC LA MALADIE OCCIPITALE.

AUGENLEIDEN IN IHREN BEZIEHUNGEN ZU HINTERHAUPTKRANKHEITEN.

BY DR. MOOREN,
Düsseldorf.

In concordance with the results of clinical observation, physiology places to-day the centre of vision in the cortex of the occipital lobes. The first impulse to this localization was given by an observation of Huguenin, in which the section demonstrated that a case of optic atrophy with amaurosis on the left eye, existing from the earliest childhood, was dependent on a defect in the cortex of the occipital lobes. The greatest extent of this occipital disturbance existed on the right side, on the left it was only visible in a slight degree. This fact became fruitful for physiological experiment as well as for clinical diagnosis. When Munk made the extirpation of the vision sphere in the occipital lobes on a dog, the animal was blind in both eyes, but the functions of the other senses remained intact. The voluntary and involuntary movements were only disturbed as far as they were dependent on the defective part in the perception of sight. The reflexes of pupillary activity remained entirely unaltered. This fact demonstrates that the light perception of the retina is transmitted by the optic nerve to the anterior pair of the corpora quadrigemina as perfectly as before the excision, exciting in this reflex centre the specific energy of the oculo-motorius upon pupillary movements. The subjective sensation has completely ceased, every perception of vision is extinguished—evidence enough that both of these qualities are bound to the integrity of the occipital lobes, and that consequently, in these parts, which are the terminal radiations of the optic nerve, the perceiving centre for light and vision is to be searched for.

The total extirpation of one occipital lobe, which Munk executed at repeated times in the course of his experiments, always caused a complete binocular hemianopsia, opposite to the side on which the occipital lobe was taken away.

The experiments of Munk are to be regarded in a certain sense as classical; they control the clinical observations made by Hirschberg, Pooley, Pflüger, Hughlings Jackson, Gowers and others. These cases, generally known in the literature and fully approved by the section following it, are more than sufficiently familiar to you. A greater number of cases are observed by the oculists of the old and new world. They are either not published or have not such a power of argument as those above mentioned, because the consecutive confirmation of the diagnosis by the section is wanting. I, myself, had occasion to see in my practice forty-two different cases, fourteen with right-sided, nineteen with left-sided hemianopsia; in four cases there existed temporal, and in five nasal hemianopic restriction of the vision field. My cases were dependent on the same causes as were found by other observers—at times hemorrhages, at times traumatic influences; again, syphilis of the brain, then a beginning tumor, etc. The time is too limited to dwell on the cases complicated with a participation of the intercalar ganglions. I will only observe that hemianopsia depending on occipital lobe affections is never complicated with mydriasis or capillary apoplexy at the entrance of the optic nerve, as we find in truncus diseases: on the contrary, corresponding to the integrity of the anterior pair of the corpora quadrigemina, the pupillary movements are always intact, but the hemianopic restriction of the field of vision exists in the one case without any further complication, in other cases it is combined with reduction of the acuteness of vision, now with absent color perception, then, again, with hemianopic color defect. In one word, the triple manifestation in the quality of disturbances is to be

Digitized by Google

found in variable combinations and in variable intensity. It is the merit of Willbrand to have collected the different observations made in this sense. He formulates the results of his investigations thus: that the color centre is to be placed on the most exterior cortex of the occipital lobes. Beneath it we have the centre for the acuteness of vision—the centre for the sense of space, as it is called by Willbrand—and in the third layer exists, next to Gratiolet's visual radiations, the light centre, that is to say, the centre for the visual field. When we consider that the physiological destruction of each of these three different qualities depends on three different cortical strata, of which the one is superposed on the other, then we must understand that with the destruction of the lowest layer—the light centre—there must be lost at the same time the sight and the color perception. The destruction of the lower centre involves the impossibility for its conducting fibres to pass over to the two higher centres. On the other hand, it may happen that in a certain case there exists only a hemianopic restriction for the color perception, without any alteration in the acuteness of vision or in the extension of the visual field, because both centres reserved to the latter qualities had remained intact. The reality of such cases is demonstrated by the observations of Bjernum and Samelsohn. A lateral restriction in the acuteness of vision does not occur because the narrow relation between the qualities of the color and space sense makes it necessary that the reduction in the acuteness of vision is always proportional to the impaired or eliminated color perception.

Hemianopic restriction more or less approaching the point of fixation—now vertical, now slightly sloping—cannot alone be attributed to the individual variations in the decussation of the optic nerves; the seat and the extent of the occipital disease influences equally its configuration. The island-shaped defects, although homonymous regarding its position, but varying sometimes regarding its reciprocal size, and also the fact that in single cases the hemianopsia disappears completely, demonstrate this influence clearly enough. I, at least, observed a case in which the enormously impaired sight returned slowly in the course of a year, with absolute restitution in the originally pronounced hemianopic restriction of the field of vision, and in a second case, the result of apoplexy, there remained nothing except a small sector-like defect.

Generally speaking, we must admit that such a restitution is exceptional, also in the cases in which the acuteness of vision is only slightly reduced. With the ominous symptoms of hemianopsia begins, commonly, the prostration of physical and moral energy; the memory becomes daily weaker, and the sudden changes in spirits, mostly of a depressing character, are for the surrounding of the patient a great burden. Two or three years will pass before the final catastrophe takes place. I have seen only one case in which the intellectual power was satisfactory for three years; then the patient succumbed to a new apoplexy.

All these disturbances have the stamp of chronicity. Notwithstanding, there are found neuropathic individuals who, under the symptoms of scotoma scintillans with serious attacks of hemicrania, pretend to see, from time to time, the objects half cut. As often as I had afterward occasion to examine the field of vision, I never found objectively in the patients an anomaly of this sort.

On the other side, Munk contends that a partial extirpation of the occipital cortex gives origin to a disturbance of sight that appears under the form of an extended central scotoma. I have seen several cases which I believe must be interpreted in the light of Munk's proposition. Already, in 1866, I could, on publishing my ophthalmiatric observations, point to cases in which the central defect seemed to me dependent on cerebral alterations. Although in the first moment the patients did not present any pathological change in the interior of the eye, a nerve atrophy took place very soon, or the patients perished by general paralysis, or a consecutive apoplexy. The form of the defect had the shape of a slightly compressed ellipse, occupying either the centre or

Digitized by Google

lying with its vertex in the point of fixation. The horizontal extent of this defect amounts generally to one inch and a half or more.

Although in the extent of the alleged defects the concomitant inflammatory symptoms are of a high influence, it is evident that their real size must be based upon the anatomical substratum in the occipital lobe. The occipital limits of pathological changes were lately determined by Nothnagel with great precision, by the method of the smaller focus. After the observations of Haab, Huguenin, Feré, Seguin, this neurologist places the centre of optic perception in the cortex of the cuneus and the first occipital convolution. This localization finds its exact confirmation by a section made by Curschmann. By a fresh embolus in the three branches of the occipital artery of the brain—which, up to the moment of the disturbance, had been entirely intact—there was created a focus of softening in the cortex of the occipital lobe, especially in the cuneus. Hemiparesis and hemianæsthesia being totally absent, the patient did not present any other symptom except a left-sided hemianopsia of both eyes. From this fact Nothnagel concludes that only the first occipital convolution must be regarded as the seat and cause of hemianoptic disturbances. In the diffusely affected second and third convolutions—that is to say, in the lobulus lingualis and fusiformis—he places the cause of psychological blindness, since in his opinion there lies the field of optical memory, which never is connected with hemianopic disturbances. Many years ago, when I did not know, as I now do, that in the optical lobes is to be found the centre of vision, I saw a patient become suddenly incapable to read print that he could formerly read very well, although the acuteness of his vision was not altered, and the interior of his eyes did not present any pathological change. A second had typhus fever, which created disturbances in the parietal and occipital lobes, whose principal symptoms were manifested in heaviness of the legs, disturbed color perception and reduced optical memory. The patient died of bulbar paralysis two years after his first presentation.

Based upon these rare observations I do not like to pronounce an opinion in regard to the anatomical substratum of psychological blindness. I might point to the fact that the occipital lobes can be involved by diffusely spread alterations, and, without being disturbed, give origin to complex symptoms that differ entirely from the above mentioned disturbances. I remember a patient who, convalescing from typhus fever, complained exclusively of intense pains in the region of the skull which corresponded to the anatomical situation of the occipital lobes; both optic nerves were atrophied to such a degree that it was not possible for the young man to read the largest number of Jaeger's scale, but the field of vision had assumed in both eyes a narrow, spindle-shaped configuration. At that time I did not appreciate sufficiently the importance of the color perception, which would have contributed undoubtedly to establish an accurate diagnosis. But another case adds more proof to this. In the spring of 1868 a boy of nine years presented himself to me, with double-sighted neuritis optica in the beginning stage of atrophy. The field of vision was reduced on the right side to a diameter of eight, and on the left side to two inches. On this side it was only possible to spell words in No. 17. With the right eye No. 8 was read with difficulty. A heavy fall on the occiput had occurred in the autumn of the year before, and from this moment the faculty of perceiving colors was completely extinguished. I could scarcely hope to obtain a result in point of amelioration of sight; the terrible headache of the child induced me to undertake a treatment consisting of the continued use of Iod. potassium and in the superficial application of a thin seton at the neck. On the eleventh of May the pains in the occiput had already ceased, and, to my great astonishment, an indistinct perception of colors began to show itself; four months afterward every restriction of the field of vision disappeared in the right eye, which, at that time, could read No. 1 with ease. With the left eye No. 13 could be read, and the field of vision had the same extent that the right one formerly presented.

Digitized by Google

The perception of colors had become perfect. The distinction between blue and lilac only was not fully marked. The suspended and afterward restored function of the color centre did not admit of any doubt in regard to the localization of the disturbance in the occipital lobes. Generally speaking, we can say that injury of the occiput and its consecutive disturbances of the sight are not uncommon. At this moment I have a young man in care, who, after an explosion, was so severely injured in the occiput that he remained unconscious for six weeks. Then the patient complained of weakness of memory, obscuration of sight and continual dull pains in the occiput, which increased more when an effort of fixing an object was made. When I saw the patient for the first time, nearly one year after the accident, the left eye was completely destroyed by the above-mentioned accident. The right eye presented only the symptoms of anæsthesia optica, with circular restriction of the field of vision, reduction of the acuteness of vision up to one-seventh; the complementary colors of green and blue were not seen. Regarding the development of the general and local symptoms I established the diagnosis of contusion of the occipital lobes. This was supported by a cicatrix found in that locality. The treatment directed in this case obtained the full result, so that all the subjective sufferings ceased and the acuteness of vision was again fully established.

In a child of eight years, whom I took in treatment four years ago, the visual disturbances were infinitely greater than in the afore-mentioned case. Intolerance of light was complicated with concentric restriction of the visual field. Characters of the largest size, on small pieces of wood, were only seen by the aid of a loup, an attempt that had to be interrupted at every moment, because a gush of tears flowed from the eyes. All methods of treatment undertaken up to the present were without any success, and the boy was to be sent, for further education, to an institution for the blind. By carefully examining the child I noticed an extended flat depression at the left occiput, that had existed since his birth. With regard to all the other symptoms I was convinced that the suffering of the child resulted from an irritation in the occipital lobes. My prognosis was, notwithstanding, extremely reserved, because it was quite impossible for me to see the interior of the eye; every attempt to illuminate the eye was accompanied by a gush of tears and the strongest contraction of the pupils. I proceeded to the systematic use of mercurial inunctions, which were always for some days interrupted when the boy presented symptoms of weakness. I had the idea that such a medication would produce a favorable effect upon the physiological filtration of the walls of the blood-vessels, and that in this matter the most favorable conditions were created to obtain a resorption of exudations, if present in the region of the occipital lobes. After ten inunctions were made, each one with the dose of one gramme and a half, the child took a warm bath. This treatment, combined with the daily application of ice to the head during an hour and a half before going to bed, and the internal use of tinctura ferri and solutio Fowleri had, after three months, produced such an effect that the intolerance of light had disappeared, and a regular school instruction could now begin, because the little patient was able to see the smallest print. The application of the ice and the internal medication were continued for the whole winter, and in the next summer the same treatment was repeated, as the patient had a tendency to become fatigued by prolonged work. After this a condition of the head and eye was established which in every sense might be called a normal one. Owing to the circumstance that the disturbance had taken its origin from the earliest childhood, the above successful treatment was repeated for the third time, in order to consolidate the excellent result, and twenty-five inunctions finished it.

It will not be necessary to devote any more time to these clinical observations, as they demonstrate clearly enough the dependence on diffusely spread disturbances of the occipital lobes, and, on the other hand, show the difference of symptoms that result

Digitized by Google

from a real destruction of any part of the occipital cortex. The diffuse form of the affection does not present any complexity of specific symptoms. As we have already observed, the disturbances of sight manifested under such circumstances appears with a certain predilection, under the form of anæsthesia optica. With the pain in the occiput, which in the one case is more, in the other less pronounced, is mostly combined a rapid failing of the sight, sometimes the existence of subjective sensations of light or a high degree of retinal hyperæsthesia. In the one series of cases the ophthalmoscope does not reveal any changes in the interior of the eye, in other cases there are to be seen the symptoms of slight atrophy, and we can only exceptionally demonstrate the existence of a pronounced neuritis optica.

After the experiments made by Prof. Adamkiewicz to determine the function of the different factors concurring in the compression of the brain, we may conclude that the symptoms of hyperæmia or inflammation only take place at the entrance of the optic nerve, when the pathological changes in the occipital lobes are sufficiently pronounced to excite a proliferation of the blood vessels. The presence of a tumor is not necessary to produce this result in the substance of the optic nerve; on the contrary, a state of optic atrophy will principally arise where proliferation of vessels is absent and the development of connective tissue predominates.

I beg you, gentlemen, to consider my remarks simply as a short sketch intended to call your attention to a subject of high practical value. You know, perhaps better than I can tell you, that a rational treatment in every form of disease will only be possible after the diagnosis is well established.

DISCUSSION.

Dr. GRADLE, of Chicago, alluded to a case of a child seventeen months old who had symptoms of scarlatina, opisthotonos and greatly impaired vision, soon afterward becoming totally blind. He prescribed iodide of potash, and complete recovery followed, with restoration to sight.

Dr. DICKINSON, of St. Louis, asked which theory of the decussation of the optic nerve fibres Dr. Mooren adopted, total or partial.

Dr. MOOREN replied by drawing a diagram showing the distribution of the nerve fibres from the right side to the outer half of the right retina and the inner or nasal half of the left, and so with the other side.

Dr. MARMION, of Washington, reported a case of a lady thirty-five years old, who had experienced gradual impairment of vision, accompanied with diplopia. He gave her mercury and iodide of potash. Ataxic symptoms showed themselves and she expired very soon afterward. At the autopsy, a large gumma of the right inferior lobe of the cerebellum was found. The color sense in this case was unimpaired.

Dr. BERMANN, of Washington, reported a case of *hemiopia* caused by caffein poisoning. He thought that the caffein acted here in producing anæmia of the occipital lobes of the cerebrum, finally producing atrophy.

Dr. J. J. CHISOLM presented to the Section for examination a man aged thirty-seven as illustrative of the paper just read. This patient exhibited a large swelling on the left side of the skull over and behind the ear. The swelling, somewhat globular in form, was nearly three inches across. It was elastic in feeling and seemed to be set in a cup of bone, developed in the posterior half of the parietal bone. All this side of the head was much enlarged, the asymmetry of the two sides of the head being conspicuous. The disease is now of two years' duration. In its incipiency there was much headache and a constant throbbing, not only very annoying to

Digitized by Google

himself but very audible to others. At the end of six months, when the swelling became prominent on the head, the noisy pulsation ceased. None can be heard now. The vision of the right eye commenced to fail about this time. When he first came under Dr. Chisolm's observation there was still light perception in the right eye; the left eye had $\frac{15}{20}$ vision. In each eye the ophthalmoscopic examination showed neuritis; outline of disc irregular; surface white; retinal vessels of full size, blurred at points, from the intraocular effusion. Although vision was good in one eye and nearly lost in the other, the ophthalmoscopic picture of the two optic discs were nearly identical. This examination was made seven months since. At this time he has lost light perception in each eye, and also hearing in the left ear, the one on the side of the tumor. His general health is good in every other respect. He has taken large doses of iodide of potassium and mercury for a long time, with no effect on the tumor. The cranial sutures on the left side of the head seem opened and are easily traced. The veins of the scalp of the left side of the head are very large and prominent, showing some interference with the deep venous circulation. He suffers no pain now, nor has he had nausea for a long time. According to the rule in such localized tumors, the corresponding ear and the opposite eye were the first to fail.

PATHOLOGICAL ALTERATIONS OF THE RETINAL VESSELS.

ALTÉRATIONS PATHOLOGIQUES DES VAISSEAUX RÉTINAUX.

PATHOLOGISCHE VERÄNDERUNGEN DER NETZHAUTGEFÄSSE.

BY DR. OLE BULL.

I have long been aware that alterations of the retinal vessels play a greater part as effective causes of retinal diseases than is generally believed. This opinion I have sought to maintain in my treatise, "The Ophthalmoscope and Lues," in which I have demonstrated that various symptoms can be explained only by supposing that pathological changes actually have taken place in the retinal vessels.

Here, however, I will only deal with such affections as are characterized by the suddenness of their appearance and in which no other ophthalmoscopical changes are seen than those of a disturbed circulation. Hitherto, such cases have been attributed solely to the existence of emboli.

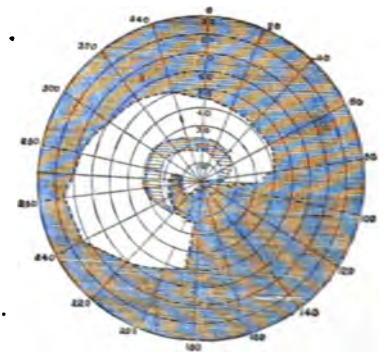
Affections of this kind are by no means infrequent. In my own practice I have, among 6500 patients, seen 18 cases, and among those one in which both eyes were attacked at different times. This gives a proportion of about three *per mille*. In eight of these cases referred to all of the retinal vessels were more or less obstructed, while in nine cases only some of them. I am, however, satisfied that the above-named proportion does not represent the true average frequency of such cases. Many cases of partial obstruction of the retinal vessels doubtless remain unobserved by the patients themselves, and many others pass unrecognized by the physician. In the two following cases the patients were at the time of examination wholly unconscious of their eye affections, although, as can be seen by the drawings, the vessels obstructed were of a considerable size, and had occasioned rather serious functional disturbances.

Digitized by Google

CASE I.—Anna Hansen, married, age fifty-eight, came to my office November 24th, 1883. Thirteen days previously she had noticed that her left eye suddenly became obscured. By examining the right eye, in which the patient told me that she had never experienced anything wrong, I found the lower main branch of the artery contracted for the length of one diameter of the optic disc. The artery in its contracted part appears thread-like. The contraction commences about two O. D. from the margin of the disc; the branch to the region of the macula is seen to extend from the middle of the contracted part as a white string. In the left eye the under main branch of the artery was visible only as a dark streak. Two of the upper veins—vena sup. temporalis and nasalis—were seen to be much dilated for a short space. In the dilated space situated about two O. D. from the disc the veins appeared quite black. All the veins on the disc narrowed. Close to the disc several small patches of blood. Grayish discoloration in the region of the yellow spot. (*Vide* Fig. 1; all figures taken in inverted image.) V right eye = $\frac{1}{3}$; left = $\frac{1}{4}$. L = $\frac{1}{5}$ R and $\frac{1}{7}$ L. C* = $\frac{1}{3}$ and $\frac{1}{6}$. The visual field much contracted in its upper part on the right side; on the left concentrically constricted. T, both sides normal.

Heart action very irregular, the sounds reduplicated, so that the pulse cannot be counted by auscultation. Impulse much increased. She has been twice attacked by

FIG 1.



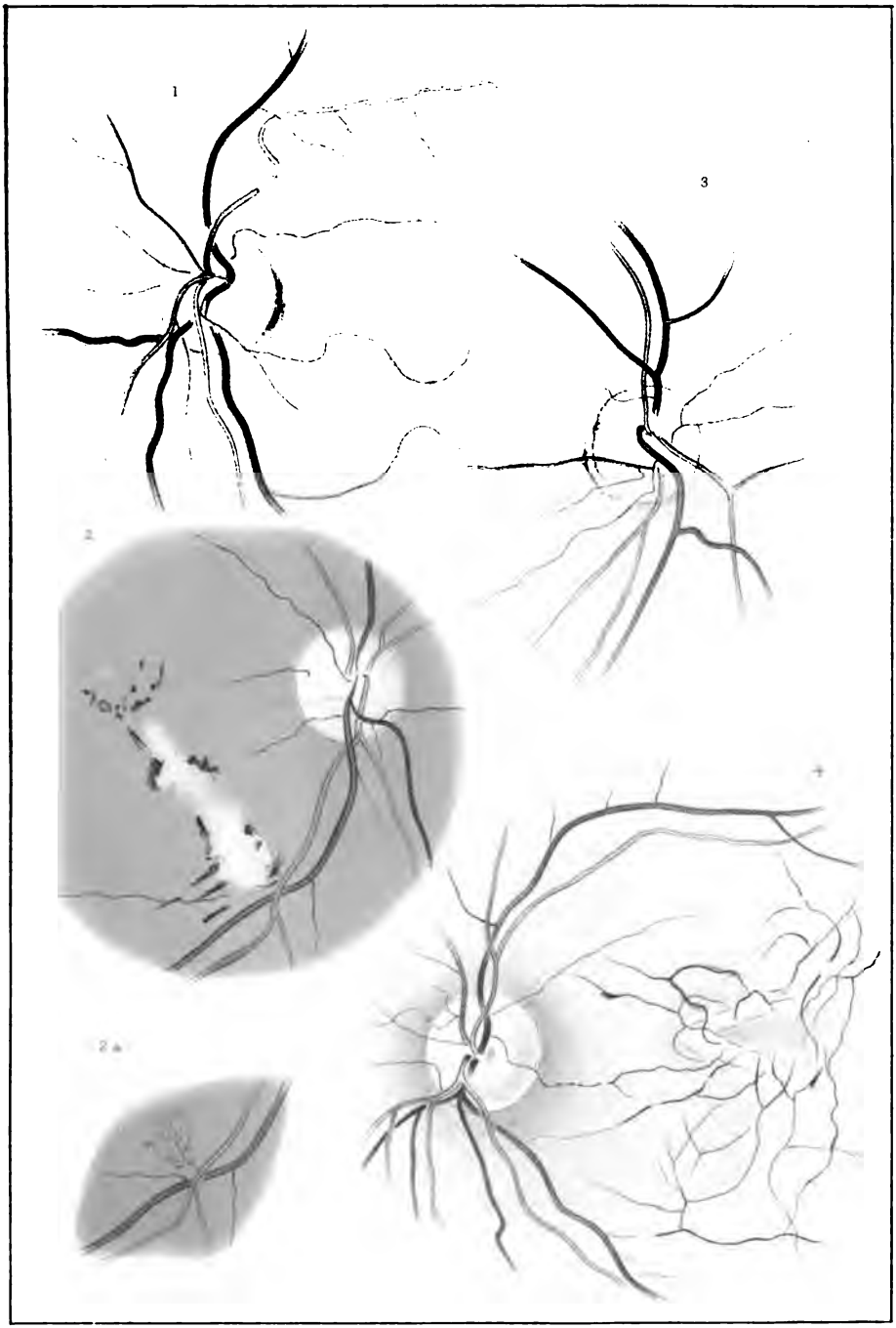
articular rheumatism. About nine months ago she was hemiplegic on the left side during eight days. A physician was not called in.

Some days later the tension of the left eye increased. The pupil became dilated, and soon all symptoms of acute glaucoma were developed. As she could not remain at home she was taken into the State Hospital, where the eye, at last, was enucleated.

CASE II.—Mr. T. M. Nass, fifty-four years, called on me 16th of June, 1885. He complained of his left eye, in which nothing can be seen but symptoms of slight irritation of the conjunctiva. By ophthalmoscopical examination considerable changes were detected in the right eye, which, according to the patient's statement, was sound. From arteria temp. superior a broad, whitish stripe is seen to extend toward the macula. It is of irregular shape, partly surrounded by extravasations of blood. Several such, lying in a half-circle, are seen above the macula, which itself appears as a bright

* L signifies the power of discerning light differences in a light room. To try this I have used either the photoptrical tables constructed by myself, or the rotating disc, with black sectors on gray disc (Pflüger). Color sense has been determined by Chromatoptrical Table. (*Vide* "Transactions of Seventh International Medical Congress." London, 1881.)

Digitized by Google



T. SINCLAIR & SON LITH. PHILA.

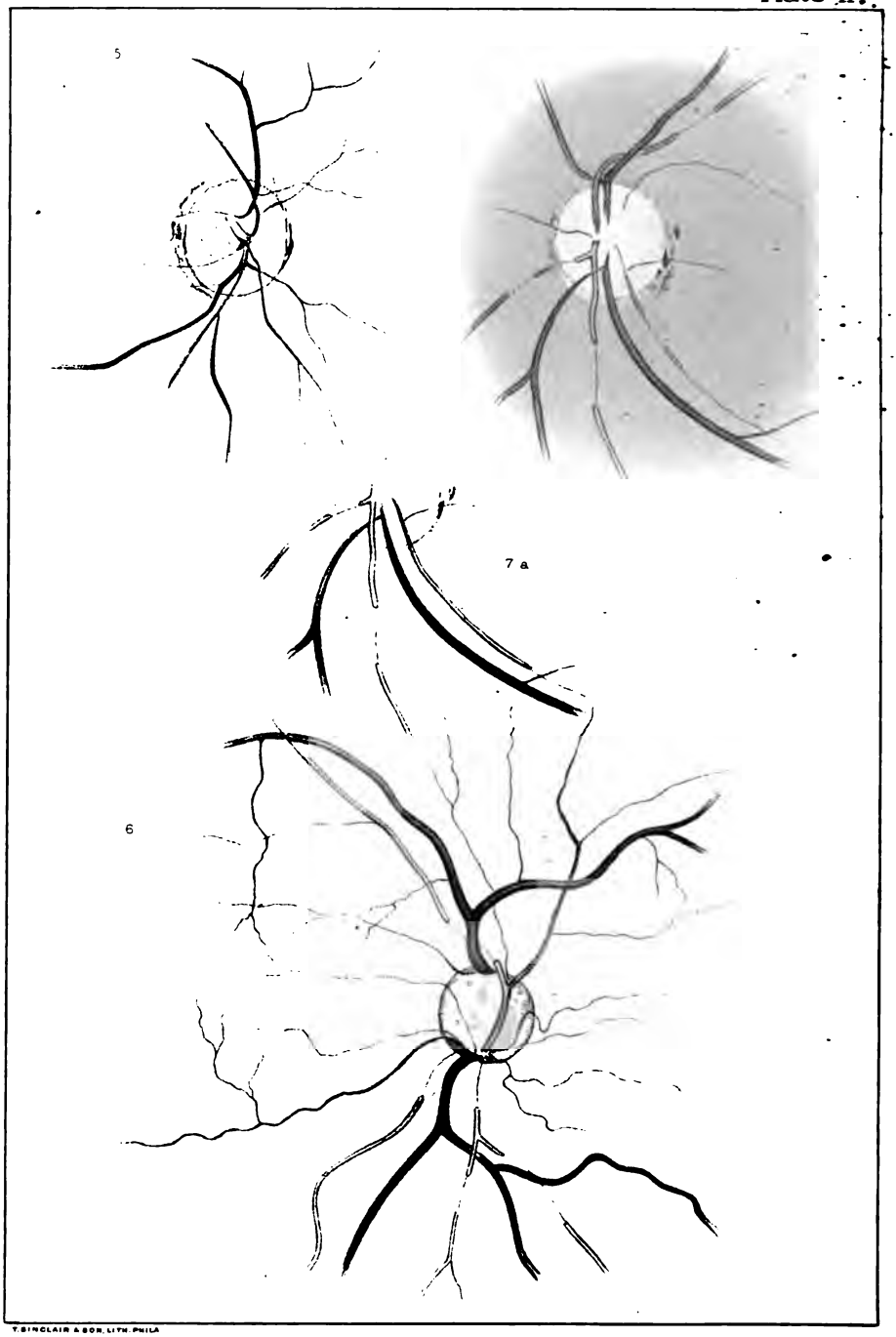
Pathological Alterations of the Retinal Vessels.

(Dr. Ole Bull.)

Digitized by Google

Digitized by Google

Digitized by Google



T. SINCLAIR & SON, LITH. PHILA.

Pathological Alterations of the Retinal Vessels.

(Dr. Ole Bull.)

Digitized by Google

pearl. When the artery has crossed the vein and is divided into two branches the temporal branch is seen to be fusiformly dilated. The part dilated is of a dark-red hue (Fig. 2). V R $\frac{4}{5}$, L $\frac{4}{10}$. L and C left $\frac{1}{14}$ and $\frac{1}{3}$. In dark room light perception but very little diminished. In the field of vision a curved pan-shaped scotoma, corresponding to the situation of the obstructed main branch.

Auscultation of the heart indicated nothing abnormal.

The patient has for some time suffered from bronchitis.

In the course of one month the whitish stripe gradually disappeared. In its place the fundus became black-punctuated and the artery going down toward the macula appeared as a hair-like streak. V increased; 28th of July it was raised to $\frac{4}{5}$. I saw the patient 21st June, this year. V then = $\frac{4}{4}$; no scotoma. The branch formerly obstructed appeared rather narrow.

By physicians many cases are undoubtedly registered under the diagnosis Idiopathic Retinitis, or, when of old standing, under Atrophy of the Disc. This has certainly happened several times to myself. By looking through my notes, made at the time when I had not become aware of the frequency of such cases, I find several cases which, had due attention been directed to them, would very probably have shown themselves

FIG. 2.

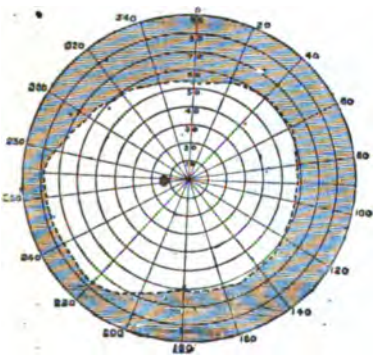
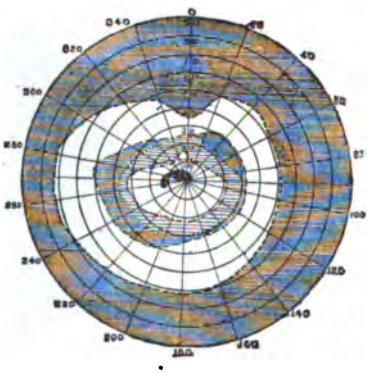


FIG 3.



to have resulted from obstruction of the retinal arteries. In the two cases about to be mentioned I at first made the diagnosis: Atrophy of the Disc.

CASE III.—Mr. W., druggist, twenty-six years; observed August 3d, 1882. Four years previously, while occupied with writing late in the evening, the patient noticed that he saw double. On waking next morning he felt a pain in his left eye. He immediately tested the sight and found it to be considerably lessened. Optic nerve as in atrophy. V R = $\frac{4}{4}$; L $\frac{4}{15}$; C left $\frac{1}{4}$ for blue; otherwise $\frac{1}{3}$. Mapping the visual field I found a fan-shaped scotoma starting from the blind spot, thence extending to the centre and ending in a horizontal line passing through the point of fixation. As the form of the scotoma seemed to indicate affection of a retinal vessel I repeated the ophthalmoscopical examination, and found that arteriola macula inferior was wanting. (Fig. 3.) the patient told me that he always had been healthy. No heart affection; urine normal. Some years later, however, he was attacked by tabes dorsalis.

CASE IV.—Mr. R., thirty-one years; observed 4th of September, 1883. The patient asserts that the sight of his right eye had once been excellent. In 1869 he, for a while, felt a tenderness in this eye when moved. Some eight or fourteen days afterward, when the tenderness had subsided, he noticed that the sight was impaired. Optic disc on the right side bluish-white e pecially in its outer half. The arteriola macula inferior

Digitized by Google

wanting. V L $\frac{1}{1}$; R $\frac{1}{3}$; C $\frac{1}{14}$ for blue and yellow; for green and red $\frac{1}{18}$ left side. From the macula a scotoma exactly like that in Fig. 3.*

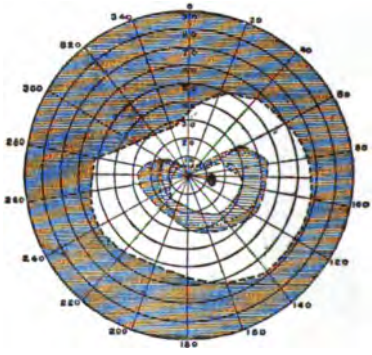
Concerning the actual cause of these affections, they have hitherto been most commonly assigned to emboli. These may sometimes be the true cause, and some of my own cases might possibly be explained in this way. Thus, the following two cases:—

CASE V.—C. Simonsen, thirty-four years; consulted me September 26th, 1869. Eight days before he suddenly became blind in his right eye. Nothing to be seen externally. T of the right eye perhaps somewhat increased. Excursions free. Ophthalmosc.: Smoky discoloration of the retina, in reg. m. l. œdem. The macula itself appears as a faint, not well-defined reddish spot. Arteries of variable volume. Both in arteries and veins blood cylinders moving slowly in right direction. In several places the blood is seen to ooze through the vessels per diapedesin.

By auscultation a loud presystolic murmur most audible at the apex. Urine normal. The patient died some months later. (Fig. 4).

CASE VI.—Helene Hansen; thirty-two years; observed 16th of September, 1882. About ten years previously she had been treated at the State Hospital for heart disease. Short time before she had suddenly become blind in left eye. During the last year she

FIG. 4.



has several times noticed a passing obfuscation of the left eye. Atrophy of the disc. Retinal vessels, especially arteries, thin and short. V R = $\frac{1}{4}$; on the left perception of light in a small part outward from the centre. First sound protracted. (Fig. 5.)

It should, however, be generally known that not all symptoms of these affections can be satisfactorily explained by assuming that emboli form their anatomical substratum.

As to their *etiology*, we certainly find that many of the patients subject to these affections suffer from heart diseases, but in many others it cannot be ascertained. Thus, of 18 cases, I have been able to demonstrate the existence of heart disease in eight only. In some cases the source of the supposed embolus has been sought in the large vessels, when these have shown signs of atheromatous degeneration. (Karwat v. Marion, Null, Hoffman, Samelsohn.) There nevertheless remains a not inconsiderable number in which no disorders of the organs of circulation have been shown. In some

* In medical literature I have found no similar case reported. Hirschberg only (*Centralb. f. Prakt. Augenheilkunde* 85) reports a case having recently occurred to a man aged sixty-nine years. He thinks that similar cases of partial embolism happen more frequently than any other.

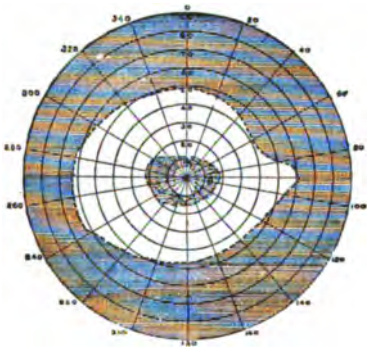
Digitized by Google

cases phlebitis in other parts of the body has been taken as the cause of the embolus. (Walter, Schülin.) On the whole, negative proof that no source exists for an embolus cannot be adduced. The more so since diseases of circulatory organs, even when present, cannot always be ascertained. It rests with those who maintain embolus to be the actual cause, to prove its source; failing to do this, their diagnosis loses one of its most important supports.

Of *Functional Disorders* there is one symptom that, in many cases, precedes the real onset of the eye affection, namely, passing obscuration of sight. Most writers have attributed these temporary attacks to emboli having for a time caused an obstruction of the central artery. That a passing obfuscation of sight can thus be caused cannot be denied, but it is in the highest degree improbable that such partial obstruction of the artery should in so many cases precede the complete obstruction, and that, as a rule, several times in the same individual. This explanation becomes futile when applied to certain cases, as, for instance, that of Loring, in which the patient had innumerable attacks of passing obfuscation in the course of several years.

The suddenness with which the symptoms occur has been correctly regarded as an argument in favor of the embolus. I have, however, seen a case in which the affection

Fig. 5.



came on gradually. The alterations of the retinal vessels were in this case quite analogous to those generally assumed to be characteristic of embolus.

Case VII.—Günild Haltan; unmarried; twenty-eight years; observed June 21st, 1884. Five years before the patient was suffering from a violent headache of the left side. When this had lasted nine or ten weeks, she suddenly got a whistling sound in the left ear, and in the course of some twelve hours she gradually became almost blind in the left eye. The sight has since remained very imperfect, and she does not hear as well with her left as with her right ear. According to her statement, the left eye has been turned inward every second day. On both sides of the neck and under the arms are traces of suppurated glands. In the heart, a tendency to reduplication of sounds. Externally, nothing abnormal in the eyes except strabismus internus. The patient was under observation three days, and the degree of strabismus was found to be greatest on the first and on the third day. Ophthalmoscope: Right eye the disc of an atrophic appearance. The upper arteria temporalis constricted for the length of ¾ O. D. The constriction commences close to the margin of the disc. Beyond the stricture the lumen is normal. The drum on both sides healthy. Eustachian tubes of good permeability. Watch in normal distance (1 meter) right side; left in 16-18″. Cranio-tympanic leading to the right.

Digitized by Google

In relation to *the changes visible by means of the ophthalmoscope*, it is well known that there are some which are difficult to explain by the supposition of embolus. Among these is a well-recognized and very frequent phenomenon, viz., that the arteries can be found filled shortly after the affection had been noticed by the patient. As Mauthner rightly remarks, it is difficult to understand how the circulation, being hindered by an embolus, can be more or less fully reëstablished within a short time. If an embolus exists we must assume it to be thrust firmly into the lumen of the artery. We cannot possibly think that an obstructing body, carried into the vessels by the stream, should simply adhere to one side of the vessel. The pressure of the current must drive it forward until the vessel becomes totally obstructed, and it will then, within a short time, become fastened to the intima by an inflammatory process. Atrophy of the vessel beyond the obstruction, and not maintenance of its lumen with reëstablishment of the circulation, would be the sure result. Some authors have endeavored to explain the reëstablishment of circulation by supposing a communication brought about either through the retino-choroidal or through the retino-ciliary vessels. The former—as Professor Leber has proved—are too small to admit a speedy reëstablishment of a collateral circulation. The latter (retino-ciliary vessels) are certainly extremely rare. It is also—as Professor Schnabel and Dr. Sachs have remarked—difficult to understand why the vessels, a collateral circulation having been established, should after a while become atrophied. Still more difficult it is to explain how the vessels can be constricted at intervals. Lastly, it seems to me inconsistent with the assumption of an embolus that one or more of the arteries can be found one day to be of normal lumen, and quite collapsed on the next. This is seen, perhaps, in most cases, and to harmonize this with the assumption of embolus the preposterous supposition would be necessary that a continuous immigration of emboli into the respective vessels took place, and that the same emboli were continuously absorbed.

Such is, in brief, a summary of the objections that may be urged against the hypothesis that emboli are the essential cause of all or most of such affections. The force of those objections has been fully appreciated by some authors, but, nevertheless, ophthalmologists continue to look upon such affections as embolic, mostly, it would seem, because the phenomena are still less explainable by the other hypotheses, such as post-ocular neuritis or bleeding in the optic nerve.

Among my own cases, the following has led me to seek an explanation in changes occurring in the vessels themselves, rather than in an immigration of emboli.

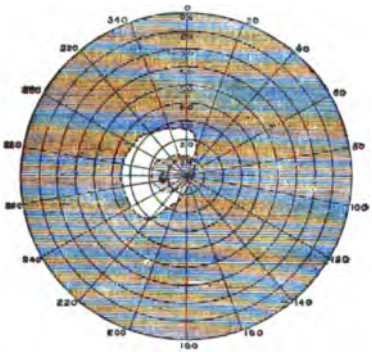
CASE VIII.—Mr. Krabbe; forty-eight years; consulted me December 6th, 1881. The patient, a strong-looking man, stated that since the autumn of 1866 he had occasionally seen colored rings around gas lights, etc., and that this has of late happened more frequently than formerly. His health has been generally good. Once, however, while suffering from a boil on the perineum, peritonitis supervened, and was followed by phlegmasia alba dolens of the left leg. By this he was confined to bed for about nine months. The phlegmasia led to gangrene of the toes, which were amputated. Heart normal. Nothing abnormal could be detected in the urine. The pupils are egg-shaped, with the smaller end turned upward and inward. Camera ant. on both eyes narrow. Sensitiveness of the cornea on the left eye less than on the right. T normal. Right eye, hyperæmia of the disc. Left, glaucomatous excavation of the disc; pulsation observed in the trunk of the artery situated on the disc. All the arteries except a. nasalis inferior are seen to be constricted, appearing as hair-like streaks at a distance of about 1 O. D. from the margin of the excavation. The two branches of a. nasalis sup., at a short distance from the point of division, become again constricted for a considerable length. Macula appears as a rather large, light spot. (Fig. 6.) V $\frac{4}{12}$ both eyes. C $\frac{1}{2}$; left, in field of vision sector-like defect downward and outward; the defect includes the blind spot. April 24th, 1882, V was a mere perception

Digitized by Google

of light left; right $\frac{4}{12}$. The left pupil much enlarged, its egg shape persisted. Later an attack of acute exacerbation of the glaucomatous process supervened in right eye. This was operated on in the State Hospital. I saw the patient 10th of August, this year. The arteries had become very much atrophied on both sides, but especially so on the left. V $\frac{4}{12}$ R. Amaurosis L.

In this case I at first made the diagnosis embolic affections of the retinal arteries, supposing the source of the emboli to be in the affection described above. The constriction of the arteries I ascribed to the pressure beyond the partially obstructed points having not been sufficient to overcome the contractility of the walls of the arteries. The peculiar condition of the art. nasal. inf. and its two branches caused me to abandon this rather hypothetical diagnosis. It is not at all likely that the weakened pressure beyond the second strictures in each of the branches of this artery should be sufficient to overcome the contractility of the walls, which, before the division, it had not been able to overcome. It is more probable that they were occasioned by spasm of the arteries themselves, and consequently the functional disturbance might be most reasonably attributed to the ischæmia brought on by the spasm. Changes in the retinal vessels are also, by recent authors, believed to play a prominent part in the glaucomatous atrophy of the

Fig. 6.



retina, and I see that Prof. Schnabel lately has supposed that the hyaline degeneration in this disease is associated with spasm of the arteries.

When I first had got the idea that the spasm of the arteries may be a factor in the glaucomatous process, it naturally led me to look out for the same cause in the cases of sudden functional disturbance for which emboli afford a by no means satisfactory explanation. Consequently I began, in all such cases, to examine the vessels very carefully for constrictions, which, especially when they take place beyond the limits of the disc, are apt to elude observation. The result has been that since that time I have seen five cases in which the arteries were partially constricted at intervals. Two of these I have already mentioned. In Case I the constriction was perhaps caused by embolus. The other case, VII, is better explained by supposing a primary suffering of the vessels themselves. So, too, according to my opinion, the following three cases :—

Case IX.—Mrs. Myhre; fifty years; came to me September 8th, 1885. Fourteen days before, after having suffered from violent headache for one week, she noticed that a dimness overspread one side of the room. The dimness has since that time been continuous and increasing. Ophthalmosc.: R, symptoms of choked disc; arteries narrow, veins comparatively wide. A minute examination was not made at once, as I contented myself with the diagnosis neuritis. V sees fingers, but cannot count them. Left eye

Digitized by Google

has always been weak. V = $\frac{4}{15}$ H m. 3 D. Tested by colored glasses, she perceives the different hues. C left without glasses, normal. Heart, the second sound a little accentuated. Urine normal. During the following days the upper lids became œdematous, especially on the right side, where the lid appeared more or less swollen all the time she remained under observation. September 24th. V begins to increase. She can count fingers at 1′. Optic nerve pale, arteries narrow. October 8th. All arteries visible are constricted. On both of the main branches the constriction commences circ. 1 O. D. from the disc and is about half O. D. in length. A. mac. lut. is apparently of its normal lumen for a short distance ($\frac{1}{4}$ O. D.); the rest is constricted to a fine streak. A. m. l. inf. is contracted for its whole length; so too art. nas. inf. On nas. sup. we see two constrictions separated by a short piece of normal lumen (Fig. 7). V = $\frac{1}{50}$. T normal. October 22d. Art. tem. sup. is now of its normal lumen from its exitus, about 2 O. D. Puncture of the cornea was now done, but did not produce any visible alteration in the fundus. Light perception in dark room much lowered. November 23d. Ophthalmosc. as before, V $\frac{4}{50}$. Visual field is indicated below.

CASE X.—Mrs. Anna Hansen; sixty-three years; observed October 15th, 1885. Fourteen years ago the patient had articular rheumatism. Over the heart there is now a slight systolic murmur audible in carotid. During the last year she has three or four times noticed a transient obfuscation of the left eye. Eight days ago she suddenly became blind in that eye. Disc very pale; all the arteries, except the lower main branch, contracted, some of them visible only as hair-like streaks. On nasal. inf. a well-defined constriction of about $\frac{1}{4}$ O. D. is seen close to the disc. On temp. sup. two constrictions are seen, separated by a piece of about $\frac{1}{4}$ O. D. in length. In the lower part of the retina, between Y. S. and the disc, a not well-defined red discolored part. Macula itself appears as a light yellow spot surrounded by a red halo. Around the latter, retina grayish-blue discolored (Fig. 8). T of left eye a little increased. V perception of light and colors in a part temporally to the centre. V R $\frac{4}{5}$. Later the disc assumed a more dead color. The transition of the constricted parts of the arteries into the more dilated ones became more fusiform.

CASE XI.—Elise Hougsleien; fifty-nine; observed 27th January, 1886. Four weeks previously the patient had received a violent blow on the left cheek, where blue and yellow discolored spots still remain. Two days ago it appeared to her as if "downs floated before her eyes." Since that time she has been blind in the right eye. The pupil on the right side appears a little wider than on the left. Ophthalmsc.: Over the fundus a slight veil. Some of the arteries hardly visible. Where the arteria. nas. sup. crosses the boundary of the disc it is seen to be constricted for the length of 1 O. D., and then again to assume its former volume. In the region of the macula an oval red spot, with a bright yellow dot in the middle. T normal. V $\frac{4}{5}$ L. R fingers in a little place outward to the centre. With this eye she can see the different hues of colored glasses. Heart normal. The following day the cornea was perforated, and, according to the patient's statement, this operation was followed by some clearing up of the visual field. The artery going from the lower main branch outward appears on the disc after the operation, to be filled for a short distance, then to be contracted as far as to the margin of the disc. Thence the two branches into which it has been divided are seen filled rather far out in the periphery. The puncture was repeated next day, but uselessly. The ophthalmoscopic appearance remained unaltered for several days. When I last saw her, March 20th, 1886, the disc had become very pale.

One of these three patients suffered from heart disease, and consequently we have in this a possible source of emboli; but if we would explain the case as an embolic affection, we should have to assume that an incomplete obstruction of the central artery had taken place three or four times in the same year. In cases IX and XI no disease of the organs of circulation could be shown.

Digitized by Google

Reviewing the whole, it seems to me that in affections of this kind no symptom exists which cannot be readily explained by assuming spasm in the arteries as the original cause of the disturbed circulation, namely, the passing obfuscations of visual field, admitted by all authors to be so difficult to explain in consonance with the assumption of emboli, the rapid reëstablishment of the circulation, the changes of the lumen of the vessels, and, above all, the local constrictions of the arteries. By supposing spasm it is also rendered easily intelligible how the constant stream may sometimes much alleviate the symptoms (Benson), or how inspiration of amyl nitrate may effect a cure (Noyes). I will here remark that spasm of the arteries has lately been mentioned by Prof. Schnabel and Dr. Sachs as a complication which possibly may play a part in the affections in question, and that Zehender, many years ago, has mentioned this cause in speaking of embolic affections.

A high degree of spasmodic contractions without mechanical obstruction can certainly produce an anæmia by which the whole or parts of the retina become, either for a time or constantly unable to perform their functions. Thus, in the case of glaucoma we find that the defect in the visual field corresponds to those parts of the retina supplied by art. temp. superior and the outer branch of nas. sup. However, as may be seen in anatomical specimens, the arteries can be actually obstructed. Judging from the ophthalmoscopic appearance, a real obstruction may have taken place in two cases in which I found a branch of the central artery to be dilated and colored a dark red. One of these cases I have already mentioned, namely, that from which Fig. 2 is taken. The second is as follows:—

Case xii.—Mr. Weidemann; thirty-three years; consulted me 29th of August, 1883. The foregoing day, while walking, he perceived a flaming light before his eyes. By testing each eye separately he found that to the right eye a large part in the centre appeared black. Ophthalmoscope: The arteries going to the nasal side considerably constricted, veins dilated. A branch of art. temp. sup. is seen to be dilated for a short distance, and of black color. Around parta vasorum a blood spot not well defined; it appears as if blood per diapedesin had oozed through the vessels. On different places blood patches; in the region of yellow spot a very large one, with serrated borders and of oblong shape. T perhaps a little diminished. V R $= \frac{4}{18}$; C and L respectively $= \frac{1}{3}$ and $\frac{4}{7}$. No constriction of the visual field. Heart normal. During the following days the extravasations of blood gradually disappeared, leaving innumerable black spots. V increased and was, when the patient left, 18th of October, $= \frac{4}{4}$. A little blood patch near the macula, situated close to a vein, was then the only visible morbid symptom.

In such cases I believe that there might have existed a mechanical obstruction, because the changes visible by the ophthalmoscope can be explained in accordance therewith. But if in these cases there has been a real obstruction, and not a simple dilatation of the vessel, I cannot believe that the body constituting the obstruction has come from a place situated without the eye itself, because I could not find any disease of the organs of circulation in these very cases. I much prefer to believe that it has been formed *in loco*. *Thrombosis*, consequently, and *not Embolus*, must be the diagnosis. Many results of anatomical investigations can as readily be explained by the assumption of a thrombosis as by that of embolus.

According to the most recent investigations of formation of thromboses (Bizossero, Hayem, Eberth and Schimmelbusch), it is more than probable that thrombi may be the result of abrupt spasmodic constrictions of the arteries.

Finally, I will remark that we may find some cases in which the real cause of the affection must be regarded to be an inflammatory process of the walls of the vessels, consequently a *Perivasculitis*. This seems to have been the fact in the following cases.

Finally, I will report a case whose diagnosis neither iritis nor thrombus would seem

Digitized by Google

to satisfy. The real cause must rather be regarded as a circumscribed perivasculitis, of whose actual cause no opinion can be had.

CASE XIII.—Miss Erichsen; aged twenty-nine; observed 22d of February, 1887. Two days previously, at noon, she suddenly perceived an obfuscation in the upper part of her field of vision. This obfuscation has remained. Externally, nothing abnormal visible. When she suddenly raises her eyes she sometimes perceives bright spots like sparks. Excursions of the eyeball free. T normal. No pain, but a sensation of dullness in the head. There is a murmur with the first sound. Close to the margin of the disc the artery, and more especially the vein running downward, are seen for a length of ½ O. D. to be enveiled by a whitish lymph. The vessels appear rather narrowed. (Fig. 9). Beyond this, fundus normal. By perimetrical examination, a large defect upward and inward. During several days no change in the ophthalmoscopic appearance. Patient seen again on 28th of June. The artery and vein had become considerably more slender, and were outlined by white streaks as far as it could be seen, which was not more than 1 O. D. beyond the margin of the disc. V and visual field as before.

Certainly in this case a systolic murmur was heard, but as the vein already the second day after the onset of the affection was found more enveiled than the artery, the diagnosis of embolus is not substantiated.

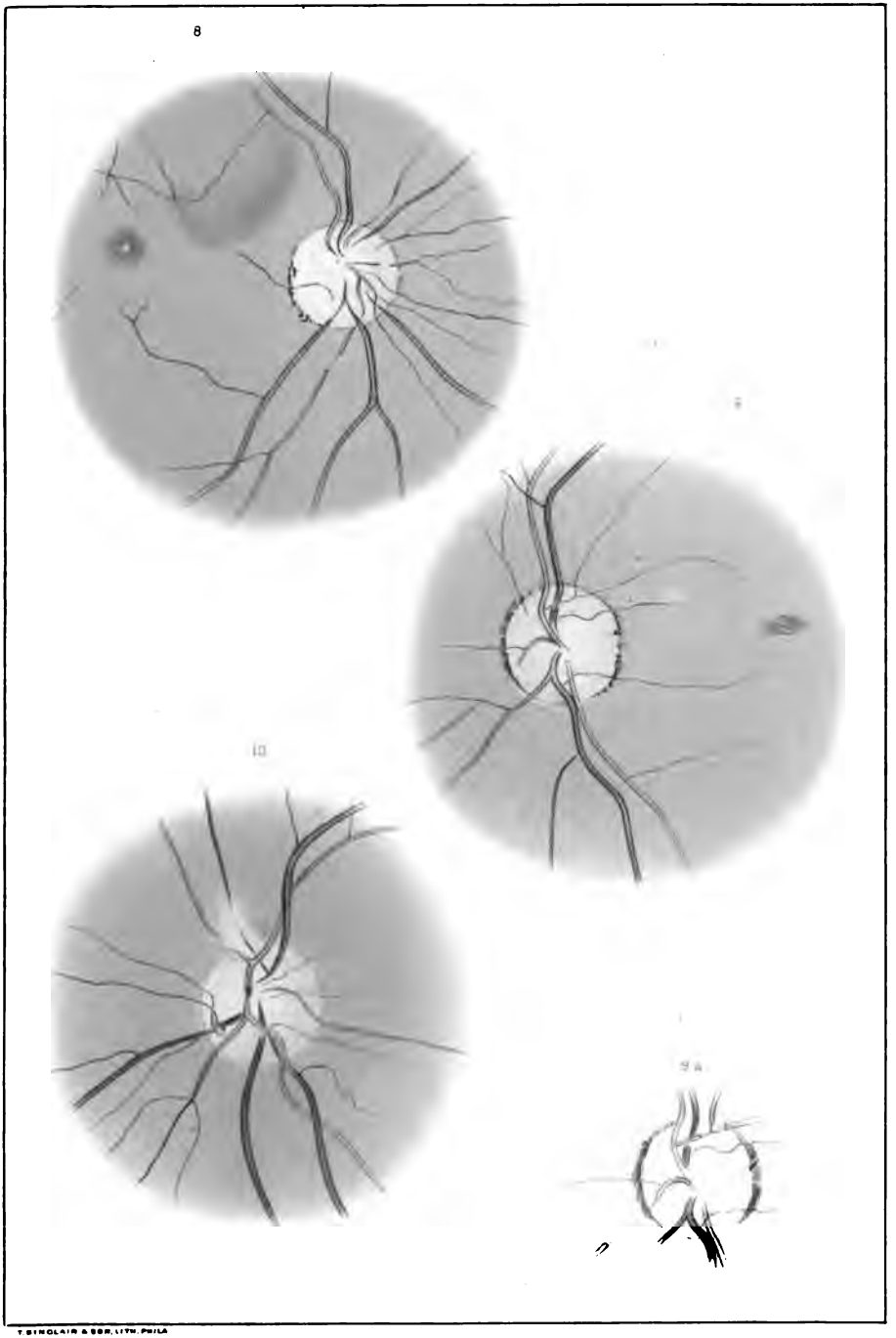
So far as my experience goes, the diagnosis in these affections must be various. I have no doubt that the actual cause in most instances is to be sought in the retinal vessels themselves. *Spasm* is probably the most frequent cause. In some solitary cases the ophthalmoscopic appearance would lead to the supposition of a *circumscribed vasculitis*. The diagnosis *embolus* should be reserved for those cases in which a source of the embolus has been ascertained, and in which it is sufficient to explain all the symptoms observed.

DISCUSSION.

Prof. P. D. KEYSER, of Philadelphia, asked if in any of the cases sight was recovered, and then reported a case of embolism in a man who recovered sufficient sight in a limited central field to see all ordinary objects, and to distinguish a man from a woman at a distance of 100 yards.

Dr. HEYL, of Philadelphia, said that many changes in the retinal vessels were not due to embolism, but to defective blood tension, due, probably, to an imperfect system of anastomosis. He mentioned the case of a young lady whose subjective symptoms were those of embolism. The skin had a peculiar pallor; nothing was of any service but local palpation or smacking the forehead, which, by its action on the supra-orbital vessels, improved the retinal circulation. He thinks, also, that glaucoma symptoms are due to some tremendous disturbance in the retinal circulation.

Digitized by Google



T. SINCLAIR & SON, LITH. PHILA

Pathological Alterations of the Retinal Vessels.

(Dr. Ole Bull)

Digitized by Google

Digitized by Google

HOT WATER IN THE MANAGEMENT OF EYE DISEASES—SOME SUGGESTIONS.

EAU CHAUDE DANS LE TRAITEMENT DES MALADIES DE L'OEIL—QUELQUES SUGGESTIONS.

HEISSES WASSER IN DER BEHANDLUNG VON AUGENKRANKHEITEN—EINIGE RATHSCHLÄGE.

BY LEARTUS CONNER, A. M., M.D.,
Of Detroit.

In the management of a morbid state in any portion of the body three things must be considered by the intelligent practitioner, viz.: first, the feeding of the parts during a continuance of the morbid state, else death or disablement may occur from simple starvation; second, the removal, in so far as possible, of the cause of the morbid state, and, third, the placing of the living matter of the part under such conditions as will most rapidly accomplish the repair of the disabled structure. The management of any disease which accomplishes these three things must be scientific and in the main satisfactory.

Eye diseases are subject to the same general laws of physiology and pathology that govern the diseases of similar tissues in the rest of the body; hence their management falls under the same general principles. Anatomical and physiological peculiarities simply modify the details of management.

All successful treatment of eye diseases is in its last analysis based upon its ability to accomplish one or more of these things. For instance, take the case of senile cataract. The morbid condition is a diseased lens (probably from starvation of the lens elements at first). However, when opaque, its management consists in its removal from the axis of vision. In doing this by extraction, care is taken that the feeding of the cornea be not shut off by too large a corneal incision, by too rough manipulation, or by an incarceration of a piece of the iris in the corneal wound. The reparative activities of the wounded parts are stimulated or assisted by the protection of the wound from all agents of inflammation, as germs, mechanical or chemical irritants, and by physiological rest to the wounded parts.

No thoughtful man will question the fact that the same principles apply to every case of eye disease. But the moment we begin to discuss the agencies by which these principles shall be applied in the treatment of any particular case or disease, divergence of opinion at once appears.

As a fact of experience, after more than ten years of careful observation and experiment, I am convinced that in the management of a large number of eye diseases, the use of hot water is a powerful agent in attaining the three things mentioned, viz.: the good feeding of the diseased tissues, the removal of morbid agents and the promotion of healthful repair. In the brief space allotted to a paper before this body, it is impossible to present in detail the clinical evidence I have collected in support of this claim. I shall only hope to so present the matter as to induce others to give hot water a fair trial. Such trial will convince thoughtful observers that hot water deserves a more prominent place in ocular therapeutics than is usually accorded to it.

In many instances it will accomplish all that is called for in the management of slighter forms of eye troubles, as mild blepharitis, mild corneitis, especially phlyctenular, mild conjunctivitis. I have known numerous cases in which, by a suggestion of one of my patients who had been taught the use of hot water, a goodly number of others had been cured of apparently similar troubles by it alone. But of these cases I do not now speak, further than to suggest, that in this manner the people have a safe and reliable substitute for quack remedies and nostrums and old wives' suggestions.

Digitized by Google

In more severe and grave affections it is used in connection with such other agencies as experience has demonstrated to possess undoubted value. Thus, the use of mydriatics and the local abstraction of blood are the ordinary means of combating iritis. To these are added, in cases of specific iritis, the constitutional remedies for syphilis, and in rheumatic iritis such agents as soda salicylate, while in all cases the general health is carefully looked after. In their place, these agencies are all indispensable, but in every case hot water will both promote the comfort of the patient, assist in dilating the pupil and shorten the course of the disease. In addition, there are some cases in which the other remedies have failed to cause any perceptible progress to recovery, that at once begin to improve as hot water is added to the treatment, and go on to a rapid recovery. I have witnessed this in so many cases seen in consultation, that I am sure it represents an important fact. The most skeptical will be convinced when he sees the diminished vascularity of the conjunctiva, the increased dilation of the pupil, feels the diminished tension that sometimes occurs when the deeper tissues of the eyeball are involved, and hears the grateful comments of the patient on the relief from pain and other discomfort.

Similar results are observed from the use of hot water in both catarrhal and purulent ophthalmia, in ulceration of the cornea, and in many intraocular troubles of great gravity. In mild forms of glaucoma it promotes the comfort of the patient until such time as an iridectomy can be performed. In cases of acute dacryocystitis it is a most important addition to other treatment and operative procedures. In all these cases it is a prominent factor in relieving the symptom of pain, but there are numerous other diseases of these same tissues in which there is little if any pain present, in which hot water is as important in promoting recovery as in those having pain as a prominent symptom. In this class are interstitial keratitis, true trachoma, corneal opacities, intraocular hemorrhage, turbid vitreous, choroidal diseases, etc.

It will thus be seen that hot water is not ranked as a specific for any particular disease, but only as an important adjuvant to the usual management of most eye diseases. Omitting personal idiosyncrasies and conditions when its use is impracticable, there are no morbid states of the eye upon which it may not exert an influence strongly in the direction of health. This claim is based upon clinical experience, physiological experiment, and well-known physiological and pathological laws.

The history of the use of hot water in treating eye diseases is a meagre one. Little has been written concerning it. As a domestic remedy it has been employed from time immemorial. As such it has generally been used in the form of a poultice, and so does constitute a hot water application in the sense that I use it. Even in the profession it has commonly been employed by means of cloths, sponges, poultices, etc. In a purely empirical manner it has found favor and disfavor during all medical history, and probably long anterior. That it did not continue in use uniformly was probably due to the fact that its mode of action had not been determined, and the means by which it was employed did not always give favorable results.

The data presented by medical history show that the divers results recorded by different observers bore a close relationship to the method they individually employed in using it. It is plain that if the method was such that the water when it reached the eye was not hot, the results of using hot water could not be obtained. Further, if sponges, cloths, or other substances were employed to convey the hot water to the eyes, we would have the effects of a mechanical irritant added to those of the hot water. Besides, as these substances speedily cool, the effects of warm rather than hot water were more likely to be obtained.

In the *American Journal of Medical Sciences*, October, 1881, I called attention to the value of hot water in producing a more or less permanent contraction of the blood vessels of the eye. At that time I had for several years been using hot water for the

Digitized by Google

definite end of producing a contraction of the blood vessels in many diseases. Since then I have continued its use for this purpose with increasing satisfaction. Step by step I learned that hot water would do more than this, and meet other important indications in managing eye diseases. Of these I shall speak presently. That there may be no misunderstanding, I will briefly explain what I mean by hot water. By observation I found that water was hot to some persons at 110° Fahr., while others would bear equally well a temperature of 115°; others 120°; others still 130° or even 140°. It was found, also, that when persistently used for long periods, frequently during the day, that the temperature which could be endured was progressively greater. Hence it became evident that the actual temperature must be made to correspond to the peculiarities of the patient. In the beginning I found it convenient to direct the patient to apply the water as hot as the end of the forefinger would bear without scalding. To quiet patients' fears respecting possible injury to the eye from the hot water, I told them that the eye would not be injured by the heat of the water, unless the skin dripped from the testing forefinger. As a fact it appeared that eyes are able generally to bear with comfort water much hotter than can be borne by the fingers.

Quite as important as the temperature of the water is the method by which it shall be applied to the eye. At first I directed the patient to sit with the head inclined over a large bowl of hot water, and with the hand gently throw the water against the eye, taking care that the hand itself did not touch the eye. This enabled the patient to apply hot water directly to the eye. But it soon became fatiguing, in cases where it was desirable to apply it for long periods at a time and at short intervals. It was also objectionable because of the liability of the water to be spilled, to the annoyance of all parties. It also was difficult to keep the water sufficiently hot.

For special cases I devised a large rubber bulb, holding a pint or more, and so arranged that the eye of the patient could be placed in the large opening at the top. By a tube at the top, hot water constantly entered, and the cooler water as constantly escaped at the bottom, stop-cocks controlling the flow, as was necessary to keep the water at any desired temperature. A thermometer was immersed in the water so that the temperature could be regulated with exactness. This apparatus gave excellent results, and was used in many experiments, as well as for therapeutic purposes. The objections to it were its expense, its not being at hand when needed, and its failure to fit perfectly every variety of face. Hence, for general practice, it could not be made available. Another method found serviceable was the construction of a clay dam on the patient's face, so that when lying flat upon the back the filling of the dam would keep the eye entirely covered with the hot water. The water was admitted and drawn off by rubber tubes, arranged in a convenient manner. A thermometer was also placed so that the temperature could be kept at a definite point, as in the preceding apparatus. In several cases of malignant gonorrheal ophthalmia this apparatus proved extremely useful, and, in my judgment, saved the patient's eyes. Still, the disadvantages of this method are insurmountable for general use. It requires too much care and intelligent watching, and so is limited to the few cases attended by proper conditions.

The last method I shall mention is free from all of these objections, and leaves little to be desired. Briefly, it consists in the application to the eye of hot water by means of a common tumbler. The glass is filled to the brim, the head slightly bent forward, and the glass so applied to the face that a dam is formed with the face below the eye and the side of the nose, so that the eye is fully immersed in the hot water. As the mass of water in the glass is considerable, the water will remain some moments at the proper temperature. As it can be renewed in a second, it is possible, with a small amount of fatigue, to keep the eye immersed in hot water by the hour, if called for. It will be apparent that the water can be made aseptic or antiseptic as may be desired in any special case. Clearly, this method meets all the requirements for universal

Digitized by Google

application, as it is inexpensive, the apparatus being found everywhere within the limits of civilization.

The use of hot water by any of the methods described is safe without the watchful care of the physician; they may not accomplish all the good possible, but they will have done no harm. The same cannot be said of other and common modes of applying moist heat to the eye. Irreparable damage often follows the application of moist heat by means of some solid substance. Among the substances employed the most common is the poultice. As a general rule, this should never be applied to a diseased eye unless under the personal observation of a physician, if it is desired to obtain the benefits of hot water. With the greatest care, it is extremely difficult to get the good effect of hot water while avoiding the evil effects of the mode of application. In unskilled hands, the most dire results are frequently witnessed. All poultices cool soon, and in such a condition they have none of the virtues of hot water, while they have the power of inducing and intensifying the very conditions which hot water tends to relieve. They dilate the blood vessels and render the circulation beneath them sluggish. Hence, if the cornea be suffering from lack of blood, they still further starve it and so tend to the destruction of corneal tissue. The poultice in any of its numerous forms is an unsafe and unreliable means of applying hot water to the eye.

In many cases the poultice mechanically irritates an eye already in an irritated condition. This would be objectionable if we were able to keep the temperature at the proper degree for a length of time.

The poultice is a dirty affair, inconsistent with the aseptic principles of modern surgery, especially when it is applied to surfaces which have lost any portion of their epithelial covering. In it may be countless morbid germs, and under it may be developed countless more poisonous elements.

The compress is another form of applying moist heat to the eye. It is less objectionable than the poultice, in that it causes less irritation mechanically, is less likely to get cool, and far less likely to become the carrier of morbific material. As a substitute for pure hot water it may occasionally be used as a matter of necessity or convenience, but the results are, generally speaking, less favorable. Singularly, those who have used hot water in this form object to the use of hot compresses in acute affections of the conjunctiva and cornea, while they loudly commend their use in chronic affections of the same tissues. Apparently, this is due to the fact that chronic diseases bring the eye into such a state of toleration that it will suffer less harm from the mechanical irritation of the hot compresses. Had these observers employed hot water in the manner suggested, they would have been quite as enthusiastic over its use in treating acute as of chronic affections of the eye.

A form of compress sometimes called for in the treatment of ophthalmia of the newborn, is made of absorbent cotton. Watched, as are similar pledgets when used to apply cold to the same class of cases, they are safe and efficient, though less so than the water alone. As they are likely to be applied by the average nurse or attendant, they are dangerous in the extreme, as promotive of suppuration rather than the reverse. Especially is this true if the cornea becomes involved in the disease.

It will thus be seen that I make a marked distinction between the effects of simple hot water applied directly to the eye and the effects when any solid substance is employed, as a poultice, compress, etc. The first I have invariably found beneficial and never harmful, while the latter often fails to do good and frequently does irreparable damage.

We are now ready to ask, what are the local effects of hot water applied to the eye?

My first proposition is that hot water causes a contraction of the blood vessels in and about the eye. The proofs of this are many.

(*a*) With the apparatus already described, I have carefully studied the effects of hot

Digitized by Google

water upon the human eye, and have always found that when applied for a sufficient length of time it bleaches the normal tissues. This can be seen in the eyelids and in the conjunctival tissues. The time required varies with different conditions and in different persons, but by regarding these it can be obtained. The longer the application is continued the longer do the effects remain when the water is removed.

(*b*) In operations upon the eyelids and external portions of the eye, as well as during the hemorrhage which sometimes complicates an iridectomy or injury to the eye, I have found that hot water most quickly and effectually controls the hemorrhage. What is still better, it stays controlled, while after using cold the hemorrhage is likely to recur speedily.

(*c*) In cases of blepharitis, conjunctivitis, in iritis, in acute dacryocystitis and other inflammatory affections of the external portions of the eye, the same results have been observed to follow so generally that I have learned to expect them with the same certainty that I do local anæsthesia from cocaine applied to the conjunctiva. If these do not follow, I know that the hot water has not been properly applied.

(*d*) With the ophthalmoscope I have examined many eyes before and after the local application of hot water for from ten to twenty minutes, and found uniformly that the retinal vessels were found reduced in size. In a subjective way I first noticed this upon myself. After some very exhausting work during an attack of indigestion, my retinal vessels became so dilated as to seriously interfere with my distinct vision. Having in mind the properties of hot water under consideration, I placed my eyes in water at a temperature of 130° F., and at the end of ten minutes the disagreeable phenomena had disappeared. Shortly after this a gentleman applied to me for relief from a similar condition. With the ophthalmoscope I ascertained the size of the retinal vessels and made a drawing of the same. Then I caused him to use hot water locally, as described. At the end of eight minutes he affirmed that his eyes were all right. An ophthalmoscopic examination showed that the vessels were reduced to their normal size, and even less. A comparison of the drawing of the vessels before and after the use of the hot water was additional evidence of the truth of the point in question. Continued clinical observation of similar cases has given me great confidence in the power of hot water to control the action of such blood vessels of the eye as retain sufficient vitality to respond to local remedies.

(*e*) Surgical, obstetrical and gynæcological practitioners all tell us that hot water contracts the blood vessels, checks hemorrhage and keeps it checked. The evidence here is abundant and conclusive.

(*f*) Dr. R. H. Murray (*Edinburgh Medical Journal*, August and September, 1886) details some very accurate studies of cold and heat upon the blood vessels of the uterus. He found that water at a temperature of from 110° to 120° F. constricts blood vessels and arrests hemorrhage from small arteries. Water at from 60° to 100° dilates small blood vessels and promotes hemorrhage. Water at from 30° to 50° checks hemorrhage by constricting blood vessels, but this only temporarily. After water at these temperatures has lost its power to contract blood vessels, water at a high temperature is still effective. From these experiments it is clear that hot water acts very promptly, that it produces a long contraction of the blood vessels, that there is an absence of vascular reaction, that there is no exhaustion following its use, and that the parts avoid all shock.

Our second proposition is, hot water will wash away or destroy or render less harmful morbific agents in and about the eye during the progress of many diseases.

Concerning the first part of this statement there can be no difference of opinion. All will grant that hot water will wash out of the conjunctival cul-de-sacs, secretions, excretions, products of inflammation, foreign substances, etc., as readily as any other liquid. Few will doubt that it will do it better even than cool or cold water. As a mechanical detergent for the eye hot water stands first.

Digitized by Google

2. Water at a temperature of from 110° to 140° will certainly check some forms of putrefaction. It matters little whether it does this by rendering less active the germ agent which produces the mischief, or by repairing its damages, or by rendering the tissues less susceptible to its ravages. The practical end is the same. We have so frequently observed the changes in the secretions of the eye under the influence of hot water that we are positive as to the result. Concerning the exact modus operandi we are not in a position to express a positive opinion. Dr. Heyl (*Archives of Ophthalmology*, September, 1886) gives reasons for believing that hot water acts beneficially in purulent ophthalmia, by placing the tissues in a condition unfavorable to the growth of the gnococcus of Neiser. The same thing is done by the application of nitrate of silver. Hence, he commends in this form of disease applications, every three hours, of a weak solution of nitrate of silver, carefully neutralized, with constant applications of hot water.

Dr. George Sternberg (*American Journal of Medical Sciences*, July, 1887) gives some experiments made to determine the degrees of heat necessary to destroy different micro-organisms. He found that a temperature of 132° F. was fatal to the bacillus of anthrax, the bacillus of typhoid fever, the bacillus of glanders, the spirillum of Asiatic cholera, the erysipelas coccus, the virus of vaccinia, of rinderpest, of sheep pox, and probably of several other infectious diseases. As the eye will endure much higher temperatures without injury, as we have demonstrated, it is clear that at least some micro-organisms may be destroyed by the use of water of such a temperature as may safely be applied to the eye. The principle being established, further observation will determine the limits of its application, and it will become a recognized factor in the management of such diseases of the external portions of the eye as are caused or maintained by microörganisms.

Our third proposition is that the local application of hot water to the eyes in the manner described promotes the healthful activity of the living protoplasm or living matter.

One function of living matter is to separate from the blood currents such elements as are required for the repair of worn-out tissues and elaborate them into tissue proper. Another scarcely less important function is to remove the broken-down or effete materials. Upon the proper performance of these two functions the integrity of any portion of the body depends. That the regulation of the blood currents is essential to such performance is self-evident. Perhaps this may explain the quickening of reparative processes observable when the eye is suffering from conjunctival or corneal inflammation. Still, I think we must look further for an adequate cause. Other remedies notably cocaine, are capable of contracting blood vessels, but they also in some manner interfere with the nutrition of the parts, so that they are harmful in purulent, corneal troubles, and of doubtful utility in other conditions.

It is well known that each portion of the body thrives best when kept at a given temperature. When it is enfeebled by disease a different and generally a warmer temperature is called for. In other cases a lower temperature is demanded, lest the parts be destroyed by the excessive heat. The temperature must be elevated or lowered as called for under such varying conditions. It would seem, from this statement of the case, that the natural application to an eye when its temperature was elevated by an acute purulent inflammation, would be cold. But I have often seen this temperature lowered nearly to the normal by the local application of hot water. When this can be done it is a safer line of practice. That it can be done in every case I cannot affirm, as my observation is limited to a few cases, but in none of these was an exception found.

We have explained this effect by saying that a better circulation through the diseased parts was effected, some of the morbid materials were removed, and the living tissue placed in such conditions that it could act more effectively in resisting the encroachments of morbific agents, and better repair damages. This is not singular as

Digitized by Google

applied to eye diseases, as it has been observed in many other organs, and to the student of general medicine may seem trite.

Our fourth proposition is "hot water has great power in relieving muscular fatigue and spasm."

Like all other muscles, those of the eye often weary after excessive use. When ocular defects exist fatigue is earlier and more marked. For the relief of this distressing condition, I know nothing so efficient as hot water. In the researches of Dr. Murray, already referred to, he gives some exact studies of the uterine muscle as acted upon by hot water. He found that the application of water at a temperature of from 110° to 120° F. caused the muscle to contract almost instantly. The relaxation was from twelve to twenty times the duration of the contraction. Successive applications were followed at once by a response. The efficiency of the contraction greatly increased. The period of relaxation and maximal contraction was much increased. In four experiments there was a gain of four times the initial efficiency. Continuous application induces a high degree of contraction broken by secondary waves of partial relaxation and contraction. Thus, the applications of hot water actually increase the contractile power of the muscles.

On the other hand, he found that water at a temperature of from 32° to 60° F. caused the muscle to contract slowly, produced a relaxation three times the duration of contraction, and destroyed the power of contractility except after a period of rest. Continuous application of the cold water produced rapid exhaustion of the muscle, so that it soon failed to respond, being completely relaxed.

From these data it would seem evident that in cases where it is desired to increase the efficiency of the muscles of the eye, the use of hot water is clearly indicated, and that of cold contraindicated. It matters not how the exhaustion be induced, hot water is a most efficient agent in relieving it. Frequently, in cases of insufficiency, moderate in extent, of one or more of the recti muscles, we have seen it cease to trouble the patient after a continued use of hot water locally applied. In most cases, however, it is necessary to correct existing defects by the use of prisms, changing the insertion of the muscles, etc., the hot water affording only temporary relief. After operations for squint I always order the local application of hot water for a considerable time, in order to bring the muscles most quickly to their greatest vigor, and so enable me to ascertain the full effect of the operation. The liability to over-correction is thus materially diminished, because the full effect of the first operation is more accurately determined before the last is performed. No doubt hot water induces these effects by other means than by its direct action upon the muscles of the eye, but to the latter we now direct attention.

Admitting the propositions advanced as substantially correct, what is their practical application to the management of eye diseases? It seems to us that every thoughtful student of such cases will at once be able to designate numerous conditions in which the patient would receive great benefit from the local use of hot water.

Active and passive congestions and inflammations, both without and within the eyeball, would all be benefited by so regulating the current of blood through the eye as to enable it to approach the normal standard. It is not claimed that hot water does this in every case, but it will materially assist such other remedies as may be employed for this purpose. In the external diseases there is always some morbific agent which this use of hot water will remove. And, finally, in every case the diseased tissues need all the assistance afforded by hot water to enable them to return to a normal condition. The list of extra- and intra-ocular inflammations is a long one, and need not be enumerated here. All will be more or less benefited by the common-sense use of hot water, to the extent of obtaining its physiological and pathological effects.

Another class of cases in which the effects of hot water are very desirable are those

Digitized by Google

in which muscular strains, weaknesses and pains form a part. Of course, the causes of these muscular derangements must be ascertained and, if possible, removed. This being done, most cases require no further attention, but, meantime, the hot water adds materially to the patient's comfort and expedites the recovery. Sometimes this can be but imperfectly accomplished or not at all. Here the regular use of hot water two or three times a day, for from ten to twenty minutes at a time, more or less, according to the nature of the case, will greatly add to the patient's comfort and materially enlarge the working capacity of his eyes.

Another class of cases benefited by the local application of hot water are injuries to the eye. In such cases as admit of its use, hot water renders the patient more comfortable and materially hastens the reparative process.

Doubtless an occasional idiosyncrasy may interfere with the use of hot water in a special case, but I have seen few such cases. Almost invariably, aside from the trouble, the patients are so materially relieved by the applications that they are greatly pleased. Hence they are the more ready to endure the trouble called for by the treatment.

I desired to detail typical cases, with the actual treatment in each, as illustrative of the use of hot water in the manner described. But time forbids. In conclusion, I present the following summary of the points I have endeavored to make plain:—

1. The best effects of hot water in eye diseases can only be obtained when the water is so used that it comes into direct contact with the eye. In practice this is best done by means of a common tumbler, filled to the brim with water at the appropriate temperature, and so adjusted to the face that the eye is immersed in the water.

2. By hot water in this connection is understood water at the highest temperature the patient can endure, viz., from 105° F. to 140° F. Lower temperatures produce quite other effects than those desired.

3. The hot water must be applied long and often enough to accomplish its peculiar effects.

4. The peculiar effects of hot water are (*a*) the contraction of blood vessels both within and without the eyeball, so as to approximate a size approaching if not equal to the normal. (*b*) The removal of some of the causes of disease, if such exist on the conjunctiva or other external portions of the eye, and the rendering of other causes less harmful. (*c*) The promotion of a greater reparative activity of the normal living matter about the morbid material. (*d*) The removal of muscular irritation or spasm, and the promoting of the normal vigor of the muscular tissue.

5. Finally, hot water does its work without any shock to the nervous system, or without any loss to the actual energy existing in the eye, and without any possible harm to the eye.

6. It is the one application that has no disadvantages or drawbacks aside from the trouble that it involves.

DISCUSSION.

Dr. Thompson, of Indianapolis, begs to differ, in some particulars, from Dr. Conner, and says that in all acute affections of the anterior portion of the uveal tract and in scleritis and episcleritis he finds poultices to be better than hot water. He would not, though, use a poultice in any form of acute conjunctivitis.

Dr. Keyser, of Philadelphia, referred to Von Graefe's method of applying ice and cold in diphtheritic conjunctivitis. Of the latter disease he had only seen four cases in his entire practice. He generally employs steam directed against heavy compresses of absorbent cotton over the eye, for heat.

Dr. Power, of London, announced himself in favor of dry heat, applied in the form of chamomile flowers or hops. He heated the leaves thoroughly, put them into a small bag and applied to the closed lids.

Digitized by Google

Dr. EUGENE SMITH, of Detroit, endorsed Dr. Power's statement as to the value of dry heat. He has often used chamomile flowers and hops with great benefit. Has never seen any good come from the use of moist heat in ulcerative keratitis, and frequently much harm.

Dr. HUCKINS, of Los Angeles, Cal., agreed in the main with Dr. Conner, and said that he generally used salicylated cotton soaked in hot water and changed sufficiently often to maintain a state of comfort and freedom from pain.

Dr. BLITZ, of Minneapolis, always uses hot applications in corneal troubles, in iritis with synechiæ. He finds the pupil dilates more rapidly with than without the aid of heat. Pain is undoubtedly relieved thereby.

Dr. ABADIE, of Paris, thinks that the treatment of eye troubles with hot water has lost much of its importance. He no longer employs it in phlyctenular, croupous or purulent conjunctivitis. In croupous conjunctivitis he prefers cauterization with lemon juice, this latter rendering far better service than hot water. In corneal troubles internal medication and judicious antiseptic applications are far superior to hot applications; in infectious ulcers of the cornea iodoform powder and corrosive sublimate solutions are far more efficient than simple hot water. In iritis, accompanied with violent pain, hot applications are good, but even here injections of morphia, leeches, and above all, judicious internal treatment, *e. g.*, quinine, or salicylate of soda, are all that is necessary.

Dr. A. W. CALHOUN, of Atlanta, Ga., uses as a means of applying heat to the eye a thin rubber bag filled with water, the water being of the required temperature. He alluded to the rubber coil of Dr. Buller, of Montreal, as being an excellent mode of application of heat.

Dr. HOTZ, of Chicago.—In applying heat to the eye, it does not matter how it is done. In all affections of the anterior part of the eyeball, wherever there are signs of venous stagnation, it is beneficial. In conjunctival affections, stagnation is indicated by œdema and chemosis; in iritis by the engorgement of the vessels and the non-response of the pupil to mydriatics. Under such circumstances warmth will do good by relaxing the blood vessels, and so favoring the efflux of the blood and the absorption of serous effusions through the walls of the vessels. Hence, when we have stagnation of the blood, apply heat, and the feelings of the patient will tell us what temperature we must keep.

Digitized by Google

SECOND DAY.

MICROBES IN EYE DISEASES.

MICROBES DANS LES MALADIES D'YEUX.

MIKROBEN IN AUGENKRANKHEITEN.

BY H. POWER, ESQ.

During the last five and twenty years great attention has been paid to the nature and actions of microbes. The starting point of our exact knowledge on the subject may be dated from the period of Pasteur's researches, in which he deposited in the Bureau of the Académie des Sciences of Paris three sets of flasks containing sterilized broth which had been for a moment opened and then hermetically sealed, near the ground, in Artois, on the lower heights of the Jura Mountains and near the Mer de Glace. Out of twenty flasks opened at each of these places, eight developed organisms at Artois, three on the Jura and one near Moulauvert. These experiments showed the relative number of the organisms in the three places, and their really great number was subsequently demonstrated by the well-known researches of Miquel at the observatory of Montsouris, at Paris. Tyndall showed that air, however abundantly it might be charged with floating particles, yet when once inhaled into the lungs issued moteless, and his observation was of great importance in showing that such particles, whether dead or living, remain in the body and adhere to the walls of the respiratory passages. Then, still more recently, the careful investigations of Klein, Koch and others with pen-culture experiments, have demonstrated the absolute and relative number of the living forms in the air and their mode of increase when introduced into the body, as well as their intimate connection with many well-known forms of constitutional disease. The first note of microbes in the eye or its appendages was probably that of von Gräfe, who removed a small swelling from the canaliculus of an eye, which, on examination, was found to consist of fine filaments intimately interwoven, and was then thought to be identical with the leptothrix buccalis, though it has since, on good grounds, been differentiated from it.

After that, and in the memory of many present, a large number of ophthalmic diseases have been found to be associated with the presence of special microbes, among which I may enumerate: chalazion, ciliary blepharitis, pterygium and trachoma, the forms of conjunctivitis that are seen in connection with diphtheria, vaccinia and variola, the conjunctivitis of jequirity, herpes corneæ, pemphigus and xerosis corneæ, keratitis and kerato-iritis, rodent ulcer, septic embolia resulting from endocarditis and metastatic pyæmia, optic neuritis, sympathetic ophthalmia and others.

MICROBES OF THE EYE.

My own observations have chiefly been made upon cases of kerato-iritis, with abscess between the lamellæ of the cornea, occurring in those who were previously healthy, or, at least, not known to be suffering from disease, except that they were low and de-

Digitized by Google

pressed. Examinations were made of the material obtained from the abscess, abstracted with antiseptic precautions and carefully examined under Seitz one-twentieth oil immersion lenses. The result showed that there were numerous microbes. Some of these transferred by puncture, also with all antiseptic precautions, into the eyes of rabbits, produced, when the cornea was punctured, inflammation, from which, however, after a month, almost complete restoration of transparency occurred, the dose having been a very small one, and the animals healthy and carefully tended. When all signs of inflammation had passed away, the corneæ were removed immediately after death and carefully examined. A few isolated microbes were still to be found, but required careful searching to be discovered.

A point that has not been much dwelt upon either in regard to the etiology or to the prophylaxis and treatment of some of these affections, has recently been strongly insisted upon by Hesse, and in papers read by Dr. Percy Frankland before the Society of Arts and published in the journal of that Society. They relate to the distribution of the microörganisms of the air, and were made in a suitable and convenient spot on the roof of the Science Schools at South Kensington. These observations support those of Miquel in showing that microbes are much more common at certain seasons of the year than at others, so that, while adopting Hesse's method, four colonies only were obtained from ten litres of air in January, there were no less than 105 colonies in August, while on certain days, as, for example, on June 8th, 1886, which was a shilling day at the Great Exhibition, the number rose to 554. In contrast with this are the observations of Fischer, who has shown that in eleven out of twelve experiments made at sea, at a distance of 120 miles, the air was absolutely germ free.

Hesse and Frankland's experiments further demonstrated that living germs are remarkably heavy, and when the air is at rest or only slowly moving, rapidly sink to the bottom of the vessel. This was well shown by drawing air at the rate of a litre in two or three minutes through a glass tube three feet long lined with a layer of agar-agar, when it was found that bacterial colonies appeared abundantly a few inches from the mouth of the tube, more rarely at a foot and scarcely at all in the more remote parts, and it was noticeable that the spores of moulds were much lighter than bacteria, their colonies being found much further up the tube.

I desire to call the attention of the Section to the immense importance of these facts in the prophylaxis and treatment of ophthalmic disease of an infectious nature, and to the duty which devolves on every medical man to impress on the municipal and sanitary authorities the necessity of abolishing all underground dwellings and to the opening up of spaces in the more crowded parts of great cities, and the provision of places of recreation for the people. Having practiced for nearly forty years in London, and having been in constant charge of the same ophthalmic hospital, I can testify to the effects of the general improvement in the sanitation of that city. It is true that we were never so troubled with infectious ophthalmic diseases, such as trachoma, diphtheritic ophthalmia and erysipelatous, as I believe some of our neighbors on the continent have been, but I am satisfied that the number of cases now presenting themselves is much less numerous than was the case in my earlier days. The improvement in this respect runs parallel with the general advance in civilization, with the improved water supply, with the opening of new streets through bad localities, but, above all, by the closing of all underground cellars as places of residence.

The great weight and speedy sinking to rest of the microbes with which the air is charged when the air was allowed to rest, clearly demonstrated by Dr. Frankland's researches, is highly suggestive, and should, in my judgment, be utilized in our efforts to prevent the excursion and to cure the effects of various infectious diseases. Children suffering from trachoma should not only be segregated from others, but should be sent to the seaside or into the country, or, at least, removed from basement and lower stories

Digitized by Google

to the upper rooms of houses, and should be kept as far as possible in the open air. I need hardly point out the importance of these and similar observations upon the necessity of cleanliness in all operations on the eye. Instruments should not only be sharp but absolutely clean, and forceps should especially be attended to. Sponges should almost be disused; their place can be taken with absorbent wool.

DISCUSSION.

Dr. ABADIE, of Paris.—The question raised by our eminent colleague, Mr. Power, is so important that I will ask your permission to touch upon it still further, from the standpoint of an ophthalmic surgeon. Pathogenic microbes play a considerable rôle in our operations, and demand, in consequence, our closest attention. The instruments, our hands, all the immediate surroundings of the patient and of the surgeon, should be the objects of the greatest care on our part. The place of operation is also of great importance, as is shown by the varying successes of those who operate in private hospitals, and of those who operate in the general hospitals, of operations in the city and of those in the country; also, the advantages that one has of operating in a room specially designed for the purpose and far removed from pathogenic surroundings. And here allow me to lay great stress upon the question of disinfection. We should have aseptic conditions and not make antisepsis our law, as is the case in the general surgical clinic. If we have asepsis, we will not have consecutive suppuration, but reunion by first intention. And then, too, antiseptics, by doing the work incompletely, may bring on a violent suppurative inflammation which will end in loss of the eye. In other words, and to condense my ideas into one practical example, I never, after a cataract operation, employ means for removing pathogenic germs. And if you leave some germs, notwithstanding your antiseptic solutions, you will, in all probability, have an erysipelatous inflammation, and thereby, undoubtedly, compromise the result of the operation. This shows us, then, that the fluids we use at an operation must not be so much germ-killing in their nature, as that they should be absolutely free from pathogenic germs. Finally, if after all our care suppuration ensues, we must employ every means to arrest its development by using the most powerful germicides. Among these latter I give the first place to the galvano-cautery. Of course, in using this means suppuration is generally arrested and the cornea saved, but the pupil is hidden by the scar and an iridectomy later on is necessary. Sometimes, after a strabismus operation, some part of the operator's armament not having been thoroughly aseptic, violent suppuration is set up which can become exceedingly dangerous. Then it is necessary to powder the parts well with iodoform, and if the suppuration persists, to resort to the galvano-cautery. If, in spite of every means employed, suppuration persists, then the question of enucleation comes up. If you consult your statistics, you will find that even death sometimes supervenes as a result of the enucleation. You open a way for the pathogenic germs straight into the orbital tissue, and thence into the brain. It is certainly preferable here to eviscerate and use strong antiseptics during the operation.

Dr. MANOLESCU, of Bucharest, thought that much depended upon the knowledge one has of the surroundings, and the promptness with which we act.

Dr. HEYL, of Philadelphia.—I wish to call attention to a few points in reference to the bacterial infection of the eye. Two errors have evidently grown out of the application of principles developed by mycologists:—

1. The supposition that, because a certain method will sterilize a flask of fluid, therefore, it will do the same for an infected tissue.

Digitized by Google

2. That one single anti-parasitic will answer under all circumstances. This is shown not to be so by several clinical experiences:—

(*a*) The purulent ophthalmia due to infection with the gonococcus of Neisser. There, I believe the palpebral conjunctivæ to be the culture ground of the coccus, while the elevations of the scleral conjunctiva, which girdle the cornea and the corneal ulcerations, are due to the mechanical accumulation of the cocci.

(*b*) The diphtheritic conjunctivitis, which, in clinical course, may be allied with the purulent ophthalmia, and the culture ground be looked for in the palpebral conjunctivitis.

(*c*) A form of epithelial mycosis described by Leber in connection with hemeralopia. Here the culture ground is the sclero-corneal epithelium. All these forms demand a particular method of treatment. Heat is useful in all of them.

(*d*) Another form of mycotic infection is that of the hypopyon keratitis. Here, I believe the starting-point of the infection is an abrasion of the corneal epithelium, the culture ground being the corneal lymph. In this case the iodoform is the proper antiseptic, while it is hurtful at least in the purulent ophthalmia.

Dr. B. J. BALDWIN regrets very much that Mr. Power did not present the surgical aspects of his bacterial pathology. Bacterial infection, he believes, is peculiar to large hospitals. In 80 cataract operations he has had five failures—two from suppurative keratitis and three from insidious iritis coming on thirteen days after the operation.

Dr. LANDOLT, of Paris, thought it of the greatest importance to avoid crowded hospitals in operations. Even private houses may have in them pathogenic germs. He uses a corrosive sublimate solution at every operation, to the strength of 1 to 5000.

Dr. DUDLEY S. REYNOLDS, of Louisville, said:—The question of septic poisoning of wounds has been studied with great profit. When a person comes to my clinic requiring any sort of operation, I first require the hands and face of the patient to be thoroughly washed with soap and water, after which a solution of corrosive sublimate, one grain to the pint of water, is used for sponging the part to be operated on. If it be a case of cataract, and the patient has inspissated mucus in the lash, or any discharge whatever from the conjunctival surface, I refuse to operate until the patient is fully recovered and the eye absolutely free from abnormal secretion. If the patient is feeble, nervous and dyspeptic, I will not operate until the state of the general system is, to my mind, favorable for the support of the reparative processes necessary after an operation. Always after an operation I wash the eye thoroughly with a solution of corrosive sublimate—1 to 5000. In all operations I wash my sponges in a solution of carbolic acid, half a drachm to the ounce of water. In this solution I also wash my instruments. Just before operating, I dip my knives and scissors into the corrosive sublimate solution, and wipe them on clean lint. Practicing these precautions, aseptic conditions may be relied upon; provided, however, the surgeon does not permit an assistant to handle the instruments at the time of operating. I have now a record of 146 cataract extractions without a case of suppuration. In fact, I have not seen half a dozen suppurating wounds in three years. I should not incline to recognize an iritis coming on thirteen days after extraction of a cataract as being, in any sense, due to local infection.

Dr. P. D. KEYSER, of Philadelphia, uses a boric acid solution, and has seen great reduction in the relative frequency of suppuration after operations.

Digitized by Google

Dr. GALEZOWSKI, of Paris, has seen suppuration of cornea and iris after a chalazion operation. He sees that the patient is perfectly clean and his surroundings aseptic.

Dr. EUGENE SMITH, of Detroit, operates in the surgical amphitheatre in the presence of his class. He uses a boric acid solution, and never has any fear of suppuration.

OPERATIVE TREATMENT OF POSTERIOR SYNECHIA.

TRAITEMENT OPÉRATOIRE DE LA SYNÉCHIE POSTÉRIEURE.

OPERATIVE BEHANDLUNG DER HINTEREN SYNECHIEEN.

BY PROFESSOR PETER D. KEYSER, M. D.,

Philadelphia, Pa.

It is a well-known fact that posterior synechiæ, especially when numerous and firmly bound down, are of serious detriment to the vitality of an eye. It is true that cases exist of isolated adhesions in which the eyes are perfectly free from any irritation or serious consequences, but it will be found in these that the attachment of the iris is generally only a small point at the mere extreme pupillary edge, and from the elasticity of the texture the iris is not drawn much upon in the action of dilatation and contraction of the pupil. But when the adhesions are more numerous and extensive, the incessant dragging of the membrane during the action of the iris, under the stimulant of changes of light, as well as in accommodation, disturbs the circulation and innervation, causing inflammation to occur in the part; and which will constantly recur as long as the adhesions remain. With every attack of recurrent inflammation the adhesions are liable to spread and increase in number, thus laying the foundation for further and more serious trouble, until, eventually, the whole pupillary margin is adherent to the capsule, and thereby cutting off the communication between the anterior and posterior chambers, which is essential to the proper nutrition of the media of the eye. If these adhesions are not removed when a cause of recurrent inflammation, the irritation will be continued, and the inflammatory process gradually extend over the entire uveal tract through the iris, ciliary body and choroid, and thus spread over the entire ball, causing its total loss, with, at times, danger of sympathetic affection and loss of the other eye.

When it is found that these adhesions are so permanent as to resist the action of mydriatics or the alternate use of myotics and mydriatics, it becomes necessary to use operative procedure to free the eye from these dangerous influences. The removal of a piece of the iris, as in iridectomy, is not always desirable, especially in cases where the adhesions are isolated, solitary or numerous, and even extensive but partial, and the centre of the capsule transparent. For such cases substitutes of various methods have been proposed, and seemed to present a history of brilliant results for awhile, but, gradually, all of the methods presented have gone into disuse, not from any defect in the methods, but from the defect in the instruments recommended, endangering the capsule as well as iris in the delicate manipulations during the operation. Streatfield presented his hook spatula, then Weber, with his hook, won some success. Then, to remove the danger of rupture of the capsule, Passavant recommends tearing the adhesions loose by grasping them with a pair of iris forceps, made without teeth at the end, so as not to injure or wound the iris.

Digitized by Google

After an experience of many years with the above methods, I have found that that of Streatfield is the best and most successful with a careful and reliable operator, but the danger lying in the instrument of Streatfield is that, it being so thin, the point of the hook on the side is liable to become bent or spring out of its level plain, and thus be a cause of either rupturing the capsule or catching the iris. This occurred to me after some previously successful operations. The point got out of position very slightly, not so as to be perceived, but enough to be fatal to the operation. Weber's hook is too large, making it difficult to get behind the adhesions, and with neither of these instruments can you manipulate from the nasal side. Passavant's operation is excellent for one or two small adhesions, but not for more extensive ones or a series of them. It is too troublesome and difficult to get patients to undergo several operations, which will be necessary if the attachments are numerous. There is also danger of iritis from the pressure on the iris by the forceps. All three of these methods and instruments require too large an opening in the cornea, so that the aqueous is lost, making the operation more difficult—the desideratum being to keep the aqueous in and the anterior chamber full, so that the iris can readily retract under the action of the mydriatic after the adhesions are torn loose.

To remove these difficulties and dangers, I had, some years ago, an instrument made which answers every purpose, and with which I have had most brilliant success. It is so shaped and curved as to permit its entrance in any part of the cornea without any hindrance from the prominence of the brow, nose or cheek bone. It is also so perfectly smooth, and without any sharp points, that it will pass over the capsule and under the iris without the least danger of injury to either.

The method of operation is that recommended by Streatfield. Atropia instilled in the eye to dilate the pupil as much as possible, so as to put the adhesion on the stretch, then cocaine is instilled to benumb the eye (local anæsthesia). A very narrow flat knife or cutting needle is used to make a small opening in the cornea, which must be drawn out without losing any or very little aqueous; then introduce the instrument for corelysis, passing it gently and delicately between the iris and capsule above the point of adhesion, after which draw it down with a slight lifting motion. In this way one or more, or a broad attachment, can be loosened in one operation. I have in this manner, and with this instrument, detached adhesions covering nearly one-half of the pupillary margin. Remove the instrument from the eye with the loss of as little aqueous as possible, then instill atropia solution, gr. iv : ℥j, every ten minutes for one hour, then twice or thrice daily. A simple bandage (Liebreich's) or shade is sufficient over the eye.

DISCUSSION.

Dr. Galezowski, of Paris.—The subject that our eminent colleague has just treated is so important that I would like to speak a few words in connection with an operation fraught, as it is undoubtedly, with a certain amount of danger and with a great deal of difficulty. You all know how difficult it is to stop an acute iritis, how, notwithstanding antiphlogistic, anti-syphilitic treatment, together with mydriatics, the disease persists; and as regards iridectomy during the acute stage, I have seen my old master, Desmarres, practice it, and, indeed, I have done the same myself, but I must confess that I was often disappointed and it ended in occlusion of the pupil,

Digitized by Google

and of course the sight was compromised. For the past few years I have adopted the following treatment: when I find that the disease obstinately resists all medication I puncture the anterior chamber. By this means I usually put a stop to the inflammatory process, and frequently, with the aid of internal remedies, bring about a cure. Later on, after all inflammatory phenomena have subsided, and I convince myself that the anterior synechiæ are not too numerous, I make an iridectomy, which under these circumstances gives excellent results.

ON THE MOST SIMPLIFIED METHODS OF CATARACT OPERATIONS.

DES MÉTHODES LES PLUS SIMPLES POUR L'OPÉRATION DE LA CATARACTE.

ÜBER DIE MÖGLICHST VEREINFACHTEN METHODEN DER STAAROPERATIONEN.

BY DR. MOOREN,

Düsseldorf.

Since the beginning of my ophthalmological activity, in the year 1855, the methods of cataract operations have been subjected to many modifications. Regarding the amount of labor imposed upon the International Congress, it cannot be my intention to enter into a history or criticism of the cultivated methods, because I would have to repeat things generally known. I confine myself to giving a résumé, as short as possible, of the methods exercised in my own practice. I cannot boast of having invented any new instrument; only I dare say that my instrumental apparatus is as simple as can be imagined, founded upon 5119 cataract operations executed by my hand; between them 2800 sclerotical extractions after Graefe's method. It is very well known to you that the combination of the extraction with the iridectomy, so essential in this and other methods, was based principally on the idea of avoiding an iritis, in order to prevent the consecutive destruction of the cornea. The progress of the antisepsis has also, in this regard, produced a great revolution, because the main point of success has ceased, in so eminent a manner, to be found in the technical execution of the operation; and, therefore, the greatest stress is now laid, with justice, upon the removing of all kinds of infectious factors, whether they result from the secretion of the conjunctiva and the lachrymal ducts, or have been brought by external influences of every kind, for instance, by instruments, want of cleanliness, etc. The notions of cleanliness and disinfection must here be regarded as identical. Generally, it may be immaterial what remedy is to be used to ensure the disinfection; only, according to my experience, the use of carbolic acid should be avoided, because to its application there is always inherent an influence more or less irritant for the eye. When a cataract operation has to take place, the patient must have washed his face carefully with strong soap water, and after this a few drops of a solution of cocaine are to be put in the eye, to obtain insensibility. Immediately before operating, the everted conjunctiva of both eyelids is washed out carefully with a 3 per cent. solution of boracic acid. After this, the equally disinfected stop speculum, to keep apart the lids, is introduced. We may dispense with this instrument, if a clever assistant fulfills this requisite. Four years ago, I had occasion to operate, in this manner, at Berlin, in Professor Hirschberg's clinic, when that eminent oculist assisted me. I learned from my friend that the best disinfection for instruments

Digitized by Google

would be to place them in a basin filled with alcohol. Aside from the stop speculum, my only instruments are Graefe's knife, known by its linear shape, and a simple pair of forceps to fix the eyeball. The instruments are taken directly out of the vessel filled with alcohol. After having executed the flap extraction in former years exclusively, with upper section of the cornea, I now prefer, for the last three years, to locate the cut downward. As soon as the corneal section is finished, the same knife will be introduced again, backward, in the anterior chamber, to open the capsule with its point through the intact pupil. Hereafter the assistant turns slightly the stop speculum, and favors, in this manner, the exit of the lens, a procedure the effect of which will be augmented when the operator exercises an easy pressure, with the forceps, upon the sclera at the same time. A fixation of the eyeball with the forceps must always be abandoned when the patient begins to be restless, in order to avoid an escape of the vitreous body. For this purpose, the instrument must be without any lock. Generally, the operation is finished in one minute. The cortical masses escape under the repeated light rubbing of the lids over the cornea, after the removing of the stop speculum. Then the eye and lids are washed again with disinfected cotton, saturated with the same solution of boracic acid. Immediately afterward, the closed eyes are sprinkled with iodoform powder. Upon this, the imposed cotton layer is preserved in its position by a simple adhesive plaster, reaching from one temple to the other. I cannot emphasize enough this method of bandaging, because it prevents the patient from pressing the cornea wound if, by chance, he moves the back of his head on the cushion. After two days the eye is cleaned and the first bandage renewed. It is not advisable to continue the application of the sticking-plaster longer than four days, because there are patients whose skin is so sensitive that erythema, or, in exceptional cases, erysipelas, might take place.

The mentioned method of operation can be applied where the form of cataract requires its extraction. If the patient presents a high degree of atheromatous degeneration, it will be better to perform an iridectomy three weeks before the extraction, because I have observed seven cases in which consecutive glaucoma forced me, some weeks after the regular extraction, to perform the iridectomy, by which I had to give up the round pupil which I had obtained by the former cataract operation. If I exclude in these cases the hardness of the temporal arteries, there were not any symptoms which would have predicted the sudden development of such an occurrence. I only had the satisfaction that the alleged cases escaped, with one exception, from the final destruction of sight.

When I have to do with a one-eyed cataract patient I perform always the combined operation, except when the form of cataract gives me a priori every guarantee of a good result. I have practiced this method in many hundred cases, for many years, and have gained the conviction that there is none that can be compared with it, regarding the certitude of success. The number of losses in these cases, which often enough offered most unfavorable chances, was so small that it averaged between 6 per cent. and 6½ per cent. at a time when the disinfection was not yet known.

It is evident that the presence of cataracta accreta requires everywhere the combined operation, although after the varying circumstances the iridectomy may be executed in the moment of the extraction or some weeks before. In these cases there exist very often deposits on the anterior capsule wall, forbidding to open the capsule with the point of the knife, because the remaining roots of capsule would always be followed by the development of a compact cataracta secundaria. It will be the best to take away the deposits of the anterior capsule wall with a fine pincette, and often it happens, that under these circumstances the cataract follows with the capsule at the same time to the tractions of the pincette.

As often as the series of cases mentioned requires the combined operation, I perform

Digitized by Google

the iridectomy in the upper part of the iris, and can afterward lay the linear cut for the extraction in the intact texture of the lower part of the cornea without exposing the patient to the influence of a dazzling light.

I must now claim, gentlemen, your attention to another form of cataract that requires a different form of operating. I mean the corneal extraction, so-called in contrast to the sclerotical one, which finds its application in soft cataracts. The technical difference in the two methods consists mostly in the choice of the knife, and in the manner of opening the capsule. Also, here the same instrument will be sufficient for both indications. The manner after which I perform the operation I have not changed in thirty years. Under strict observation of the same precaution in disinfecting the field of operations and the required instruments, I fix the eye on the lower margin of the cornea with the pincette and introduce the triangular lanceolate knife from the upper corneo-scleral limit in the anterior chamber, opening with its point the capsule as soon as the instrument has passed over the pupillar ring. When I then make with the handle of the instrument a slight movement backward, the larger part of the knife rests upon the external edge of the wound as upon an hypomochlion, and causes the softened lens to glide over the blade of the instrument. It is a very extraordinary exception that during this manipulation a part of the iris protrudes, because it is returned by the back of the instrument. When, in spite of this, such an event takes place, the protruded part of the iris must be cut off, because the slightest contusion of this membrane can give origin to the development of a state of protracted irritation or inflammation. This method of operation cannot be too highly recommended. I performed it, up to the end of July, in 657 cases, in the first years of my activity, in combination with iridectomy, but now in no other way than I described. If it would be possible to eliminate the traumatic cases, there would exist scarcely any loss. I shall never change it in favor of another now known method. This procedure is everywhere practicable where the lens is of soft consistency. It may happen that the cataract, although soft, presents a considerable coherence, or involves at the limit of a more advanced age a nucleus, a circumstance which prevents the lens from escaping through the corneal wound. In this case it would be advisable to enlarge the wound by drawing the lanceolate knife from one side to the other; it is certain that now the lens glides with greater ease through the enlarged opening. In a few cases this manœuvre was not followed by the expected result, so that I was forced to give the wound a larger extension by introducing Graefe's linear knife between the cornea and iris. The necessity of this exceptional procedure happens only when a trauma has caused an iritis, that always affords to the lenticular masses an uncommonly great coherence. In such a case the simultaneous execution of an iridectomy is advisable, and it becomes absolutely necessary when a foreign body, for instance a fragment of iron, is imbedded in the lens, because we risk, without iridectomy, that the foreign body is brushed away by the iris in the very moment of extraction, and remaining in the interior of the eye, becomes the cause of a highly dangerous inflammation, that not seldom gives origin to sympathetic affections.

From the other side we meet with cataracts, which, although more or less soft, have not yet arrived at a state of entire maturity. They can be matured in a relatively short time by practicing, with Graefe's linear knife, a split in the anterior part of the lens without entering in the deeper parts. In this way the cataract ripens in a few days and can be removed by corneal extraction without any danger. The discission with the needle, in the old meaning of the word, is abandoned by me for more than twenty years, because it is a procedure uncertain in its results, and under certain circumstances insidious in its consequences. This method has never deserved the enthusiastic admiration with which it was extolled by the followers of the old school.

Graefe's linear knife is in its use not restricted for ripening soft cataracts; it is an

Digitized by Google

equally valuable instrument in every other form of cataract, to bring them to maturity, when three weeks before an iridectomy was performed. These hard forms of cataract are mostly reserved to the sclerotical extraction. As I already observed at the International Congress of Copenhagen, I performed this system of operations at any age. The operation was performed in two cases at the age of seventy-six and seventy-eight years with the same perfect success as in younger years. When in a given case the patient presents a decidedly pronounced atheromatous degeneration of the arteries, the operation must be carefully avoided, because we risk the outbreak of glaucoma acutum. I had to make this experience by the loss of one eye of an old lady. Only I had the consolation that the first eye was perfectly cured by the mentioned method, when on the second the glaucomatous inflammation broke out in consequence of a severe cold, in a moment when the patient was more than eight hundred miles away from me. Another operator of great reputation in Germany had the misfortune to see his patient lose both eyes because the atheromatous degeneration of the arteries had not entered into his calculations. Although I had cultivated the method of artificial maturation with such exceedingly brilliant results during more than twenty years, I have adopted for the last three years Förster's method. It does not require such a minute attention on the part of the operator, and is preferable for the patient, because he has not to sacrifice so much time. The puncture of the anterior chamber can be carried out with any pointed instrument we may have at hand. It will then be sufficient to rub the cornea for a certain time over the closed eyelids to obtain the desired result. The introduction of special instruments, so highly praised by some oculists, is abandoned by me completely, because I had the misfortune to make with it some very disagreeable experiences. A patient, in whose eyes I performed the maturation by introducing a curved spoon in the anterior chamber, presented, a short time afterwards, an insidious chorio-iritis with hypopyon. The affection could only be checked by a long-continued treatment and the consecutive execution of an iridectomy. Finally, I had to be satisfied to obtain moderate sight in one eye; the patient refused to be operated upon in the other one. In a second case the first eye was perfectly cured, the second became phthisic. A third patient escaped with a partial leucoma. Every one will conceive that I abandoned a method which had brought me greater losses than all the artificial maturations in former years. To-day I am completely convinced that these insidious coincidents must be attributed to instruments not treated sufficiently with antiseptic solutions. Since I changed the method, I have not seen any case that took an unhappy turn. Easy as may be the artificial maturation under normal circumstances, it cannot be denied that cataracts complicated with chorio-iritis require the greatest precaution. In two cases which were aggravated with chorio-iritis and presented cataracts of hard forms with fine radial stripes, the puncture of the anterior chamber had to be repeated three or four times before I could observe any progress in the development of cataract. One of these cases required a year before the maturity was perfect, and in the second I was obliged to extract the semi-transparent lens fifteen months afterward; the cataract had remained stationary. The results were in both cases perfect. In a third case I could neither obtain a result by a twice-repeated puncture, nor by splitting the anterior capsule, that was repeated three times without any success. The following morning the capsule wound was already so densely agglutinated that further operation appeared to me to be dangerous. I will add that these three cases had been subjected, many years ago, to the iridectomy, and were all distinguished by the malignant form of the complication.

The different cataract operations that I performed during so many years were executed without ever administering chloroform. The use of an anæsthetic is to-day still less necessary, because the instillation of some drops of a cocaine solution insures to the patient the desired insensibility. The after treatment of cataract patients consists only

Digitized by Google

in the careful observation of physical and intellectual rest. It is always an exception when the application of ice compresses has to take place during twelve to twenty-four hours.

This exposition regarding the technical execution of cataract operations would be insufficient if I would forget the iridotomy, an operation that was revived by Dr. deWecker, and can be regarded as a necessary complement to every kind of cataract operation. When in the course of acute or latent iritis an extensive agglutination between the iris and the residues of the cataract masses has taken place or led to an occlusion of the pupil, there is not any method of operation that, under apparently desperate conditions, can create such brilliant results for the restoration of sight as the iridotomy. I performed the operation 424 times in cases that came partially from my own, partially from the practice of other oculists. I never performed the operation after the complicated prescriptions and with the complicated apparatus invented by Dr. de Wecker, but from the first to the last case in such a way that the point of Graefe's extraction knife was introduced through the cornea and the anterior chamber directly into the iris, enlarging by saw-like tracfions the formed aperture to such an extent as corresponded to the wants of a given case. The split in the iris, linear in the first moment, takes in a short time a round or elliptic shape, so that the new created pupillary aperture resembles a natural one. It may happen that the operation must be repeated two or three times in longer intervals in eyes that are already approaching the limits of a beginning phthisis, before the desired result can be obtained. If with a natural consistency of the eyeball and a perfect technical result, the new pupil begins slowly to close, then the suspicion will not be unfounded that the cause is to be searched in the presence of latent syphilis. It is evident that an anti-syphilitic treatment has to take place before the operation can be repeated. I do not hesitate to call the introduction of iridotomy by de Wecker an operative advance of the first rank, because the operation can bring help where formerly we would have said : *lasciate ogni speranza.*

If you wish it so, gentlemen, my method of iridotomy is nothing else than a sort of linear discission, capable alike to ripen a cataract, to operate on a cataracta membranacea, and not less useful in many forms of cataracta secundaria.

My exposition, which I have given you, does not pretend to be complete; it simply will explain in short outlines that we can reach the greatest results with the most simple instruments.

DE L'EXTRACTION DE LA CATARACTE SANS IRIDECTOMIE, ET DU MOYEN DE PRÉVENIR LES COMPLICATIONS.

THE EXTRACTION OF CATARACT WITHOUT IRIDECTOMY, AND THE MEANS OF PREVENTING COMPLICATIONS.

ÜBER DIE STAAREXTRACTION OHNE IRIDECTOMIE, UND ÜBER DAS MITTEL ZUR VERHÜTUNG DER COMPLICATIONEN.

PAR LE DR. GALEZOWSKI.

Permettez-moi de vous parler de la nécessité qu'il y a à revenir à l'extraction de la cataracte à grand lambeau sans iridectomie, par conséquent à l'ancienne méthode française à grand lambeau, et de supprimer, au moins dans la grande majorité des cas la méthode combinée de Graefe.

Le sujet que je vais traiter devant vous, est des plus intéressants, vous l'avez mis vous même dans le programme du Congrès. Il s'agit de savoir quel est le procédé d'opéra-

Digitized by Google

tion de la cataracte qui doit être adopté comme le plus avantageux, et si les complications qui se produisent à la suite de l'opération sont faciles à guérir.

Je vous parlerai, si vous le permettez, de mon procédé, qui n'est qu'une modification de la méthode de David.

C'est en 1882 que j'ai fait ma première communication à ce sujet à la société de chirurgie de Paris, à l'époque où tout le monde restait fidèle à l'ingénieuse méthode de de Graefe. A ce moment je ne faisais l'extraction à lambeau que dans des cas isolés, je dirai presque exceptionnels.

Aujourd'hui, cinq années d'expérience ont ratifié mes premiers essais, et je puis vous dire, Messieurs, que ce procédé peut et doit occuper la première, la principale place dans l'opération de la cataracte.

La méthode d'extraction avec iridectomie commence à perdre de plus en plus de terrain ; en Europe comme en Amérique on commence à se persuader qu'il est plus naturel et plus avantageux sous tous les rapports de ne point toucher à l'iris dans cette opération.

Dans le dernier numéro de Juin, 1886, *The American Journal of Ophthalmology*, le docteur Culbertson publiait un article sur les résultats satisfaisants de l'extraction de la cataracte faite d'après mon procédé. Ce travail porte pour titre : *Four Cases of Galezowski's Method of Cataract Extraction; the Last Slightly Modified*, p. 149.

En France, Messieurs Parcas et Abadie ont adopté l'extraction sans iridectomie pour un certain nombre de cas de cataracte, et avec des modifications différentes. Mais il faut avouer que la grande majorité des chirurgiens n'ose pas abandonner la méthode combinée de Graefe, et craint de pratiquer l'extraction à grand lambeau. Il existe partout une certaine hésitation et une sorte de crainte dans la généralisation de cette méthode.

C'est pour dissiper cette crainte que je viens vous apporter des nouveaux arguments, qui vous prouveront d'une manière saisissante, que les inconvénients qu'elle présente, sont presque nuls en face des avantages qu'elle procure.

Je n'ai pas besoin de vous décrire ce procédé, je vous dirai seulement, que c'est à l'aide du couteau de Graefe, qu'il faut tailler un grand lambeau cornien, mais en modifiant sa forme et un peu sa position. Ce n'est pas, en effet, un grand lambeau sphérique, à la périphérie de la cornie qu'il faut placer, comme faisaient nos prédécesseurs. Mais il faut donner à la plaie une forme ellipsoïde, tenant juste milieu, entre celuide de Graefe et de David; il ne faut pas la plaie descendre jusqu'au diamètre transversal de la cornée, ni qu'elle aille non plus s'avancer trop près du bord sclérotical. C'est en rapprochant la ponction et la contreponction de la sclérotique mais n'engageant jamais le couteau dans cette dernière membrane, on arrivera à tailler le lambeau ellipsoïde que je recommande, et qui est le point capital du succès. L'iris dans ces cas n'empêchera pas la sortie du cristallin, aussi volumineux qu'il soit, et on ne sera pas obligé de l'exercer.

Mais on m'objecte à cela, que sans iridectomie on s'expose à avoir de nombreuses complications et que le résultat définitif de l'opération n'est pas aussi complet que celui qu'on obtient par la méthode allemande, en faisant une entaille dans l'iris. On s'expose, notamment dit-on, à avoir la hernie très fréquente de l'iris avec staphylomie consécutive, l'iritis et la cataracte secondaire, c'est à cet objet que je veux répondre aujourd'hui, en vous montrant, mes chers confrères, comment on peut éviter ces accidents et comment y porter remède lorsqu'ils se produisent.

Permettez-moi maintenant de vous signaler quelques points que je considère essentiels dans mon procédé opératoire sans iridectomie. Le danger de l'ancienne méthode de David était la forme de la plaie, grand lambeau circulaire, longeant la sclérotique, qui rendait la cicatrisation difficile et provoquait la suppuration et la hernie. Dans mon procédé que je vous recommande, cette forme est semi-elliptique, et son étendue ses

Digitized by Google

aproportionnelle au volume du cristallin. Mais ce qui importe surtout, c'est que la ponction et la contre-ponction doivent être placées, non pas dans la cornée transparénte comme le faisait David, mais dans le bord de sclérocornien opaque de la cornée, comme cela se faisait de Graefe.

Un autre point important dans mon procédé, c'est de placer le sommet de lambeau à deux millimètres et quelquefois même deux et demi millimètres. Par ce moyen j'évite la hernie de l'iris.

Le troisième point de mon procédé que je considère aussi très important, est la discission de la capsule, immédiatement après la ponction cornienne, avant de faire la ponction, et elle est pratiquée avec la pointe même du couteau de Graefe. La discission de la capsule avec le couteau à cataracte avant la contre-ponction est selon moi une des conditions importantes du succès de l'opération; depuis plus de 15 ans je l'ai adoptée dans mes opérations de la cataracte et je ne saurais trop la recommander.

Mais lorsque la capsule a subi un dégénérement certain, grasse ou autre et qu'elle est devenue d'un opaque résistant, il est inutile alors d'essayer à la déchirer, et il faut dans ces cas la saisir, après que le lambeau sera terminé, avec une pinu pupil lain et l'extraire.

Ce procédé présente des avantages incontestables mais il n'expose pas moins à quelques accidents et à des complications, et c'est de ces complications que je vais m'occuper.

Les complications les plus fréquentes que peut amener le procédé sont les suivantes: (1) Hernie de l'iris. (2) Suppuration de la cornée. (3) Iritis. (4) Cataracte secondaire.

1. *Hernie de l'Iris.*—C'est une complication qui peut se produire, lorsqu'on fera une plaie trop périphérique ; c'est pour cette raison que je tiens à recommander, à ce que le sommet de la plaie ne soit pas trop rapproché du bord sclérotical, ce qu'on obtiendra en donnant à l'incision la forme semi-elliptique.

La hernie de l'iris est souvent produite par la faute du médecin lui-même, je dirai, presque par sa trop grande curiosité de savoir comment se comporte l'œil et la plaie dès le lendemain de l'opération. Il ouvre l'œil, pour examiner la plaie, recommande au malade de regarder en bas ; ce dernier est nerveux, irrité, fait un effort pour exécuter ce mouvement, qui provoque une écartement du bord de la plaie, évacuation de l'humeur acqueuse et la hernie de l'iris. C'est pour cette raison je vous recommande, Messieurs, de ne pas ouvrir l'œil opéré avant cinq ou six jours.

A quoi peut servir, en effet, l'examen de l'état de la plaie dans les premiers cinq jours après l'opération. L'état des paupières nous indique suffisamment s'il existe ou non une inflammation ; il suffira donc de changer tous les jours le bandage, et si l'état des paupières et les autres symptômes éprouvés par le malade indiquent une inflammation, on pourra appliquer le traitement antiphlogistique et attendre avec l'examen de la plaie, j'acquiers qu'on peut supposer son adhérence et sa cicatrisation bien avancée.

2. *Suppuration Partielle ou Totale de Lambeau.*—Je dois vous déclarer, Messieurs, que sur plus de 700 opérations de la cataracte, faite par ce procédé de passé cinq ans, je n'ai pas vu plus de deux suppurations par 100. Mais pour éviter, en général cet accident, il faut avoir recours à la méthode antiseptique très sérieuse. Soigner bien les instruments, employer les latraces soit avec le sublimé à la dose d'un gramme pour 10000, soit l'acide borique.

A ce propos, permettez-moi de vous communiquer et recommander l'application directement sur la plaie d'une plaque de gélatine antiseptique que j'ai introduite en ma pratique depuis plus de deux ans, et à laquel le j'attribue les grands succès dans mes opérations. Ce sont des plaques gélatinées préparées avec le sublimé, que j'ai l'honneur de mettre sous vos yeux. Je l'introduis entre les paupières immédiatement après l'opération, et je fais le pansement habituel par-dessus les paupières. Cette gélatine roule sur

Digitized by Google

la plaie et reste 18, 20 ou 24 heures avant de se dissoudre. Pendant ce temps, l'humeur acqueuse remplit la chambre antérieure et les larmes, qui contiennent, comme j'ai prouvé, des microbes, ne s'introduisent pas dans la plaie et n'amènent pas de suppuration.

3. *Iritis.*—Je l'ai observée de temps en temps chez mes opérés. Cela tient souvent à la constitution syphilitique, rheumatismale, goutteuse, glycosurique. C'est pourquoi il faut faire, dès que cela se produit, un traitement antiphlogistique, antisyphilitique énergique, antigoutteuse, etc.

Mais, ce sur quoi j'insiste, c'est de ne pas employer dans les premiers trois ou quatre jours après l'opération ni atropin, ni aucun mydriatique, de crainte de provoquer la hernie de l'iris, qui serait plus dangereuse que l'iritis elle-même.

4. *Cataracte Secondaire.*—Cet accident peut se remontrer assez souvent dans l'extraction sans iridectomie, mais y a-t-il pour cela une raison quelconque à avoir des inquiétude pour le résultat définitif de l'opération? Pas le moins du monde. Si la cataracte secondaire devient apparente dans la première semaine après l'opération, il faudra attendre avec une nouvelle opération, jusqu'à ce que toute trace d'inflammation ais disparu. Alors, une simple discission de la cataracte suffit le plus souvent pour retablir la vision.

Maintenant, que j'ai fini ma communication sur l'opération de la cataracte, permettez-moi de vous exprimer toute la joie que j'éprouve à me trouver au milieu de vous, malgré les péripeties d'une traversée orageuse; j'ai pu enfin réaliser un rêve longtemps caressé, et je suis heureux de pouvoir saluer un des plus grands peuple du monde, et les plus éminents ophthalmologistes.

OPÉRATION DE LA CATARACTE.

OPERATION FOR CATARACT.

DIE STAAROPERATION.

PAR LE PROF. N. MANOLESCU,

De l'University de Bucharest, Roumania.

L'extraction de la cataracte, depuis quelque temps est revenue sur le champ des grandes discussions; et comme elle est l'opération la plus importante de la chirurgie oculaire, je crois qu'aujourd'hui c'est bien l'occasion pour exprimer nos opinions là dessus.

Mon opinion, en ce qui concerne l'opération de la cataracte sénile, est la suivante: L'extraction simple mérite toute notre attention; c'est à elle que devra s'arrêter la perfection de la méthode de l'extraction.

Elle est encore beaucoup plus facile à exécuter: on peut se passer dans certaines circonstances même de l'assistance et à la rigueur, on peut encore la faire, en ne se servant que du couteau linéaire.

Ce n'est pas la même chose que j'ai à dire de l'extraction combinée: celle-ci s'exécute, comme vous savez très bien, avec plus de difficulté et encore nécéssite l'ablation d'un morceau d'iris, ce qu'il serait à désirer d'éviter, je l'avoue carrément.

Mais en jugeant, l'extraction simple et l'extraction combinée, au point de vue du résultat optique total ou mieux, au point de vue du service optique satisfaisant rendu au plus grand nombre d'opérés, avec grand regret, j'avoue, je ne partage pas l'opinion de M. Galezowski, que je compte permis mes Maîtres.

Digitized by Google

Mon opinion est qu'actuellement c'est encore à l'extraction combinée que nous devons nous adresser, comme procédé général d'extraction.

Si la vue totale ou en somme obtenue par le procédé simple était même supérieur à celle obtenue par le combiné, je suis persuadé que cette somme-là de vous est partagée injustement entre les opérés, sans pouvoir faire autrement.

Cette injustice dérive surtout de l'enclavement de l'iris, qui est fréquent; on ne connait pas encore précisément ni les conditions qui le favorisent, ni celles qui le défavorisent; les mesures que l'on préconise comme préventives ne sont pas sûres et parmi celles que l'on reconnait favorisant l'enclavement, et que l'on pourrait éviter, il y en a qui se présentent quelque fois inopinément et avec la force nécessaire pour ouvrir une plaie que l'on croyait même définitivement fermée.

Cette complication post-opératoire, cette bête noire, comme l'appelait Crittchet est cause de beaucoup de dégats dans l'organe visuel; il ne s'atrophie que lentement; presque toujours il nécessite des opérations secondaires, souvent il est le point de départ des troubles traphiques s'étendant en arrière, ou en avant, dans les parties de l'œil et toujours il produit un fort astigmatisme.

En règle générale, je puis dire que plus il est heureux celui qui a échappé à cette bête noire, plus il est malheureux, celui qui n'a pas pu éviter cette complication.

Il y a encore des reproches à faire à l'extraction simple, mais tous ceux-ci, j'en suis complètement d'avis, resteraient d'une importance très secondaire, si l'enclavement de l'iris pouvait être combattu.

En tout j'ai pratiqué 109 extractions simples et j'ai fait tout mon possible pour les exécuter correctement: j'ai visité les cliniques en Europe où l'on préconise l'extraction simple, comme procédé général et j'ai mis à l'épreuve tout ce qu'on a dit qu'il prévient l'enclavement de l'iris; j'ai même fait des essais personnels dont j'aurais l'honneur de vous parler; pourtant j'ai la douleur de ne pas pouvoir partager l'opinion de mes vénérés maîtres de Paris.

Je cherche encore la consolation pour les enclavements, qui selon ma statistique s'élève à 30 pour 100, dans des dispositions particulières à mes opérés, à un manque de repos que des courbatures musculaires, qu'on voit très communément dans mon pays, ne leur ont pas permis de garder la tranquilité nécessaire.

J'ai dit, Messieurs, que j'ai pratiqué 109 extractions simples et que dans quelques cas j'ai fait des essais personnels pour m'opposer à l'enclavement de l'iris, permettez moi de vous les communiquer sommairement.

J'ai enregistré d'abord 37 cas, dans lesquels j'ai fait simplement l'extraction simple, sans instiler rien, ni dans la chambre antérieure, ni dans les culs-de-sac conjonctivaux et sur ce nombre j'ai 25 résultats sans le moindre enclavement de l'iris; 12 enclavements plus ou moins grands à la suite desquels il y a quatre mauvais résultats permettant aux opérés à se conduire difficilement, et huit médiocres (permettant seulement à se conduire) après une guérison lente et interventions chirurgicales secondaires.

Dans 21 cas, j'ai fait le lavage ézérinique, d'après la méthode de Mr. de Wecker et le résultat a été le suivant: huit enclavements plus ou moins gros, qui ont causé quatre résultats médiocres. Je dois noter que sur les 21 extractions il y a eu cinq résultats médiocres mais un à la suite des restes corticaux.

Dans 14 cas je n'ai fait qu'instiller de l'ézérine un pour 100, une ou deux fois dans le sac conjonctival. Sur ce nombre j'ai enregistré quatre enclavements, dont le résultat a été trois fois médiocre et une fois mauvais, par irido-choroïdite consécutive.

Dans une série de 33 extractions simples j'ai recouru, contre l'opinion générale, à l'instillation des mydriatiques et spécialement à l'atropine (trois pour 100) dans quelques cas immédiatement avant l'opération, et aussitôt après; dans d'autres cas, après l'extraction seulement. Le résultat est beaucoup plus satisfaisant au point de vue des enclavements. J'ai enregistré huit enclavements plus ou moins gros, dont la suite

Digitized by Google

a été six résultats médiocres et deux mauvais, par irido-choroïdite et infiltration de la cornée. Dans quatre, j'ai fait l'extraction après avoir pratiqué 24 heures avant, l'arrachement du nerf nasal externe (opération de M. Badal). Je me demandais si ce arrachement ne pouvait pas avoir une influence sur la circulation de l'œil, puisque on a vu qu'il abaisse temporairement la pression intra-oculaire en cas de glaucôme, et par conséquent s'il ne pouvait pas défavoriser la hernie de l'iris. Le résultat est le suivant: un enclavement sur quatre cas.

Comme vous voyez, dans ma statistique la grande cause des résultats médiocres ou mauvais c'est l'enclavement iridien ; dans d'autres statistiques ce sont les inflammations consécutives dans le tractus uveal, et pour avoir obtenu de bons résultats on a du faire beaucoup de discissions.

Ce sont, je crois, au moins des difficultés très désagréables, surtout aujourd'hui quand le monde sait qu'il réussit du premier coup.

Messieurs, si jusqu'à ce qu'on trouve un moyen sûr contre l'enclavement de l'iris, je ferai, comme procédé général, l'extraction combinée à l'iridectomie, c'est parce que je suis persuadé que la brêche iridienne n'empêche nullement d'avoir l'acuité visuelle satisfaisante, pour tout travail, et que cette brêche iridienne me permet d'être plus sûre du résultat que je poursuis, puisque l'enclavement de l'iris, grâce à elle, ne peut pas se présenter qu'exessivement rare et alors très partiel, et puisqu'il m'est beaucoup plus aisé de nettoyer la chambre postérieure. J'emploie presque toujours la pincette à dents pour enlever un lambeau de la capsule antérieure.

Encore cette brêche iridienne, pendant la durée de sa guérison, rend l'opération de la cataracte très peu pénible pour les malades, parmi lesquels il y a beaucoup dont le premier mot est celui-ci : Mais Docteur, on dit que je dois me soumettre à la torture de rester immobile pendant plusieurs jours?

Je me suis fait, Messieurs, la conviction qu'un bien opéré par le procédé combiné, peut même quitter la clinique avec un bandeau bien fait et revenir le lendemain.

Mais je dois ajouter ce qui me préocupe au plus haut degré pendant l'opération : C'est de faire une jolie pupille artificielle, je veux dire qu'elle ne dépasse pas en largeur quatre millimètres ; qu'elle ait les côtés parallèles, que l'iris soit bien coupé jusqu'à son insertion, et ça va sans dire, qu'elle soit faite à partie supérieure.

Pour obtenir une pareille pupille je ne cherche pas, comme on le faisait de tout temps, le sphincter de l'iris ; mais je pince l'iris du milieu de ses fibres radiaires et je ne donne le coup des pince-ciseaux qu'au moment que j'aperçois au bord de la lèvre inférieure de la plaie un trait noir.

Et comme c'est la condition essentielle d'une jolie pupille, je tâche de prendre l'iris dans la chambre antérieure même, je veux dire que s'il venait à prolaber, je le fais rentrer et cette maneuvre qui consiste dans un léger massage de la plaie est des plus simples et sans danger aucun.

Vient-on à prendre l'iris en dehors du milieu de ses fibres radiaires, alors la pupille sera plus large du côté de l'insertion iridienne ; mais l'éxécution de cette pupille, très favorable à la coaptation, est un peu douloureuse, par le tiraillement qu'on exerce du côté de l'insertion de l'iris.

Ce n'est pas encore finir ma préocupation ; j'éloigne soigneusement, au moyen de la spatule en caoutchouc, l'iris des angles de la plaie et je passe, de la même manière, la spatule entre les lèvres de la plaie, ainsi que j'ai vu faire mon Maître de Wecker.

Je lave de nouveau l'œil avec une solution de sublimé corrosif trois pour 100, dont un courant, que j'exprime d'une pelote d'ouate imbibée, passe plusieurs fois sur l'œil, et pendant qu'il est plongé dans cette solution, j'applique un bandeau occlusif, c'est-à-dire, de l'ouate imbibée de la même solution, remplis le cavité orbitaire, un goutte-perche couvre largement l'ouate et puis une bande légère et longue de trois à quatre millimètres fixe le tout sur l'œil.

Digitized by Google

Le lendemain, je change généralement le bandeau que je trouve propre et chaud.

Grâce à ces précautions, que je viens de dire, je trouve que l'extraction combinée, excessivement rare peut se compliquer par l'enclavement d'iris dans les angles, que par conséquent la pupille conserve, sans changement sensible, la forme qu'elle a au moment de l'opération ; que l'enclavement de la capsule, que je ne considère pas du reste très grave, se présente aussi très rarement (sur 1386 extractions combinées, je ne l'ai vu que trois fois ; une fois il y a été cause d'irido-choroïdite et deux il a retardé seulement la guérison qui a eu lieu avec une bonne acuité visuelle). Grâce à ces précautions, je crains encore moins l'ophthalmie sympathique ayant le point de départ dans la plaie faite dans l'angle irido-cornéen et dont je n'ai pas encore des exemples bien nets enregistrés. Du reste je considère l'ophthalmie sympathique après l'extraction, n'importe qu'il soit le point d'infection de l'œil opéré, absolument identique, en ce qui concerne les conditions de sa présence, à toute ophthalmie sympathique éclore en dehors de l'extraction de la cataracte.

Pour ce qui concerne la suppuration, le grand danger après l'opération de la cataracte, j'ai moins de raison à croire que l'extraction combinée y exposerait d'avantage.

Au contraire, je crois, qu'un lambeau conjonctival donne plus de vitalité, par conséquent plus de résistance à la plaie contre des causes nocives.

Du reste, je ne cesserai pas les essais pour pouvoir arriver moi aussi à acclamer le procédé de l'extraction simple qui doit faire, je le repète, l'objet de nos aspirations.

WHAT IS THE BEST AFTER-TREATMENT FOR IRIDECTOMIES AND CATARACT EXTRACTIONS.

QUEL EST LE MEILLEUR TRAITEMENT POUR LES IRIDECTOMIES ET LES EXTRACTIONS DE LA CATARACTE.

WAS IST DIE BESTE NACHBEHANDLUNG FÜR IRIDECTOMIEEN UND STAAROPERATIONEN.

BY T. E. MURRELL, M. D.,

Of Little Rock, Ark.

All surgeons will grant the one indispensable requisite to primary union in wounds in any part of the body, viz., a perfect and unmolested coaptation of the divided surfaces. The main object of all dressings is to this end, to which modern surgery would also add asepsis or antisepsis. A solution of continuity of tissue in any muscular or elastic structure must needs be forcibly retained in proper position by sutures, or bandages, or both, but for no other purpose would the surgeon use them, unless to retain some antiseptic agent in contact with the wound, to guard against the intrusion of germs. If a rigid structure is severed, as one of the long bones, it must be held in proper position until union is firm, so as to obviate deformity, and so, to overcome muscular tension, splints and counter-extension become necessary. If a surgeon makes a clean incised wound in the eyeball, does he not observe the same general principles of surgery?

Then the question arises as to the best method of securing perfect and immobile adjustment of the lips of a corneal incision, for whatever purpose made, where primary union is desired. If one will carefully study the structure of the eye and its appen-

Digitized by Google

dages relative to this point, he can hardly fail to see that much of the interference in the way of bandages and dressings, as now commonly practiced following operations on the globe of the eye, is largely, if not altogether, unnecessary.

If a healthy eyeball is enucleated and an incised wound made in the cornea, the lips of the wound fall together in the most exact manner, so that the globe preserves its figure perfectly, provided no unequal force is brought to bear on the outer surface at any point. This inherent tendency in the outer tunics of the eye—the sclerotic and cornea—to retain their proper curvature is very striking, and nothing but some disturbing force will overcome it. When the eyeball is *in situ* with all its attachments, there may arise influences that will disturb its normal rest, and the chief of these is muscular action. If the cornea or conjunctiva is offended, and at the same time the eye is forcibly retained in a fixed position, violent muscular spasm of the ocular muscles is likely to occur, which may, if there be an opened globe, force out the contents of the eye. But, on the contrary, if the eye is free, and the muscles, both the recti and obliqui, move harmoniously, the eye is rotated with an even force, and no tendency to gaping of a wound in the cornea is witnessed. Every operator has, doubtless, observed this after an iridectomy or extraction, when he has requested the patient to move the globe voluntarily in different directions.

Since, therefore, the harmonious action of the eye muscles does not disturb the equilibrium of the globe, unless, of course, their movements are extreme, the question occurs—is it necessary to retain, or is it wise to attempt to retain, the eye in a fixed position after an operation has been performed upon it; or is it ever so retained? Will compress bandages secure quietude of the eye without great danger in such cases? My impression is, from close observation and careful questioning of my operated cases, that although both eyes are snugly bandaged, there is very frequently a rotation of the globe underneath the lids. If, then, bandaging the eyes does not perfectly insure against rotation of the globe, even though both eyes are so closed, what good do bandages accomplish, and what good reason can be offered for dressing the unoperated eye as well? If we choose, however, to close the unoperated eye also when we come to apply dressings after, say, an extraction, it matters little what dressing is used on it, but for the operated eye it does matter immensely. We know that the eyelids when gently closed, so as to obliterate the palpebral fissure, form a most exquisite mould of the front of the globe. The slight but uniform tension of the orbicularis palpebrarum over the nicely adjusted tarsal cartilages keeps up such an even pressure on the cornea as to nicely balance it against the elastic cushion in which the globe is imbedded posteriorly. The conjunctiva is so perfectly even and soft that no friction whatever can occur in the movements of the cornea against it.

When the lid margins just touch there is formed, therefore, over the front surface of the globe of the eye a splint of the most perfect evenness and exquisite adjustment. Instead of any tendency to displace the lips of a corneal wound, therefore, the lids offer the greatest barrier to such displacement and serve to retain most perfectly their accurate coaptation.

In the rotation of the cornea under the lids it meets with no unevenness to disturb its perfect symmetry, or to endanger its being in any way distorted. Hence, absolute fixity of the eye, even were it possible, is not called for. It will be observed that the normal curvature of the lids has been relied on to maintain this perfect adjustment of a corneal wound, and to secure the unaltered shape of the globe. What more do we want, therefore, in dressing an eye with a corneal incision, than to secure passive closure of the lids? No doubt every eye surgeon will say this is exactly what he does. Let us examine the ordinary bandage and cotton, or charpie dressing, and see if they fulfill these indications. In order to retain the lids in a quietly closed position, it is necessary to exert a slight pressure either directly on the central portion of the upper lid over the

Digitized by Google

apex of the cornea, or else between this and the rim of the orbit, as some do by carefully filling in around the globe with the dressing, to produce, as they claim, equable pressure.

All pressure on the globe after an iridictomy or extraction is inimical to the best results, however evenly applied, but more so when unevenly applied, as it almost necessarily is by any mode of bandaging.

If the pressure happens to be greater over the apex of the cornea, the globe is flattened and the lips of the corneal incision displaced; or, should the pressure be too equatorial the globe is laterally compressed, its contents forced forward, and the corneal wound caused to gape. Again, if too little firmness in the pressure exists, the lids are not sufficiently supported and are liable to voluntarily or involuntarily open, as I have had my patients to tell me actually occurred. This may be the result of fixing the bandage too loosely in the beginning, or by its yielding or being slipped out of position by the movements of the patient's head on the pillow. Some authors put great stress on the nice adjustment of an elastic bandage to the eye after an extraction, offering the consoling suggestion that the skill can only be acquired by experience. But I have, after the most artistic application of the bandage, been somewhat shocked on the following morning to find it misplaced and gathered into strings over the eyes. This may occur in any very restless patient.

I have had some headstrong and self-willed patients to deliberately push the bandage off in hot weather, to cool the eyes. In very warm weather the bandage and dressing often become uncomfortable, and I have frequently, in answer to complaints on this score, removed all dressings and bathed the eyes freely with cold water, to the very great satisfaction of the patient. Another objection to this form of dressing is, by the retention of the secretions and the saturation of the material next the eyes with the same, and by the heat produced, septic processes are liable to arise and endanger the eye.

Conjunctivitis of a greater or less degree is almost certain to occur in very warm weather. Or, if there is a blennorrhœa of the lachrymal sac, infection of the eye is eminently prone to occur. Some have discarded bandages, and rely, instead, upon long strips of adhesive plaster passed from the forehead to the cheek to hold the dressing in place. All the main objections to the bandage are applicable to this.

As has been stated, fixed retention of the lids in a passively closed position until perfect union in the corneal wound shall have taken place accomplishes all that can be accomplished in the way of dressing, and whatever will do this without endangering the normal exquisite balance of the globe against the smooth lid surface will answer all the requirements of rational surgery.

Nothing answers the purpose so nicely as a narrow strip of thin isinglass plaster, extending from one lid surface to the other. By this means the lids are passively held closed, all tendency to action of the levator palpebræ superioris is successfully controlled, and there is no unequal pressure on the cornea or globe, no accumulation of heat, no retention of secretions, and, consequently, no necessity of daily or thrice daily changing the dressings. Too much stress cannot be put upon the perniciousness of so often disturbing a wound or its surroundings, where primary union is desired.

In addition, this simple dressing allows of free and repeated inspection of the lids, by which we can judge of the healing process. What material is used, and just how applied, does not matter very greatly, so it is pliable, adheres well, and does not exclude light or preclude thorough inspection of the eyelids without disturbing it.

For myself, I have been in the habit of using a light strip of isinglass plaster, while Dr. Chisolm, I believe, uses the same, but a piece large enough to pretty much cover both lid surfaces. Dr. Michell prefers gold-beaters' skin, of which he applies only a very narrow strip in the manner I have suggested.

As it is universally agreed that a well-behaving wound should not be molested

Digitized by Google

while reparation is going on, it is best not to disturb the eye in any way until sufficient time has been allowed for healing, when it should be finally removed and the eye set at liberty.

The mode of dressing an eye after an extraction or iridectomy having been decided upon, the question arises as to what other elements figure more or less conspicuously in the final success or non-success of the case. One of these is as to bodily restraint.

Is the customary rule of confining patients for a number of days in bed at all necessary? It has been generally so claimed, and almost universally practiced, but recent experiences go to prove it altogether a mistake. With the lids closed by an adhesive strip, the globe of the eye is equally supported in all directions; so that whatever comfortable position the body may assume, whether prone or erect, will in no manner interfere with the condition of the eye. So that it may be considered safe to allow the patient to assume whatever position affords him the most grateful sense of physical comfort, whether sitting or reclining, so that all strained positions or violent motions are avoided, and which such patients are not likely to attempt. These liberties may be granted immediately following the operation, and, according to the large experience of Dr. Chisolm, no harm whatever, but only comfort and gratitude, have come of it, thus establishing the entire innocence of such procedure.

Another subject of much and vital importance in the after management of operated cases, especially extractions, is, in how far does light influence the result, favorably or unfavorably. Just here has been almost universal agreement hitherto on the accepted belief that light is an irritant to the eye, and if an irritant it should be excluded following operations, so as to afford as complete quiescence to the eye as possible.

But, unless some morbid process has engendered a photophobia, ordinary diffused daylight is not an irritant, but is more grateful than its entire absence for a lengthened period. By rest to the eye after an operation, which it requires as well as any other organ after a surgical procedure, is meant complete abeyance of all muscular effort, such as convergence and accommodation, and absence of all retinal labor in picture forming and transmission. This, for the eye, is complete rest. The lids alone afford sufficient protection against any possible excess of light, and yet they are sufficiently diaphanous to allow of the penetration of sufficient light to the retina to afford it its natural stimulus, and to obviate photophobia and depression of spirits. As any one knows, he can look directly toward the sun through his closed lids, without detriment to the eye, and yet with closed lids he can readily distinguish any change in the degree of illumination of a room. The total withdrawal of light, however, for a period of several days has a most pernicious influence over both body and mind. On the eyes it has the effect of exciting a chain of morbid phenomena, whereby there is finally a complete abhorrence of it, and attempted exposure to it inflicts the most excruciating distress. There is a great difference in the effect of conscious and unconscious darkness on the eye. If consciousness is wanting, as in lethargic sleep, the eyes may be kept in profound darkness for days without consequent photophobia, but it is not so if normal hours of sleep are observed and lucid intervals exist during which the nervous functions are all aroused and alive to environments.

If the eye has for some days been confined in almost utter darkness, a very moderate degree of illumination cannot, for a while, be comfortably borne. And, moreover, as is well known, the dungeon is the worst of punishments, leading in some instances to madness, and acute mania has been known to occur in the after-treatment of extraction by confinement in the dark for several weeks. Many of our cataract cases are old people, who are very impatient of restraints of all kinds, and often particularly unwilling to confinement in the dark. In fact, this of itself, they have often told me, was their greatest dread of the operation, and having once experienced it, often prefer having only the one seeing eye rather than submit to the same punishment in order to

Digitized by Google

obtain vision in the other. Personal experience has satisfactorily proven to me that iritis making its appearance about the fifth or sixth day after an extraction is oftener due to the brief exposure of the eye to the light during the first examination than to any other cause. There is almost invariably a scringing and shielding of the eye and a gush of tears, on the attempt to make anything like a careful examination of an eye four or five days confined in the dark, and often within a few hours after the patient complains of pain, and then follows iritis. In proof of the position here taken, iritis is far more infrequent under the methods here advocated.

Discard all bandages, discard all thick dressings, allow full diffused daylight in the room occupied by the patient, and as a reward there will be rarely an iritis, and never photophobia unless there is iritis. But, on the contrary, when the adhesive strip is removed after healing has occurred, the patient will complacently open the eye, deliberately look about him and out the window without the slightest manifestation of discomfort. It requires about five days for firm union to take place in the corneal wound, so that it is best that the eye be not disturbed in any manner whatever until the end of this time. If all goes well there is no necessity for it. The lids are free to inspection, there are no pent-up tears or mucus, and any accumulations in the inner canthus can be easily removed with absorbent cotton.

In one case of incurable dacryo-cysto-blennorrhœa, extraction was performed and the discharge from the lachrymal sac very carefully and frequently removed from the inner canthus with absorbent cotton from day to day until the sixth day, when the eye was liberated perfectly healed, with nothing more than a hyperæmic condition of the conjunctiva. Who would have dared seal this eye after the usual method and have taken the consequences? At the end of five, six or seven days, as we may judge advisable from the patient's reparative powers, we may remove the adhesive strip and let the eye go free. Within two or three days the patient may be allowed to go out of doors with perfect impunity. As a rule, with this method of treatment, the eye will be only moderately red on first opening it, in some instances scarcely at all injected, the iris bright, with no indications of inflammation, there will be no lachrymation or shrinking from the light, and the patient will at once want to test his sight. The time of convalescence is reduced about one-half over the system by dark room and bandage, the patient often being ready to dismiss in one week from the time of operation, ten days being about the average time he should be kept under observation.

When an iridectomy is performed, less care and less restraint are called for. As will be seen, by this method cataract cases are treated with less restraint than has been usual with iridectomies; but now iridectomies are no longer house patients. They come to the surgeon's office to have the iridectomy performed and, with only the operated eye dressed, they walk or ride home by the guidance of the other eye, with instructions to call at the office in two or three days for examination. If all goes well the eye is set at liberty and the patient discharged on the fifth or sixth day. Ordinarily, no trouble whatever occurs in these cases, but speedy and smooth healing in a few days may confidently be looked for. Of course, if the eye operated on is more or less degenerated, or in a glaucomatous condition, more watchfulness of the case is demanded, so as to be able to deal with any complications that may arise. But in a smoothly-made iridectomy in a sound eye there is no more risk in performing it in the surgeon's office, and treating it altogether as an out-door case, than there is in dealing with a squint operation in the same way.

While following an iridectomy there appears to be almost no risk in allowing the fellow eye to be at liberty, the greater disturbance of the globe by an extraction seems to offer some objections to this apparently reckless procedure. But if, as has already been declared, ordinary voluntary movements of the eye do not tend to disturb its symmetry, there may exist a harmlessness in it beyond the expectation of those who have

Digitized by Google

not tried it; and as facts always take precedence of hypotheses, the experience of Dr. Chisolm in some fifty or sixty extractions, in which he has boldly let the unoperated eye go free, seems to afford satisfactory evidence that our former views have been ill-founded.

What can be more gratifying to both patient and surgeon than to be assured of a system of dealing with so important an operation by methods at once so simple and so satisfactory. It strips a once formidable undertaking of nearly all its horrors, and inspires both surgeon and patient with a confidence hitherto not attainable with all the dread of slow convalescence, dark rooms, confinement in bed and expectant iritis and long-continued photophobia. Certainly a bright era has dawned upon those unfortunately afflicted with cataract, since with cocaine to take the place of chloroform or ether, the operation can be performed painlessly and absolutely free of danger to life, and the necessary care following the operation is reduced to such perfect simplicity and conformity with almost every comfort, that it is no longer to be dreaded, even by the most timid.

I do not in this paper, the purpose of which is to set forth the advantages and sound principles in this system of after-treatment for cataract and iridectomy operations, propose to act as historian, but feel it my duty to accord thanks where due.

Personally, I am indebted to Dr. Charles E. Michell, of St. Louis, for those important innovations—the simple strip to close the eye and the admittance of abundant light into the room the patient occupies. Dr. Michell has also for many years performed iridectomy as an office operation, dressing only the operated eye, and his suggestions first led me to adopt it. With his experience in iridectomies thus treated with so little restraint, I wonder that he did not fall upon a freer method of dealing with his cataract cases, it having been his custom until quite recently to confine his patients to bed for eight or ten days, for the one naturally suggests the other.

Dr. J. J. Chisolm, the worthy President of this Section, was the first to remove bodily restraints and to allow the patient to sit up or lie down immediately after the operation, much as his personal comfort should incline him. He also took the initiative in closing only the eye upon which extraction had been performed, and of emancipating the fellow eye from all dressings and restraint. These two important additions to the other methods have wonderfully added to the comfort and gratification of cataract patients; for introducing and establishing the safety of which, and for his very liberal promulgation of these methods in all their details and his experiences with them through the medical press, whereby they have become so generally and thoroughly known, that any operator may intelligently test them if so disposed, he is entitled to the thanks of every ophthalmologist.

DISCUSSION.

Mr. Power, of London.—I must acknowledge myself an adherent of that method of operating for cataract in which no iridectomy is done. I divide the cornea at the sclero-corneal junction, making a rather large opening. As soon as the section is made, the capsule is lacerated with a cystotome, and the lens is gently tilted till it escapes. What cortical substance can be removed is then cleared out with a curette. A drop of eserine (2 per cent.) is then instilled and the eye is tightly bandaged. Of the four accidents mentioned by Dr. Galezowski I do not find that hernia of the iris is very common, and think that the less the eye is examined and tortured, the less likely is it to occur. Suppuration of the cornea is to be avoided by attention to the general health, and by care in securing perfect cleanliness. I think iritis is more apt to occur after an iridectomy than when that proceeding is not done. The true bugbear of cataract is undoubtedly secondary cataract; but surely those

Digitized by Google

who practice iridectomy do not mean to say that they do not meet with a similar difficulty; and in both instances it must be dealt with in a similar manner, with one or two needles. In conclusion, I think we should, if possible, operate without iridectomy, but if it be found difficult to replace the iris after the section, or if much cortical matter be left, or if adhesions exist between lens and iris, then it is easy to do the iridectomy.

Dr. LANDOLT, of Paris, said:—If I differ in some respects from our eminent colleague, Dr. Mooren, I am glad to say that in the main I am of his manner of thinking, and from what he has said I draw these two conclusions: 1st, That there does not exist, and there will never exist, a single unique method of cataract operation. 2d. That it is necessary to pay the greatest attention to the general condition of the patient, and that in by far the majority of cases we shall be compelled to hold on to iridectomy. Each of us has experienced an epoch, in which we tried to simplify the operation for cataract, and it ended by our returning to the methods of our fathers. As regards myself, I went through this epoch in 1878, at which time I performed all my cataract operations without iridectomy. I had some superb results, and again some misfortunes, and finally I came to the conclusion that the operation with iridectomy was, in the strictest sense, the most satisfactory. I do not see that antisepsis and the discovery of cocaine have, in the true sense of the word, reduced the cataract operation to the point where we can call it simple. The dangers are, from my standpoint, adhesions of the iris and the great difficulty of keeping sufficiently clear the field of operation. To avoid the first danger I determined, at the beginning of my career, to make the incision —as first conceived by Liebreich—as far from the corneal border as possible. But I very soon found that this method was associated with great risks. As regards the method of incision, I always make the sclero-corneal incision, whether I do the operation with or without an iridectomy. But I shall relegate this method of cataract extraction (without iridectomy) to those cases which are absolutely favorable, absolutely simple, absolutely ripe. And here allow me to make clear an evident misunderstanding between Mr. Power and myself. Neither myself nor any one of my colleagues who perform the operation with iridectomy pretends to assert that this procedure is an absolute guarantee against iritis and secondary cataract, but experience has taught me that these complications occur far less frequently than when the iridectomy is left out. And one word about the iridectomy. I consider as dangerous a too small iridectomy—one, in a word, that is not in proportion to the corneal incision. In such a case the danger of prolapse of the iris is exceedingly great. And then, as regards the excision of the iris, which it is often necessary to make the second or third day after the so-called "simple operation:" it is by no means comparable to the iridectomy we make prior to the exit of the crystalline lens. In this subsequent excision there remains always an anterior synechia, with all its accompanying dangers. Finally, it has been said in this discussion that the sublimate solution is irritating to the eye. I declare as an absolute fact that the sublimate solution—1 : 5000—that I have used in my practice for years, is by no means irritating, and is perfectly antiseptic.

Dr. ABADIE, of Paris.—The question of operating in cataract with or without iridectomy has agitated our minds a great deal nowadays. It does not seem to me that the question should be so divided, but rather the distinction should be made between those cases which should be operated upon with iridectomy and those where iridectomy is useless. For a considerable time have I endeavored, as far as possible,

Digitized by Google

to operate without iridectomy, and my efforts were crowned with success in those cases where the cataract was simple and had arrived at perfect maturity, and in this kind of cases I assert that this method is superior to all others. Unfortunately, at this date we can no longer, as in the time of Daviel, wait for the cataract to completely mature. Now, the patient, just as soon as he finds he can no longer read or walk about alone, comes to us and demands an operation, and we cannot refuse him. In such cases, gentlemen, the operation without iridectomy does not give such satisfactory results as the combined operation. The evacuation of the cortical mass is difficult and incomplete. The iris is bruised, and we very frequently have a prolapse, and a second operation is almost unavoidable. Hence, in all forms of cataract which have not reached perfect maturity an iridectomy is indispensable.

Dr. MARMION, of Washington, said that he had listened with a great deal of interest to the arguments in behalf of the two methods—with or without iridectomy; but, said he, nothing that I have yet heard has shaken my faith in the iridectomy. The percentage of successes is undoubtedly in favor of the combined operation. Among the many arguments in favor of the latter operation is the free exit it gives to the cortical substance.

Dr. DUDLEY S. REYNOLDS, of Louisville, said:—It is evident that extraction may be done without iridectomy in carefully selected cases. I do not think the experience of the gentlemen who have read the exhaustive essays, nor of those who have taken part in the discussion, tends to establish any universal rule. In well-matured cataracts in persons whose eyes are otherwise sound, and in a good state of general health, the pupil may be widely dilated with cocaine, which, of course, should always contain either carbolic acid or corrosive sublimate. In such cases the operation should be performed with great care. I use in such cases Graefe's knife, which I aim to introduce, as nearly as possible, in the plane of the anterior surface of the iris, and passing the point of the knife well over the pupillary margin, I suddenly depress it, so as to prick the capsule, taking care not to allow the knife to penetrate deeply into the lens, lest it may be dislocated. I make the puncture in the capsule as the knife advances, and, passing the point on, when I come to the sclero-corneal juncture I raise it; pressing the blade upon the iris, I make the counter-puncture. Advancing the blade by a sawing motion, I stop and allow the aqueous humor to escape slowly before the section is completed. I aim not to turn the edge of the knife forward into the cornea, but to cut as nearly as possible in the plane of the anterior surface of the iris. Having proceeded in this way to the completion of the section, I find that the central incision of the lens capsule allows it to tear in different directions, and the difficulty in removing the cortical substance is not so great, nor is the iris so apt to prolapse where the section is made as I have described. If, however, I contemplate iridectomy when I begin the operation, I use Beer's knife, making the section upward and puncturing the capsule with the point of the knife as it advances through the anterior chamber. I attach great importance to this method of opening the capsule, as it seems to me fraught with less danger than the introduction of the cystotome. I never use fixation forceps, nor permit the hand of an assistant to touch the patient's eye. In fact, I have no use for an assistant. When the eye is once impaled upon the knife with which the section is made, the touch of the finger places it entirely at the command of the operator. If aseptic conditions are secured, the dangers from cataract extraction seem to me reduced almost entirely to secondary inflammations of the iris and ciliary body. The dressing I employ after extraction has been simplified some-

Digitized by Google

what after the suggestion of Dr. Michell, of St. Louis, and in accordance with the experience of our President, Dr. Chisolm, of Baltimore. I now use a small bit of absorbent cotton and a single piece of adhesive plaster drawn from the cheek to the brow, the cotton being used simply to take up the tears and any other fluid that may escape from between the lids. If the eye shows the least sign of irritation, I remove the dressing daily; otherwise, I permit it to remain until I think the wound has well united.

Prof. P. D. KEYSER remarked that, as experience was a good teacher; and as he had operated considerably over two thousand times for cataract; and as the natural result of the meeting of so many together was to give experience and exchange views on subjects, he desired to say that it was impossible to express whether the extraction of cataract should be made with or without an iridectomy; that the operator must examine his case carefully, to see what kind of a cataract he had before him, and be the judge of what is best to do. During his number of years' practice he has made operations according to all of the methods recommended, and his experience is that where the lens is completely hard, that is, with little or no soft cortical, it can be very readily and well extracted without an iridectomy. But where there is much soft cortex an iridectomy is preferable, making it easier and surer in freeing the eye of all foreign substances. He notices that Dr. Mooren recommends the iridectomy downward, which is, in his idea, the improper position. There are some persons who cannot be made to look down; this he has specially noticed in the Irish people. Before the introduction of cocaine, not using any anæsthetic, he often made the iridectomy below, with such persons; but the upper section is much preferable, for many reasons well known to the profession. In all cases of sluggishness in the iris to dilate promptly and widely on the instillation of atropia, he invariably makes preliminary iridectomy; for with such patients he has always found that iritis follows the operation of extraction if made, with or without iridectomy, in one sitting. As to Förster's method of ripening immature cataracts for operation, it has not been fortunate in his hands. He found, in many cases, that the rubbing, or manipulation, caused a low grade of iritis which formed posterior synechiæ, and such are not desirable concomitants in the operation for extraction of cataract. In the opening of the capsule, he desires to remove as large a piece as possible from the centre, and to accomplish this he had a cystotome made, with the fleam or cutting part at right angles to the stem of the instrument. Introducing it through the incision of the cornea, he makes a horizontal cut in the lower part of the capsule and, catching the membrane on the end of the instrument, draws it up and out of the corneal incision far enough to snip off a piece with the scissors. Sometimes the capsule is drawn up in a fold, so as to be easily grasped with forceps and thus drawn out for cutting off. In cases of cataracta Morgagni (s. cataracta lacta), when the lens is broken down into a chalky, milky liquid, it is desirable to remove all this substance from the eye, and not let any remain to float around and irritate and set up inflammation. To accomplish this, he always makes an upward iridectomy, and opens the capsule at its extreme upper periphery with a v. Graefe's cataract knife. In this way the capsule remains like a sack, with only an opening at its upper end, just at the mouth of the incision through the cornea. All of the broken, disintegrated lens matter readily flows out. If any remains, it lies in the capsule harmless, while dissolving in the aqueous. An operation can easily be made afterward on the capsule, at any time it becomes opaque. He operated in this way on an old negro woman, at his clinic, three years ago, and the capsule is still perfectly transparent and vision excellent.

In relation to Dr. Galezowski's method of operation without iridectomy, he

Digitized by Google

desired to ask if he ever had a prolapsus of the iris with consequential glaucoma after the extraction. He desired this information for the reason that, in 1875, he operated on a man, by the method of de Wecker, without iridectomy, and on the second day a prolapsus occurred, accompanied with great pain and hardness of the eyeball. To reduce the protruding iris he instilled eserine, and discovered that the tension was very soon diminished. This was his first discovery of the anti-glaucomatous effect of eserine. He communicated it to Dr. de Wecker, who mentions the subject in an article on eserine in the *Annales d'oculistique* and in Zehender's *Monatsblätter für Ophthalmologie.*

He has found the mercurial solutions irritating, and uses always the boric acid solution as a cleansing liquid. He always examines the urine of his patients for albumin before operating, and, if found, postpones the extraction until the albumin is removed by treatment, or declines to operate. He has found that inflammation of a serious nature invariably occurs after an operation with patients suffering with albuminuria.

Dr. J. L. Thompson, of Indianapolis said :—I have been greatly interested by the remarks of the distinguished essayists, as well as by those of the eminent gentlemen who have participated in the discussion.

No one operation suits all cases. With numerous other plodders in this field, I have clung to the advice given by Dædalus to his son, as he was escaping from the Cretan labyrinth, "*to take a middle course,*" and have, therefore, avoided the extreme peripheral section of the renowned von Graefe, as well as the more central one of Liebreich, and now, in most cases, make a similar one to that of the very eminent gentleman, Galezowski, of Paris.

A majority of us cannot number our cataract extractions by the thousand, and are therefore constrained to keep in known and well tried paths, leaving the experiments to gentlemen of the large cities of the world. For my part I would rather be certain of restoring the greatest number of my patients to useful vision than to get a smaller per cent. of very brilliant ones at the risk of loss of vision.

I usually, in senile cases, make a broad iridectomy; consequently, rarely have the iris imprisoned in the incision. When I meet with a failure, it is usually from capsulo-iritis or from iritis, and that even after the thirteenth day, from malaria and other exciting causes.

Suppuration of the cornea I now rarely meet with, and this is doubtless due to the use of the mercurial solution, which is now so universally used.

Dr. Baker, of Cleveland, Ohio.—After witnessing operations for cataract extraction by the leading American, English, German and French ophthalmologists, some years since, I came to the conclusion that as an operator with average skill and experience in these cases, I could not take the responsibility of performing the operation without an iridectomy. In order to avoid as far as possible the objections to an iridectomy, I have performed the operation as suggested by Wolf, of Scotland. Instead of making a large iridectomy and thus leaving the pupil keyhole shaped, I remove a small portion of the iris and leave the pupil *oval* after the operation.

With regard to the opening of the capsule, I believe with Dr. Reynolds that this is a very important part of the operation. I make the incision in the capsule at the periphery, corresponding as nearly as possible with the corneal incision; I am careful to remove all the cortical substance from the incision, but if there is a small portion retained within the capsule I do not resort to the spoon to remove it, as it will soon become absorbed and do no harm.

Digitized by Google

Many operators do not use enough care in delivering the lens. They seem to be in a hurry and deliver it as quickly as you can wink. It should be done slowly and disturb the contents of the eye as little as possible. I never touch the cornea with the finger scoop or other instrument while delivering the lens, and in this way add much to the comfort of the patient after the operation, as it prevents much of the disagreeable smarting always present when the epithelium of the cornea is disturbed by rough handling during the operation. I always follow the precaution mentioned by Mr. Power this morning, of using a new knife for every operation. A good, clean, sharp knife is of very great importance.

Dr. SWAN M. BURNETT, of Washington, had been lately doing all his extractions without iridectomy, and they amount now to twenty-three. He was so well pleased with the results that he should not return to the method with an iridectomy until bad results drove him to it. He did not believe that in all cases it would be found to be best, but he had recently extracted a black cataract with an immense nucleus, and a Morgagnian cataract by this method, and with good results in both cases, showing the wide range of applicability of the method. It was least applicable to lenses with much soft cortical, but only a few days ago he had successfully operated on such a case in this way. He had worked out some of the remaining cortical with antiseptic fluid, and he believed that such a procedure, if properly done, was not dangerous. The liability to iritis and other complications he did not consider greater without than with an iridectomy. He also thought that there was a great advantage gained when we could assure timid patients that the operation for the restoration of sight would be entirely painless without general anæsthesia, as it certainly is with cocaine and without iridectomy. It was his opinion that the good results by this method are better, and the bad results were not worse, than with iridectomy.

Dr. EUGENE SMITH thinks we cannot settle upon a method which is applicable to all cases, but as far as he himself is concerned, he thinks the iridectomy should be performed.

Dr. F. VALK, of New York.—The question before the Section being the advisability between the operation of cataract with iridectomy according to the method of Graefe, or without iridectomy according to Galezowski's method, it seems to me that we may have an operation which may combine both methods, that is, without an iridectomy and with the advantages which an iridectomy gives. For this purpose the past few months I have operated with a little instrument called an iris retractor, with which I draw back the iris to the position of full dilatation, and while holding it in that position, by a few gentle strokes of the spoon on the lower part of the cornea, the lens easily passes through the section. As it escapes, the ends of the retractor are raised and the iris passes backward to its position. We then retain a clear, round pupil.

Dr. J. J. CHISOLM stated that in his cases of extraction without an iridectomy he was still fearful of increased tendency to iritic complications. Although he has had the good fortune to lose no eyes by this method, he has not turned out a single case with an absolutely free pupil. They are more or less central and look well, but will not dilate regularly under atropia. In two cases he has had a hernia of the iris, although he had used liberally eserine drops after the extraction. He preferred to make a small iridectomy, only cutting off by one snip of the scissors so much of the iris as is drawn out by the iris forceps.

He uses freely to the eye operated upon a biniodide of mercury solution, 1 to 15,000. He operates under cocaine. After the speculum is inserted he floods the

Digitized by Google

eye with the mercurial solution, so that it comes in contact with every part of the conjunctival surface. After the operation is completed and before the speculum is removed, the eye is again flooded with the antiseptic liquid. He has seen this fluid, along with bubbles of air, flow in and out of the anterior chamber as the movements of the speculum would make more or less pressure upon the eyeball, gaping the corneal wound. Although he has seen no inflammation follow upon this accidental admission of the mercurial solution into the eyeball, he has never intentionally washed out the aqueous chamber.

He laid great stress upon two points in the after treatment of cataract extraction cases. The first was the closing of only the eye operated upon, leaving the other eye open for the guidance of the patient. The second was the closing of the eye operated upon by a slip of isinglass plaster, so diaphanous that light would readily pass through it to the retina. He treats his cataract cases in light rooms, from which sunlight only is excluded and in which friends can read to patients. He feels convinced, from a large experience, that the eyes are stronger and much less sensitive after cataract extractions since he has abandoned compresses, bandages and dark rooms. The comfort to the patient by leaving one eye open is immense. Experience has shown to him that the usual restraint practiced by surgeons on their eye patients is not only unnecessary but harmful. He does not now put his cataract extraction patients in bed. After operation they sit up till bedtime, getting up in the morning and dressing themselves, as is their daily habit. For a week they are confined to their rooms but are allowed to *see* their friends, walk about and eat their meals without being fed, thanks to the eye that is left open. His experience proves that if light is not excluded during the treatment, the eye, when exposed, will not inject nor weep. The exposure hastens convalescence. Unless some inflammation should occur, which is seldom the case, the original piece of isinglass plaster is not removed till the fifth day; after that time no dressing is used. Unless the symptoms call for it, he does not remove the primary dressing to inspect the eye. He believes that the daily inspection of the cornea, when not called for by the symptoms, is meddlesome and not without danger. His cases do not require smoked glasses for indoor use at any time in the treatment, because the eyes have never been made sensitive by the exclusion of light.

Digitized by Google

THIRD DAY.

RESTORING THE NORMAL POSITION OF THE TARSAL BORDER IN TRICHIASIS OF THE UPPER EYELID.

RESTAURATION DE LA POSITION NORMALE DU BORD TARSAL DANS LA TRICHIASIS DE LA PAUPIERE SUPÉRIEURE.

WIEDERHERSTELLUNG DER NORMALEN STELLUNG DES TARSALRANDES BEI TRICHIASIS DES OBEREN AUGENLIDES.

BY DR. F. C. HOTZ,
Chicago, Ill.

The free border of the upper eyelid forms with the tarsal conjunctiva nearly a right angle, and therefore stands also vertically upon the surface of the eyeball, and the eyelashes form with the free border an angle of at least 90 degrees. Owing to this position of the free border, the distance between the globe and the eyelashes is equal to the whole thickness (or height) of the lid border; and as long as this maintains its normal position the eyelashes may be considerably deflected from their normal directions before they come in contact with the cornea and cause any annoyance to the eye. In patients, for instance, who have suffered from ulcerative blepharitis we often find the growth of the eyelashes greatly disturbed, many being abnormally curved downward; but they seldom touch the cornea unless at the same time the lid border is more or less inverted. But when the free border has lost its upright position, becoming inclined or "inverted," the eyelashes brush over the cornea, even though the angle they describe with the free border is not changed, and thus prove that they grow out in perfectly normal order and direction. Their relatively faulty direction to the eyeball is not the result of faulty growth, but the unavoidable consequence of the inversion of the lid border, because by this inclination the ciliary edge, together with the eyelashes, is necessarily brought down in close proximity to the cornea.

And this is just the condition of the tarsal border in trichiasis as we commonly find it, after trachoma. By tissue waste and shrinkage on the conjunctival side of the lid the posterior (or conjunctival) edge of the border is retracted, and the latter itself is turned down, so much so, that in the worst forms of trichiasis its surface is pretty well on a level with the tarsal conjunctiva; and, coming in contact with the eyeball, and being constantly bathed with the lachrymal fluid, it becomes red and moist, like the conjunctiva. It really seems as if the free border had wasted away and the conjunctiva extended closely to the eyelashes. But on careful inspection we can convince ourselves at once that the free border has lost nothing of its breadth, but only its normal position; and we also find the eyelashes, though they may be scanty and ill-developed, depart very little, if at all, from their normal position in relation to the lid border; their faulty relation to the eyeball being the inevitable result of the inversion of the free border.

If we keep these facts before our eyes; if in this perverted position of the free border of the eyelid we recognize the chief anatomical feature of trichiasis, it should

Digitized by Google

certainly lead to the conclusion that in trying to remove the deformity of the eyelid we must direct our main efforts toward restoring the free border to its original position. If this can be accomplished the eye is not only relieved of the mechanical irritation by the cilia, but the eyelid recovers an absolutely normal appearance, and the result is far superior to that attained by operations which try to divert the eyelashes alone from the eyeball by transplantation.

Since the anomalous position of the free border has been brought about by the retraction of its posterior edges, the reposition might be attained either if the posterior edge be brought down again to the place it occupied before the retraction, or if the anterior (or ciliary) edge be set back just so much as the posterior edge has been retracted. In either case the free border would regain its upright position. But the first plan is impracticable, for we have no means to overcome permanently the retraction of the posterior edge. I could not before this assemblage enumerate the many unsuccessful attempts which have been made in this direction.

The other plan, however, is quite feasible, and I will now explain, by means of the accompanying diagrams, how the reposition of the tarsal border can be accomplished. Let Fig. 1 represent the section of the upper eyelid, with its free border (*c a*) inverted; take out the wedge-shaped piece *b a d*, so that the piece *b a c* can be turned on the point

Fig. 1.

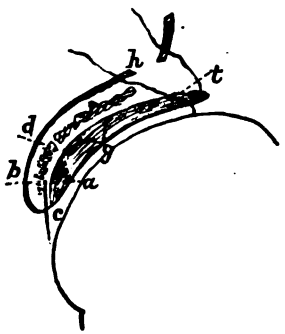
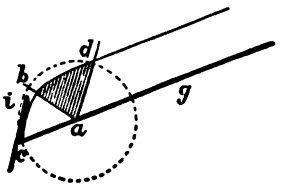


Fig. 2.



a until *b a* comes together with *d a*. What effect will this rotation have on the position of the free border and eyelashes? Fig. 2 will give the answer; the posterior edge (*a*) of the free border (*c a*) is the centre of a circle, consequently the lines *c a*, *b a* and *d a* are radii of this circle, and if the segment *b c* of this circle is equal to *b d*, the angle *c a b* is equal to *b c d;* and when, therefore, *b a* moves to *d a*, the line *c a* will make an equivalent rotation and come into the position before occupied by *b a*. In other words, the free border (*c a*) is turned up so that it forms again an almost right angle with the conjunctiva (*a g*); and as the eyelash (*i*) follows this upward rotation of the tarsal border without the angle *i c a* being changed, it is turned away from the eyeball and resumes a direction more or less parallel to the conjunctiva.

You will observe that the principal condition of success in this operation consists in making the posterior edge (*a*) the turning point, "the hinge," on which the free border is to be turned; and that, therefore, the wedge must be excised in such a way that its sharp edge is placed as nearly as possible to the junction (*a*) of the free border with the conjunctiva. This is the point which distinguishes this method from the grooving operations of Streatfield and Snellen. Their object was to straighten an assumed incurvation of the whole cartilage, and for this purpose they removed the wedge from the central portion of the lid. But if the removal is made at any other place than at *a;*

Digitized by Google

if, for instance, it be made at *g* (Fig. 1), it cannot rectify the position of the free border; the whole piece, *g a c*, would be turned and lifted away from the globe, only to sink down on it again, because it could not stay in the air, but has to rest on a solid foundation; and when the lid comes down again on the eyeball, the eyelashes will again brush over the cornea as before the grooving, because this had not altered the position of the free border of the eyelid.

But the reposition of the free border alone does not restore the perfectly natural appearance of the eyelid. For in trichiasis the fibres of the orbicularis muscle are crowding down against the eyelashes, and the skin has become more or less relaxed, so that it hangs heavily over the ciliary edge, making this look unnaturally thick, round and baggy. To make the effect of the operation perfect in every respect, it is necessary to remove this unsightly appearance of the lid. Skin and muscle must be slightly drawn upward and fastened to the upper border of the tarsal cartilage, to render their shifting down again toward the free border impossible.

For this reason I do not incise the skin near the free border where I propose to groove the cartilage, but make a transverse incision parallel and just a little below the upper border of the cartilage (at *h*, Fig. 1). A few strokes of the scalpel sever also the tarsal portion of the orbicularis muscle from its portio orbitalis, and expose the upper border of the cartilage with its aponeurotic fascia. A narrow strip (three to four millimetres wide) of the muscular fibres immediately below the upper tarsal border is excised; the rest of the orbicularis muscle, however, is left, but together with the skin dissected off from the external surface of the cartilage all the way down to the roots of the eyelashes. Skin and muscle of the lid are thus converted into a flap which is attached to the lid along the free border only. Turning this flap down upon the lower lid I hold it between the thumb and index finger of my left hand, pushing the top of the index finger up under the upper lid just beyond its free border. At the same time an assistant seizes the upper border of the cartilage with a forceps, and I make a transverse incision through the whole thickness of the cartilage, following closely the line of the roots of the cilia, and directing the course of the knife toward the posterior edge of the free border, so that if the incision was continued through the conjunctiva it would just sever the junction of the lid border with the conjunctiva. To avoid cutting through the conjunctiva I cut the cartilage on my index finger, believing its feeling gives me the best information about the thickness of the tissues lying between it and the knife; this feeling is the most accurate gauge by which I can tell when the incision is deep enough. This incision finished, a second one is made, from two to four millimetres above the first, in such a slanting direction through the cartilage that it meets the first incision just at the posterior edge of the free border, and if both these incisions are made with care and precision, the wedge of cartilage they inclose can be taken out in one piece. The size of the wedge varies in different cases; but as a general rule, it may be laid down that the wedge will always be sufficiently large if its base is about as wide as the distance between the first incision and the ciliary edge of the lid ($b\,d = b\,c$, Fig. 2). If the wedge does not come off in one piece the sides of the groove must be carefully trimmed to look smooth and even.

Though we should try to avoid cutting through the conjunctiva, this accident will occasionally happen even to the best and most painstaking operator. These small, accidental buttonholes in the tarsal conjunctiva neither delay the healing process nor vitiate the success of the operation.

The wound is finally closed by three or four sutures, which I apply in the following manner: After the needle has been passed through the cut edge of the tarsal skin I seize the upper border of the cartilage with a toothed forceps and transfix it with the needle so that it reappears through the tarso-orbital fascia just above the cartilage; and finally, I pass it through the upper cutaneous edge of the wound. The loops of these

Digitized by Google

sutures enclose only the cut edges of the skin and the upper border of the cartilage, and when they are tied the skin of the eyelid is drawn up to and united with the upper border of the cartilage. This traction removes the abnormal relaxation of the skin and also relieves the ciliary edge from the pressure of the muscular fibres, while at the same time it turns the free border up and brings the edges of the groove in the cartilage in close coaptation. But the sutures must not be tied before the bleeding has ceased, and the whole wound, particularly the groove in the cartilage, has been carefully cleansed from all coagula and thoroughly irrigated with the antiseptic lotion (sublimate or carbolic acid), which has been employed all through the operation.

Gentlemen, this operation may seem rather complicated and tedious, but I can assure you that it takes much less time to perform than to describe it, and the perfect results you attain by it will make you feel that your time was spent for a good purpose. If you only aim to relieve the eye from the injurious contact of the lashes, you may accomplish this object probably by simpler methods. But I put a higher standard upon plastic operations on any part of the face, and certainly the operations for trichiasis belong to this class. I am not satisfied that the operation merely relieves the injurious consequences of trichiasis without regard to the subsequent appearance of the eyelid; my ideal of a perfect operation for trichiasis is that operation which relieves the troublesome effects, and at the same time restores to the eyelid its normal beautiful contours and natural appearance. And this standard is attained by the operation I have the honor to present to your kind consideration.

DISCUSSION.

Dr. Dibble, of Kansas City, said: I usually modify the operation of Dr. Hotz, by dissecting the ciliary border. Here we have generally a degeneration, a bagginess, and by dissecting here we induce absorption, which leaves the ciliary border much thinner.

Dr. S. J. Jones, of Chicago, said, it is well known that nearly all of the many different operations that have been proposed from time to time to meet the varying requirements of these cases give temporary relief from this serious condition. It is, however, desirable to secure more lasting results, and time alone can test the value, in this respect, of any operation. Dr. Hotz had not made clear, in his statement, regarding the danger to the integrity of the eyelid which attends the dissection of so large a flap in the primary operation, nor the amount of the deformity of the parts which results from it, but he had stated that the operation may be repeated with safety on the same eye.

Dr. Jones said that he had subsequently seen several of the patients on whom Dr. Hotz had operated, in which the result had been much less satisfactory after the lapse of a period varying from a few months to one or more years, but he was not aware whether, in those cases, the operations had been performed in accordance with the modification which Dr. Hotz says he now practices and advocates.

Dr. J. L. Thompson, of Indianapolis, said: Greatly as we are indebted to Dr. Hotz for the very ingenious and beautiful operation which he has described, I am, after years of observation, fully convinced that a recurrence of the trouble frequently follows them all. Occasionally we do meet with cases where the tarsal margins are so uneven—here with hypertrophy, there with atrophy; nipple-like projections and attenuated bunches of ciliæ—that a combination of many methods has to be practiced, and, in spite of all these, a return takes place. And I am as positive of this as I am of the revolutions of the earth upon its axis, that there never has, and never will be, this side of the Lethean stream, any one operation suitable to all cases.

Digitized by Google

Dr. KEYSER said that he was in entire concordance with Dr. Thompson, namely, that there can be no specific method of operation. He has had excellent results in very severe cases by a method recommended some time ago by Prof. B. A. Pope, of New Orleans, in which he splits the lid and removes carefully the cartilage, thereby permitting the conjunctiva that has been contracted and drawn in such a manner as to curve inward, to straighten out and face down the length of the outer flap. A fold of the outer skin is drawn slightly up and held in position a few days, by either a plaster or sutures.

Dr. G. E. FROTHINGHAM, of Michigan, said: Notwithstanding the experience of the last speaker, he thought most ophthalmologists would feel obliged to resort to surgical interference in all aggravated cases of entropion. The question of great importance for them was to consider and determine what method of operation would give the best result in the majority of cases. He had been much interested in the mode of operating presented by Dr. Hotz, and had, for the first time, seen it properly presented. He had, for some time, made a similar operation, but he had made the first incision through the integument lower down than Dr. Hotz seems to have done, though, as he understood the operation from reports he had seen in the journals, he had supposed Dr. Hotz also recommended it. Few of these cases could be treated successfully without removing the wedge-shaped piece of the tarsal cartilage, and he had often removed, with advantage, some of the redundance of muscular tissue which had been drawn down to the free border of the lid by the long-continued blepharospasm. The incision through the skin recommended by Dr. Hotz has the advantage that it comes higher up on the lid, and when the eyes are open the scar would be largely concealed, and add much to the cosmetic value of the operation. Another point of especial importance is, that it allows us, without unduly everting the lid, to stitch the integument, when drawn up, to the underlying palpebral fascia, to which it will become attached during the process of cicatrization, and thus become fixed, and to a considerable degree resist the future action of the orbicularis to drag it down toward the free border of the lid. He had had experience that convinced him that an important advantage could often be gained in this way, and he considered it one of the most valuable features of Dr. Hotz's operation, which was the best method he knew of for the majority of cases, and one which reflected much credit upon the Doctor.

Dr. J. J. CHISOLM desired to know why Dr. Hotz now discarded the outcurving of the cartilage, which seemed to be the strong point in his operation for entropion, as published a few years since.

Dr. HOTZ.—I have not dropped my former method, as the chairman seems to think. I have retained the sutures, which are its chief feature; but I have added what I regard an important feature for the aggravated cases of trichiasis. What I have abandoned is the idea that the incurvation of the cartilage is the anatomical basis of trichiasis; and the change of my views on this point has led to the change in grooving the cartilage, which I demonstrated here.

Because in former years I followed Snellen's and Streatfield's precepts for grooving, I am not surprised to learn that some cases I had operated on proved to be failures; in speaking of the difference between Snellen's grooving and mine, I referred to this very point, showing why, after Snellen's operation, the recurrence of trichiasis must be expected. I myself saw such relapses, and spoke of the fact in a paper giving my experience of three years with my former operation; it was those failures which induced me to persevere in my endeavors to improve the method and to trace

Digitized by Google

the causes of these failures. Even after my present improved method, I shall look forward to learn of an occasional recurrence of trichiasis; for the eyes have not received from me an immunity from trachoma. And should they be seized again by this disease, it is clear that the causes which had once produced trichiasis caused it again. But should it recur, my operation can be repeated, as I said, "with impunity." I meant to say that "it can be repeated over and over on the same eyelid without mutilating it." In all my experience I have not observed any dangerous consequences connected with the operation or with its repetition.

Dr. Frothingham is mistaken if he thinks I had formerly made the incision lower down. In my second paper on entropion operation, I touched upon this very point, because I had found several surgeons insisting upon making the incision very low down, like in Arlt's operation. This is a grave mistake, because you never can cover the whole surface of the lid with so narrow a strip of skin, and, therefore, the sutures are placed in the middle of the lid, instead of the upper border. This mode will always lead to an unsatisfactory result; you must preserve the skin of the eyelid to cover it, for as soon as you drag down the heavy skin from above, you overload the lid, and its movements are embarrassed by the increased weight. Only when there is an actual superfluity of tarsal skin it may be trimmed down to the proper extent. I trust no one has received the impression that I regard my operation as infallible. What I contend, and what I have been striving for, is that it should be adopted in place of those barbarous methods which mutilate the eyelids, and are a disgrace to ophthalmic surgery.

TREATMENT OF ENTROPIUM.

TRAITEMENT DE L'ENTROPION.

BEHANDLUNG DES ENTROPIUMS.

BY BARTON PITTS, M. D.,

St. Josephs, Mo.

Since the introduction of the process of electrolysis for the permanent removal of cilia, operative procedure for the relief of entropium is by no means as frequently required as formerly. When there is a slight thickening of the lids with a tendency to turn inward, bringing a few eyelashes against the cornea, causing irritation and injury to the eyeball, some of the many cutting operations in vogue for entropium were formerly required, for the relief of the condition. I desire to call the attention of those interested in eye surgery to the method of electrolysis in the treatment of such conditions, and particularly to call attention to its usefulness, not only in the removal of the cilia, a most troublesome factor in these cases, but to its resolvent effect upon hypertrophied tissue, causing an absorption of the thickened lid edges, and thereby often overcoming the tendency of the lid to turn inward. Where there is incurvation of the tarsal cartilages, with a marked tendency of the lid to inversion, a cure could not, of course, be expected from the use of the electrolysis needle; but even in this condition great assistance may be obtained from using the needle before an operation, in the way of removing any greatly displaced eyelashes and reducing the hypertrophied

Digitized by Google

lid tissue; recourse may also be had to the process after an operation which would otherwise have been successful but for a few straggling hairs, thus simplifying and assisting much in the treatment of cases of entropium.

In the spasmodic or acute form of entropium, where there is simply folding in of the lid or lids as the result of spastic contraction of the orbicularis palpebrarum muscle, associated with no structural alteration in the tissues of the lid, nor disarrangement of the eyelashes, little or no operative interference is required. Removal of a foreign body, alleviation of photophobia by the instillation of a few drops of a 4 per cent. solution of cocaine or atropia, cure of corneal ulcer or crop of phlyctenulæ, or painting several coats of collodion on the surface of the lid, is often sufficient to overcome this form of entropium. In old age there is often present, owing to a redundancy and lax condition of the tissues of the lids, a persistent tendency of the lids, especially the lower, to roll in against the eyeball. For the relief of this condition the removal of a horizontal fold of skin tissue, of greater or less width as required, will usually be sufficient to overcome this malposition of the lid.

It is not, however, in these acute spasmodic displacements of the lids that entropium assumes its troublesome features. In chronic ophthalmia and granular lids of long standing, often hastened and aggravated by the injudicious use of irritating caustic substances, such as the commonly used blue stone, nitrate of silver, etc., the structure of the conjunctiva becomes almost or entirely destroyed, being replaced by cicatricial or scar tissue, which, upon contraction, usually results in incurvation of the tarsal cartilages, and inversion of the lid edges and cilia against the globe of the eye. In a number of these cases of entropium, as the result of long-standing irritation, there is a notched, ulcerated, thickened and contracted condition of the edges of the lids. The eyelashes are scattering, dwarfed, and displaced, often being brought in contact with the cornea, producing corneal roughness, with consequent great impairment of sight. If data could be obtained, it would, doubtless, show a larger number of cases of blindness after childhood, as the result of entropium, than from any other one cause. This condition of corneal involvement, while it is almost universally present in granular lids of long standing, results not so much from the inflammatory process of the granulations, as from the irritating effect of the displaced eyelashes. It becomes a point of importance, then, in the management and care of these cases, that such an important structure as the cornea be protected from injury to as great an extent as possible, and while the management and cure of granular ophthalmia is an admitted slow and not readily tractable affection, still, with the timely use of the efficient means of the electrolysis needle now attainable, for the permanent removal of the offending cilia, there need be very few cases of corneal impairment and destruction as the result of entropium. In many cases of destruction and absorption of the conjunctival tissue, the result of chronic ophthalmia and trachoma, with cicatricial tissue covering the inner surface of the lids, and bands of the same tissue extending from the lids to the eyeball, there will occasionally be observed not the least impairment of the cornea, leaving the individual useful eyesight after passing through the slow and usually destructive process of neglected granular ophthalmia. This immunity of the cornea from involvement in such cases is due to the fortunate escape of its surface from the grating, irritating effect of the cilia which happen, in such cases, not to be displaced against the eyeball. If, therefore, in cases of cilia displacement, timely recourse should be had to the electrolysis needle in the permanent removal of each displaced eyelash, the number of cases of corneal involvement, and consequent impairment of vision, would be greatly, if not entirely, prevented in all cases of entropium.

There are, however, aggravated conditions of entropium in which the displaced cilia are associated with a thickened, ulcerated and notched condition of the tarsal cartilages, which requires for its relief and restoration operative interference. In this condi-

Digitized by Google

tion of the lids, while the electrolysis needle cannot be depended upon for the cure of the entropium, the resolvent effect upon the hypertrophied lid tissue is desirable and beneficial in lessening and simplifying the operative procedure. In referring to the different entropium operations, usually named after the author suggesting the procedure, it is a matter of difficulty to give the preference to any one method except in special cases, as the conditions present may determine.

In an hypertrophied, thickened lid, with incurvation or doubling up of the tarsal cartilage, the operative procedure of Von Graefe will be found to afford relief,—the removal of a triangular portion of the skin of the eyelid down to the cartilage, the base of the triangle being parallel with the margin of the lid, reversing the process in the removal of a triangular wedge from the cartilage, cutting entirely through the thickness of the cartilage to the conjunctiva; uniting the edges of the cartilage in uniting the edges of the skin. The operation of canthoplasty being usually required primarily, in order to avoid too much shortening of the palpebral aperture. A simpler operation and one that is claimed by the author Von Burrow as affording success in over one thousand cases of entropium, consists in everting the lid and dividing the tarsal cartilage through its entire length, parallel with and a few lines above the edge of the lid, leaving the incision free; if there is not sufficient gaping and turning out of the lid edge, it may be increased by excising a strip of more or less width from the upper surface of the lid, uniting the edges of the skin incision by sutures.

In some conditions of entropium the operation of transplantation gives satisfactory results. The operation consists in slitting the lid along the edge for about five or six mm. deep, through the entire length, the upper portion containing the skin, hair bulbs, orbicularis muscle, etc. An incision is next made through the skin about five to six mm. above and parallel to the edge of the lid, reaching down to the first incision. Another incision is then made above this last, varying in width according to the degree of the effect desired; this riband of skin may be dissected up and removed—the Arlt-Jaesche operation—or it may be brought down and attached to the intermarginal edge, the band of integument containing the cilia being carried up and over the bridge of loose skin and stitched to the upper border of the highest incision—Deanoux's operation.

Dr. Stephens, in a late edition of the *Archives of Ophthalmology*, proposes the insertion, after slitting open the lid edge, of a piece of transplanted lid tissue, which, upon union, is claimed to so stiffen the lid tissues and elevate the eyelashes as to overcome the entropium; further experimentation will be necessary to establish its usefulness. As to the adoption of the method of transplantation, Von Graefe's operation of removal of a portion of the lid tissue or Burrow's operation of dividing the cartilage, will depend upon the conditions present. The operation of Von Burrow is simple and has given some brilliant results, and is worthy of more universal trial.

My desire, however, is not so much to call attention to the features of the different operations for the relief of entropium, as to the simplifying and ameliorating effect of the process of electrolysis in permanently removing any displaced cilia, and in the further effect of reducing the thickened, hypertrophied tissue usually present, thus relieving the cornea from the presence of the irritation of the displaced eyelashes and in reducing the hypertrophied lid tissue, overcoming often the tendency to entropium; such treatment, in a great number of cases, will afford entire relief.

The process of using the electrolysis needle in destroying the hair bulbs and in reducing the hypertrophied tissue, in the treatment of entropium, needs no special reference in this connection, further than to call attention to the thoroughness and frequency with which the application should be made.

Digitized by Google

THE OPERATION FOR STRABISMUS.

L'OPÉRATION DU STRABISME.

ÜBER DIE SCHIELOPERATION.

BY DR. EDMUND LANDOLT,

Paris.

The operation for strabismus has marked peculiarities which distinguish it from all other surgical procedures performed upon the eye. All other operations, however much they may differ among themselves, have this in common, that they concern one eye only. The operation for strabismus, on the other hand, is, so to speak, *binocular*. It always involves both eyes, and this for the simple reason that strabismus is a binocular affection, a defect in the relative direction of the two eyes. The cyclope does not squint; he who has only one eye has no cause to fear a strabotomy.

This observation might appear self-evident. It seems to me, however, that we are far from taking it sufficiently into account in our daily practice. We may operate for cataract upon an eye without paying much attention to the other, the first day we see the patient, provided the cataract is mature and uncomplicated. Strabismus, on the contrary, should not be attacked surgically before the relationship of the two eyes, as regards direction and mobility, as well as the movements of each eye separately, have been thoroughly ascertained. The neglect of this principle is the chief cause of the failures and imperfect results of our strabismus operations, while to take it into account will increase considerably and confirm their success.

To make myself clear: In the great majority of cases strabismus occurs in the horizontal plane, it is either convergent or divergent. Now, there exist most potent functional concomitants tending to increase the convergence, others which induce the eyes to diverge. He who is aware of these factors and knows how to profit by them, makes them his most precious auxiliaries in the surgical treatment of strabismus; he who neglects them will have them as antagonists. They will often deprive him of the fruits of his operation, however well performed.

We all know the causes which produce *convergence:* the desire to see distinctly a near object, the stimulus to accommodation, the excitement of the ciliary muscle. Conversely, the lack of an object of fixation, the repose of the eyes, absence of light, paralysis of accommodation, are conditions which diminish convergence and favor *divergence.*

But above all these factors, the one which tends to do away with strabismus, of whatever kind, our most powerful aid in the cure of squint, is *the desire to see binocularly.* If binocular vision is wanting, sometimes in consequence of strabismus come on at an age when it was not yet firmly established, its absence becomes more frequently still the principal cause of the deviation of an eye and of its persistence in squinting.

Consequently, everything which tends to the reëstablishment of binocular vision helps us toward our goal, the cure of strabismus, and, let me add, to its complete, its ideal cure. For without binocular vision we gain for our patient only an apparent cure, we obtain merely a cosmetic effect. To have attained this is, of course, not to be despised; it is, frequently enough, all we can hope for; but this cure, in addition to being incomplete and existing almost always for one distance of fixation only, is often also limited in its duration, because it lacks the principal element which maintains the eyes in their normal direction.

Now, what are the conditions which favor binocular vision? Equal vision in the two eyes and their harmonious working. To correct the optic defects in the weaker eye, to equalize the refraction more or less in anisometropia, visual exercises of the

Digitized by Google

amblyopic eye and orthoptic exercises of both eyes together,* will, therefore, render us the greatest service in the surgical cure of strabismus.

I beg you, gentlemen, to believe that these simple considerations, which have probably occurred to all of us, are no mere theory. I would not dare to present myself before you so poorly armed. No, I wish to lay facts before you, and what I just said is as much the result of experience as of reflection.

When I have to deal with a squint, I proceed in this manner: After having determined its *nature* (paralytic or concomitant), I measure its *degree* † and the *mobility* of the eyes (field of fixation) at least in the plane of deviation. I take into consideration the duration of the affection, the patient's age, his amplitude of accommodation as well as his refraction, and his acuteness of vision.

I then try how far binocular vision has been lost, if it is possible to produce diplopia or binocular vision by means of a prism or of my stereoscope. If the patient is young, I set myself, in the first place, to use exhaustively all the means which tend to diminish the strabismus before proceeding to an operation.

In *convergent squint* I order the cessation of all ocular work, I atropinize both eyes, furnish them with convex lenses, correcting the total hyperopia, and institute stereoscopic exercises as far as possible.

Our non-surgical means of removing *divergent squint* are less numerous and less efficient, except when the condition is due to a general or local muscular weakness, which can be relieved by a tonic treatment. In this case, orthoptic exercises, especially stereoscopic, can render us good service, whereas we cannot count upon the most natural and most energetic factor which combats this form of strabismus, namely, the excitation of convergence. The latter is, in fact, nearly constantly invoked, that is to say, every time one fixes a near object. If, in spite of this, he has developed a divergent strabismus, this would indicate that the equilibrium of his ocular muscles is altered (through their insertion, their relative power, the form of the globe, or through some other cause), to such an extent that orthoptic exercises have scarcely any chance to reproduce the normal conditions. We shall see that they assume an importance so much the greater when once the operation shall have given them an opportunity of action.

Be that as it may, I never operate until I have satisfied myself that the non-surgical methods have yielded their full result. It is upon this degree of strabismus, often considerably less than the one found at the commencement of treatment, that I base the selection of the method of operating and the extent to which I will carry it. It is evident that this is especially important in a case of convergent strabismus in a young person, because we know that this strabismus is frequently spastic, associated with an excessive contraction of the ciliary muscle, that it often disappears spontaneously as years advance, and that it nearly always diminishes under an appropriate treatment. Now, if a child be brought to us with a convergent strabismus of, say 30°, and we at once correct it by operation, it is certain that, sooner or later (perhaps within six months), a divergent squint will develop, a squint for which we are to blame, and which will become more extensive, more disfiguring, more troublesome and more difficult to correct than the original deformity. We, therefore, begin with placing the child under a course of treatment such as we have just sketched, and continue it until no further improvement can be obtained. A strabismus of 30° may thus easily be reduced to 0° or to 10°. Suppose that in this case a squint of 10° remains. This is still a very apparent strabismus. The child's parents may say that the treatment has been of no use, and the surgeon, unless he employs an accurate method for measuring the

* Exercises with a colored glass, with a distant flame and prisms, stereoscopical exercises, experiments with Hering's test, etc.

† Its *angular* amount, of course, the linear measurement being as illogical as imperfect.

Digitized by Google

degree, is unable to contradict them. But we would felicitate ourselves upon the ten degrees gained and the danger of hyper-correction avoided.

We now may proceed to the operation without fear. We have to correct 20° of convergence. Is it possible to operate with such precision that we can say at once that the correction of 20° is assured? No. Our operative procedures, with whatever skill performed, are so coarse in comparison with the delicate action of the ocular muscles, that he who relies exclusively upon the operation would incur almost certain failure by either exceeding or falling short of the aim before him. He can only say in a general way, "In this case tenotomy is indicated, in that, advancement; in another, the combination of both is preferable—here a stronger, there a lesser effect is required."

If it is necessary to operate upon both eyes, to perform, for instance, the tenotomy of two homonymous muscles, we never do both at the same sitting. We, on the contrary, find a great advantage in basing the second operation upon the definitive result obtained by the first. But although we cannot count upon obtaining, by the operation alone, the precise correction of the error, still we possess in the orthoptic methods the means of perfecting the result of our surgical intervention.

Now, since it is easier, as a rule, to diminish than to increase the immediate effect of a strabismus operation, I am not afraid of operating rather freely. Let us return to our example. We have 20° of convergence to correct by an operation. I know by experience that such a strabismus could not be removed by a simple tenotomy, and if I attempt to force the effect, by conjunctival or capsular sutures, beyond a certain degree. I diminish the power of the internal rectus (and even, as we shall see, of the external rectus also) by the exaggerated setting back. I must, therefore, choose between tenotomy of both internal recti and the combination of tenotomy of the internal with advancement of the external rectus in the same eye. I prefer the first, if the strabismus is alternating, but where it is monocular, I should prefer the second. The latter would occur much more frequently. I would perform a moderate tenotomy and advance the antagonist to about one millimetre from the cornea, according to the lines laid down in a former paper.* I would continue instilling atropine into each eye, and protect both eyes with an antiseptic dressing. When the patient has recovered complete consciousness (if an anæsthetic has been used), I should probably find a certain degree of divergence, the amount of which I would measure either with the perimeter or by my subjective method of strabometry.† It is, then, simply a question of gradually diminishing this excessive result to the desired degree. Nothing easier to the skilled observer.

In the first place, he will, of course, not have committed an error so great as to necessitate the advancement of the internal rectus originally set back. But even if this should happen, it would not be a great disaster. Otherwise the eye should not be touched, except to change the dressing, until the muscles begin to be attached to the globe. If the divergence is rather marked, the atropine should be stopped. If it persist the third day, the stitches of the advanced muscle should be removed, but both eyes should still be covered. But if, on the fourth day, the eye still diverges, the good eye should be uncovered. The use of this eye invokes convergence which helps to do away with the hyper-correction obtained. One can commence even on the fourth or fifth day, systematic exercises of convergence, by desiring the patient to fix his finger at a distance and gradually bring it nearer. The operated eye, now free from stitches,

* Landolt. Art. Strabismus in "Dictionnaire encyclopédique des Sciences médicales," Paris, p. 279. In higher degrees of squint, I would bring it right to the corneal border. See Landolt, in de Wecker et Landolt's "Traité complet d'ophthalmologie," III, s. 858.

† Landolt. "Annales d'Oculistique," Juillet, 1875, and "The Diagnosis of Diseases of the Eyes."

Digitized by Google

follows this movement under the bandage. Exercises which require the patient to look at the side opposite from the operated eye, are useful adjuncts. It is very rare not to have, by these means, the excess removed.

Having obtained this correction we now proceed to the establishment of what I have called the *amplitude of convergence*,* that is to say, the faculty of converging and diverging. Indeed, in no case should the patient have lost the latter, unless a relapse of convergent strabismus could occur. If, therefore, he can fix binocularly a candle flame placed at a great distance, he should still be able to see it single even through an abducting prism of, say 4°.

It is, of course, not less important to exercise his power of convergence proper. For this purpose we may employ simply a sheet of white paper with a black spot on it, or if the vision of one eye is very defective, the luminous slit of my dynamometer,† the other eye being provided with a colored glass. The object selected is then gradually brought nearer to the patient, who tries to maintain a single and binocular vision. It is wonderful to what an extent the amplitude of convergence can be increased by this means. It is self-evident that stereoscopic exercises are also of great use in the establishment and confirmation of binocular vision.

Even if binocular vision should not be obtained, these exercises, by exciting accommodation, have an important influence in lessening the divergence. Should, however, divergence remain in spite of all our efforts, we must have recourse to the advancement of the tenotomized muscle. This can be easily done up to eight or ten days after the primary operation. A strabismus hook is introduced through the original wound in the conjunctiva and passed under the muscle. A stitch is inserted in the tendon, which is then separated from the globe by drawing the hook gradually forward. By means of the stitch, the insertion of the muscle is drawn to the cornea as far as is thought necessary. The pain and hemorrhage produced by this little operation quickly subside under the use of an iced compress.

This is, in outline, our method of procedure in convergent strabismus. DIVERGENT STRABISMUS offers, as you are aware, a much greater resistance to all forms of treatment. Here the danger is rather to fall short of the required correction than to pass it.

With regard to the method of operation, it is not enough to know the degree of divergence; it is essential to ascertain, as exactly as in convergent strabismus, the range-action of the abducting and adducting muscles. The examination of the field of fixation will often show us a marked limitation toward the nasal side, not only of the deviated eye, but also of the other.‡ This should not surprise us. Binocular vision quickly disappearing under the influence of divergent strabismus, the internal recti are little exercised by lateral movements, and to a still less extent by their simultaneous contraction in convergence, which, in the normal condition, gives their preponderance over the abductors.

It is only when divergent strabismus is of slight amount (l. g. 10°) and the field of fixation of each eye extends inward to at least 47°, that one tenotomy will suffice to correct. When there is no great difference in the vision of the two eyes, and when the muscular conditions of the two are good, one can easily correct, by a second tenotomy, later, on the other eye, any deviation which may remain from the first.

If, on the contrary, as is frequently found, the divergent strabismus is of long

---

*Landolt, in de Wecker et Landolt, "Traité complet d'Ophth.," III, 185, and Landolt, "Refraction and Accommodation," p, 190.

† Landolt, loc. cit. III, 279; p. 283: and *Proceedings* of the Heidelberg ophth. Soc., 1885.

‡ A similar fact is observed in convergent squint, where, very frequently, both fields of fixation are considerably limited on their temporal side.

Digitized by Google

standing and high degree, if the squinting eye is very amblyopic and its internal rectus acts feebly, then the combination of a free tenotomy with a complete advancement is absolutely demanded. It will often even be necessary to operate also on the other eye (either by tenotomy alone, or combined with advancement), in order to complete the correction. But before doing this, we should have recourse to all means favoring convergence.

We should not instill atropine. We should dress only one eye, in the case of a tenotomy, and, where an advancement has been performed, leave off the dressing from the good eye as soon as union has taken place. Exercises must be begun immediately, which have a tendency to turn the eye in the required direction; those which induce convergence being the most important.

It is scarcely necessary to say that, if we shall have overshot the mark, we must employ exactly opposite treatment. (Paralysis of accommodation, prolonged dressing of both eyes, exercises with abducting prisms and stereoscope, in extreme cases, even advancement of the tenotomized muscle, but, of course, short of its original insertion.)

A peculiar interest attaches to a form of muscular anomaly which is not yet a divergent strabismus, but which, notwithstanding, enters into that category, because it often forms its initial stage. I refer to *insufficiency of the internal recti*, or, more correctly, *insufficiency of convergence*. I have treated this subject elsewhere* and, therefore, content myself with merely insisting upon the following points: The greatest prudence must guide us in the surgical treatment of this affection; we must not undertake it until all other treatment, especially general tonic treatment, has failed; our procedure must be based upon an intimate study of the muscular conditions, especially the amplitude of convergence; and frequently simple advancement is to be preferred to tenotomy.

It seems to me that we do not thoroughly appreciate the action of these two operations. We too often content ourselves in giving the preference to tenotomy because it is the easier of the two, and because it can be made to yield very great effects, thanks to conjunctival and capsular sutures, and other similar procedures; whereas advancement seems to us more complicated and even dangerous. More complicated it is, certainly, without, however, being very complicated. But, as regards danger, I see none, except the production of a vertical relative displacement between the two eyes. This should, however, not occur if the operation has been done skillfully and carefully. In cases in which it would be especially troublesome, that is to say, where binocular vision exists, nature comes all the more powerfully to our aid, tending to restore to the eyes their proper direction, provided we know how to follow her suggestions.

To arrive at a proper appreciation of the influence which an operation exerts upon the direction and especially, the mobility of the eyes, we must abandon those erroneous conceptions contained in certain standard—we may even say classical—memoirs upon this subject. In tracing, for instance, an eye diagrammatically, on paper, giving it a centre of motion and applying the parallelogram of forces, we are very liable to fall into the gravest errors. The movements of the eyes are far more complicated than this; they are even *too* complicated to be predicted by the deduction. We must proceed by *induction*.

Let us first make the experiment and then try to explain it with our anatomical knowledge—if possible; if not, we will have to extend it. In the question before us, let us measure the excursions of the eye as well as the amplitude of convergence, before and after operation. When we have collected a sufficient number of cases, we are then in a position to arrive at conclusions about the action of the operation.

Now, experience has shown us that, *ceteris paribus*, the advancement of a muscle has an action more powerful and more favorable than tenotomy of its antagonist, and

* Landolt. *Proceedings* of the Heidelberg Ophth. Soc., 1885; and *Ophth. Review*, v, 185.

Digitized by Google

that the combination of advancement with tenotomy in the squinting eye is preferable to tenotomy practiced upon both.

Muscular advancement, far from limiting the field of fixation, increases it, whereas a simple tenotomy diminishes it, and tenotomy exaggerated by sutures may limit it in a very disastrous manner. This is easily understood. If, by a forcible rotation of the eyeball away from the divided tendon, I force the latter to take an insertion far behind its original one, I weaken its action so that objects situated at that side are seen double. At the same time, the field of fixation is frequently found to be limited on the opposite side also. Krenchel's theory* is thus demonstrated to be correct. This muscle has, so to speak, also undergone a setting back. It is relaxed, its elasticity and contractility, though undiminished, have less effect upon the globe. The eyeball has, as it were, partially escaped from the grasp of the two muscles by advancing in front of them. The muscles are not wrapped so far around the eyeball, and as it is during their unwrapping that the muscles act at their full mechanical advantage, their power is consequently diminished.

To enter more into these considerations would lead us too far. Let me finish my communication by reviewing it in a few sentences: The operation for strabismus ought not to be considered alone a cure, but only as one step in a systematic orthoptic treatment which oftentimes has to precede, and always to follow, the operation. The orthoptic treatment will regulate and render more perfect the effect of the operation; it will oftentimes restore binocular vision in cases where this has been considered impossible, and will thus realize the ideal cure of strabismus.

As regards the method of operation, I believe that muscular advancement has, in general, a great advantage over tenotomy, and should be oftener practiced than it is generally done.

SOME IMPORTANT PROBLEMS RESPECTING INSUFFICIENCY OF OCULAR MUSCLES.

PROBLÈMES IMPORTANTS CONCERNANT L'INSUFFISANCE DES MUSCLES OCULAIRES.

ÜBER EINIGE WICHTIGE PROBLEME BEI INSUFFICIENZ DER AUGENMUSKELN.

BY GEORGE T. STEVENS, M.D., PH.D.,

New York.

Practical advances in the knowledge of the anomalies of the directing ocular muscles have by no means kept pace with those which have been made in the understanding of the anomalies of refraction and accommodation. Less crude, indeed, are the prevailing methods of dealing with these muscular defects than were the older methods of correcting near and old sight. Yet much is to be done in this department to bring it abreast the science of refraction and accommodation.

Graefe and his co-workers, as the great pioneers in this branch of ophthalmology, taught the important doctrine of muscular asthenopia, locating the trouble in the insuffi-

* Krenchel. *Arch. f. Ophth.*, XIX, 2; 275.

Digitized by Google

cient force of the interni to overcome the resistance of the externi when the eyes were employed at near range, and principally in cases of extreme myopia.

Certain methods for the discovery of this condition were almost universally adopted, chief among which was that of the dot and line, or method No. 3 of Graefe—a method which, notwithstanding the introduction of others by distinguished observers more recently, holds a preëminent place to the present time in text-books and, in great measure, in the special literature of the subject. Even the more recent methods are, for the most part, directed to the determination of the ability of the interni to overcome the resistance of their direct antagonists.

The question of muscular irregularities then, excluding strabismus and the results of paresis, has been, and still remains, in the great mass of the literature almost exclusively one of "insufficiency of the interni" at the reading-point. It is true that the literature is not destitute of recognition of other so-called "insufficiencies," and I believe that it was my own privilege to first call attention to important results following the operative correction of "insufficiency of the externi."

If it were true that difficulty in overcoming the externi in convergence were the great, if not the exclusive, difficulty which might arise from the absence of muscular equilibrium, not regarding strabismus or effects of paralysis, the methods of Graefe, or those more recently introduced for the determination of convergence power, might doubtless be, in a certain proportion of cases, satisfactory. Unfortunately, "insufficiency of the interni" is not the exclusive anomaly of its class; it is not even the one of the highest importance nor of the most frequent occurrence. Unfortunately, too, for even this single anomaly, the methods referred to are inexact and very frequently misleading in their results. I have elsewhere called attention to this important fact, but will, in order to emphasize the truth of the statement, introduce one or two illustrations.

Making due allowance for the "muscular mesoroptea," the ocular muscles are, when the eyes are directed straight forward at a point about twenty feet in advance of them, and in the same horizontal plane, in the state of minimum innervation. It must be evident that it is at this point that an actual "insufficiency," or, more properly, an anomalous tendency of the ocular muscles must be most truly shown.

Now, it is not an uncommon circumstance to find conditions not unlike the following, which is taken from an actual record:—

At a distance of twenty feet, with a prism, its base down before the right eye, there is shown esophoria (insufficiency of the externi) of 8°. In other words, there is a tendency on the part of the visual axes, when not directed by an effort of the will, to deviate inward 8°. At the distance of one-half metre, employing the method of Graefe, there is "insufficiency of the interni" of 10°, with a power of adduction of 50°. It is apparent that both "insufficiency of the interni" and "insufficiency of the externi" do not exist to the extent indicated by these results, for at the distance of one-half metre, even if no adducting force were employed, there should be little or no "insufficiency of the interni." With this real tendency on the part of the visual lines to deviate inward 8° these lines should meet at a little more than one-half metre in front of the eyes; hence, without any unusual innervation of the interni, there should be no important insufficiency of these muscles, much less an insufficiency of 10°. Still further, even if any exertion of the interni should, in this case, be demanded, the adducting force of 50° prism is an assurance of abundant energy on the part of the converging muscles.

There is, then, in this instance, associated with actual insufficiency of the externi, an apparent insufficiency of the interni of a high degree, when tested by the method of Graefe. Evidently, an operation for relaxation of an externus in such a case would be a grave mistake, and probably the employment of prisms for the near point would prove unsatisfactory and of no permanent value.

Digitized by Google

Lest it might be supposed that the influence of a prism in making the examinations in the above case should account for the contradictory results obtained, I will present another illustration in which the influence of a prism is entirely eliminated. A gentleman of thirty years of age, with no important refractive error, when looking at a lighted candle on a level with his eyes, and at a distance of twenty feet, has homonymous diplopia of 4°, with the two images approximately in the same horizontal plane. If, on the other hand, he directs the eyes to the flame of the candle at a distance of only two feet, he has no longer homonymous, but crossed diplopia of 5°. His actual convergence power, according to the method of Landolt, is one-half metre angle. Here, then, with no extraneous disturbing influence, we have both slight converging and diverging strabismus.

The above illustrations represent only a single class of anomalous results which are frequently encountered by the observer who investigates these deviating tendencies, not by a single method, but under a variety of circumstances.

How can we reconcile these contradictions, and how are we to proceed for the relief of the nervous irritations to which they give rise? The question is too broad to admit of a single reply, and the purpose of this paper is more especially to suggest the problems rather than to offer a solution of them. It will, however, be proper to consider in this relation an important element which is doubtless the main factor in many such cases. It is that condition of the ocular muscles in which there is a tendency on the part of the visual line of one eye to rise above the direction of that of the other eye. This important condition cannot be properly called an "insufficiency" of one or other superior or inferior rectus muscle, for it may be the result of a failure of any one of the four superior and inferior recti, or it may not be from failure of either of these. Hence, the term "insufficiency" is even less appropriate than in the conditions more commonly described as such. To this condition I have applied the term hyperphoria*, and shall employ it in this paper.

This condition, hyperphoria, frequently lateral in part or entirely, is doubtless the most important disturbing element in these contradictory conditions of the lateral muscles, and is, at times, an exceedingly troublesome factor, not only in disguising or complicating other conditions, but in presenting serious obstacles in the way of treatment.

If it is asked, why should this peculiar condition give rise to so many unlooked-for complications? a ready answer might be found in the assertion that the oblique muscles, acting in a somewhat complicated manner, must account for the anomalous results found. Science which satisfies itself with such easy answers is usually mistaken science. To give a reply to the question which shall be of practical value, we must present a reason which will enable us in advance to determine what would be the result of certain anomalous tensions of certain specified muscles. Failing of this, the oracular announcement that such and such irregular phenomena must result from the irregular tension of a certain class of muscles, only because the actions of these muscles are less perfectly understood than that of others, is simply evading a reply.

My own experience leads me to suppose that the irregular results found in hyperphoria are not attributable primarily to undue tension of the oblique muscles more frequently than to that of the recti. Nevertheless, much has still to be observed in this direction.

With this suggestion regarding the influence of hyperphoria as a disturbing influence, I shall further illustrate the perplexing phases of some of these anomalous tendencies by citing one or two somewhat remarkable complications of the muscular conditions.

These illustrations will be taken from cases in which the deviating tendencies

* *Archives d' Ophthalmologie*, Nov.–Dec., 1886.

Digitized by Google

amount to slight actual strabismus, because these exaggerated cases of insufficiencies can be better studied, or, at least, better described, than those of more moderate anomalous tendency.

A lady, aged about forty years, has in the right eye myopia ¾ D., in left eye, hyperopic astigmatism 1.00 D. These refractive conditions are verified by the employment of atropine. With or without correcting glasses, the following phenomena were observed in respect to the directions of the visual axes, the lady having, after considerable practice, acquired the ability to direct her attention to the image of one or other eye at will. She has habitual diplopia, of which she was unconscious until her attention was directed to it in the consultation room. The double images were very nearly in the same horizontal plane. If the lady covers the right eye for a moment, allowing the left eye to fix an object at twenty feet distance, upon removing the cover there is homonymous diplopia of from 10° to 15°, with a tendency of the right visual line above the other of from 1° to 2°. If, now, the left eye be covered, permitting the right eye to fix the distant object, on removing the cover from the left eye, *crossed* diplopia with slight right hyperphoria, results. This condition continues until, by an effort of the will, the lady fixes the object with the opposite eye, a change which she can effect at pleasure. After both eyes have been for some time uncovered, the hyperphoria disappears and a prism of a single degree, with its base up or down, throws one of the images out of the horizontal plane.

In the above illustration the condition of hyperphoria is probably causative of the remarkable phenomena of double vision described, a presumption strengthened by experience in other similar cases—for this instance is by no means unique.

It is interesting to note the fact that in this instance the hyperphoria, which is usually latent while the two eyes are directed toward the object, becomes manifest, to a certain degree, when one of the eyes has been for a short time covered. It is not always that hyperphoria becomes so readily manifest, and the greatest patience and much skill may be required for its detection.

This suggests the subject of latent anomalous tendencies of the ocular muscles. These latent anomalous tendencies are often of great importance and may be easily overlooked. Indeed, latent "insufficiencies" are as frequent as latent refractive troubles and are much more difficult to discover. A single example will illustrate this principle: A young gentleman had manifest esophoria (insufficiency of the externi) 4°, with abducting power of 4°. Partial tenotomy of one of the interni was made, and immediately after the operation the following conditions were found: The power of abduction had been increased from 4° to 11°, showing that an important relaxation of the tension of the interni had been effected. At the same time the insufficiency of the externi, which had before the operation been but 4°, was now 8°. Thus, the manifest insufficiency immediately after the operation was double that shown just before it, notwithstanding a positive relaxation of 7°.

This example, which is but a fair representative of conditions which may be frequently observed both after operations and after the use of prisms for a few days, shows us that we are to direct our efforts to reveal the whole of these tendencies, not contenting ourselves with the results of a single examination. Indeed, one of the great problems in respect to the anomalies of the ocular muscles is that of revealing latent heterophoria, or faulty muscular tendencies. In hyperopia we may, by means of the influence of atropine upon the ciliary muscle, render latent hyperopia manifest. It is equally desirable that some means be found of determining absolute heterophoria. No greater service could be rendered in the science of ophthalmology than in pointing out a method of revealing absolute heterophoria as readily and as effectually as we now reveal absolute hyperopia.

It is not my purpose to call attention to all the difficulties which may arise in the

Digitized by Google

course of a series of examinations of the eye muscles, for that would be a vast subject. The few illustrative examples which I have given suggest, however, several important problems:—

1. In what direction are we to look for the complicating causes in such apparently inconsistent conditions as those shown in the examples?

I have already intimated that the existence of hyperphoria will in many cases account for these apparently contradictory results, but it remains to discover in what proportion of cases it is thus influential, and it remains for us to inquire, if other causes exist, in what direction are we to seek for them.

2. In case the existence of hyperphoria is ascertained, in what manner are we to determine through the influence of what especial muscle or muscles the equilibrium is lost?

This is a question of prime importance, for it may be to little purpose that we are able to attribute the tendency on the part of the visual lines to deviate inward or outward, according to the distance for which the eyes are adjusted to hyperphoria, unless we are able to deduce from this conclusion some practical method of dealing with the faulty condition. It is, however, a legitimate question whether in case of hyperphoria it is always essential, in order to restore the equilibrium, that we abase the higher-tending visual line or exalt that which tends below. My own experience leads me to think that in a certain proportion of cases it is a matter of comparative indifference which course is taken, but that in another proportion of cases the selection of the exact method for restoring equilibrium is of essential importance.

3. Allusion has already been made to the problem of determining the absolute anomaly in muscular equilibrium.

4. The question of the manner of procedure in making examinations for heterophoria is an important one. In some recent publications I have indicated what, in the main, I regard as suitable procedures, and I need now only refer to my present method of arriving at a knowledge of absolute heterophoria.

It is my custom, more especially in cases of hyperphoria, to supply for temporary use correcting prisms of a degree rather less than the manifest heterophoria. Thus, if heterophoria of 3° should be found, a prism of 1° for each eye would be given. A day or two, or longer, after the examination is repeated, and if the heterophoria manifest has increased still stronger prisms are given, still avoiding a full correction. When, after several examinations at intervals of some days, no further difficulty is revealed, it is assumed that the whole, or nearly the whole, of the fault is revealed, and the question of operative relief may then be settled; for I do not think that we can hope for much permanent relief to heterophoria from the use of prisms in the great majority of cases.

In order to render this method of rendering latent heterophoria manifest as expeditious and as little troublesome as possible, I have had made for my own use a considerable number of spectacle frames, all taking glasses of the same dimension. I have then a supply of plane and of weak prismatic glasses which can be quickly adjusted in these frames.

The patient is supplied with one of these frames and the proper glasses, and when at a subsequent visit it may be necessary to supply a stronger glass, the change can be effected in a moment. When the hyperphoria has been revealed to the full extent which is practical, the frames are returned and are ready for another case. Thus far I find this the most advantageous method that I have employed, but I look forward to a quicker and more effectual method.

Digitized by Google

ADVANTAGE OF OPERATING EARLY FOR STRABISMUS.

L'AVANTAGE D'UNE OPÉRATION PRÉALABLE POUR LE STRABISME.

DER VORTHEIL EINER FRÜHEN OPERATION BEI STRABISMUS.

BY J. F. FULTON, M.D., PH.D.,
Of St. Paul, Minnesota.

There is no operation the ophthalmic surgeon feels more certain of success in, nor which his patient usually desires more, than the operation for strabismus. We, as a rule, are safe in promising to overcome the deformity of all who come to us for that purpose, provided they permit us to operate as often as necessary. But there are some other troubles connected with this deformity which are not so easily overcome, namely, diplopia and the amblyopia so frequently met with in these cases, and it is to these two symptoms that the ophthalmic surgeon should direct his attention chiefly in the treatment of this class of cases.

I agree with Sœlberg-Wells that the active suppression of the pseudo-image results frequently in amblyopia, and this amblyopia increases rapidly in children shortly after the strabismus first shows itself. In this connection he says: "The operation should never be unnecessarily deferred. The question is often debated, as to whether a child of two or three years of age should be operated upon for squint, or whether it is not better to postpone an operation until it is much older. My opinion is very strongly opposed to the latter practice, and is urgently in favor of the operation being performed as soon as possible, while binocular vision still exists and the sight of the squinting eye is good. When it is, however, absolutely necessary to postpone the operation, the vision of the squinting eye should be frequently practiced, and each eye apparently used for reading, etc.

Stellwag, although strongly opposed to operating before puberty, urges the necessity or importance of separate exercise for the squinting eye, in order, he says, to avoid the development of a central anæsthesia. For this purpose, he says, it is necessary to exclude from the visual act the eye usually employed for fixation, and to exercise the defective eye several times daily. I think this whole subject can be made clear by recognizing the fact, which clinical and careful investigation seems to distinctly prove, that amblyopia met with in cases of squinting is either primary or secondary. What I mean by primary amblyopia is that which is due to some defect either in the structure of the eye or in its perceptive powers, and is always congenital. For this there can be nothing done in the way of operating or treating the strabismus, but I am very positive in my opinion that a large proportion, at least, of the cases of amblyopia thus met with is secondary to and the result of the strabismus. It is one of the laws of nature, to which there are certainly but few exceptions, that suppression of activity will result in deterioration of the function of the part. It is so here. The objects not being focused on the same place on either retina results in a double image, and finally in a suppression of the image in one eye and the disuse of that organ, which gradually, in some cases very rapidly, loses its functional activity. As proof of this, how rare it is to meet with amblyopia in alternating squint, or in those cases in which the refraction and acuteness of vision are alike in the two eyes. In each it is a mere matter of accident which eye will be directed toward the object and which toward the nose, the reason of this being that both eyes are exercised alike. But in the other class of cases, in which the refraction of the eyes is different, the vision of the best eye is used by instinct, and the squinting is always done by the defective eye. The squint is then said to be fixed, and under such circumstances the vision of the squinting eye will usually undergo a steady, progressive deterioration. (Carter.)

Digitized by Google

And I maintain that this progressive deterioration of vision can be prevented in a great many cases by curing the squint, either by the use of glasses, or, if necessary, by an early operation. In this connection, and in proof of what I have just affirmed, permit me to refer to the history of a few cases:—

A young man, eighteen years of age, came under my treatment some years ago, with the following history: He had been examined eight years previous to this by an oculist. His books showed the condition of his vision to be as follows: Acuteness of vision in right eye $\frac{20}{20}$; the vision in the left eye $\frac{20}{30}$. This eye was hypermetropic to about one dioptry. Vision could be raised to normal by a + 0.75 glass. Convergent strabismus had already commenced. He was advised to use atropine and glasses, but for some reason this had been neglected, and the strabismus developed, and, as the future history proved, deterioration of vision in the left eye developed also. Just before coming to me he met with an unfortunate accident. A little boy shot a horse-shoe nail into his right eye, which entirely destroyed the sight. It was necessary to enucleate it. The vision of the left eye was $\frac{20}{60}$; even this vision was uncertain, as he had the so-called "come-and-go" acuteness of vision. This could be improved but very slightly by the correcting glass, but by constant and persevering exercise the acuteness of vision gradually improved, and continues to improve up to the present time. He now reads $\frac{20}{30}$. This case certainly proves, beyond the possibility of doubt, that amblyopia does develop as strabismus develops, and that it can be relieved by proper treatment and systematic exercise. Is it not but reasonable to suppose that, had the operation for strabismus been performed when it first developed, deterioration of vision in this eye would have been prevented.

I wish here to relate briefly the history of the other case, which has strongly impressed me as to the importance of, in some way, relieving strabismus as soon after it shows itself as possible. Two members of the same family—a little boy and little girl—each had convergent strabismus, apparently the result of a low degree of hypermetropia. Strabismus began to develop in each about the sixth year. The boy was eight years of age and the girl nearly seven when they came under my observation. The left eye of both children was slightly amblyopic, each reading in the defective eye $\frac{20}{40}$, as near as could be made out in children so young. The strabismus was well marked, reaching a little over three lines in both. An operation was advised for both, but the parents consented only to have the operation performed on the elder child at that time. Both, however, had the refraction carefully corrected with glasses. The child upon whom the operation was performed had binocular vision perfectly restored, and his acuteness of vision remained at $\frac{20}{30}$ with the correcting glass, which he always used. The strabismus remained stationary in the younger child, but the amblyopia increased, as an examination several years afterward showed distinctly. I think it is but fair to conclude that, had this child been properly treated, she would not have suffered deterioration of vision in her defective eye.

Any one who has had much experience in the treatment of squint recognizes how difficult it is to restore binocular vision after it has once been lost, yet this is an object paramount in importance, and I maintain that it is much easier to do this by operating early, soon after strabismus shows itself, than by operating later in life. I have from time to time carefully examined young people who had their strabismus cured by a timely and proper operation early in life, and in but few such cases have been able to demonstrate the absence of binocular vision. All the defects of the operation, the deformities resulting from it, such as sinking of the caruncle, are less likely to take place after an operation on children, in my experience, than after operating on adults. The loss of balance between the "accommodation" and the internal recti muscles, together with asthenopia, so frequently met with after this operation has been performed upon adults, is much more easily overcome, or I should say much more easily overcome

Digitized by Google

by nature herself, when the operation is done on children, than at other periods of life. And it so frequently happens that the treatment of asthenopia, after the operation has been successfully performed, is much more tedious and much more annoying to both patient and surgeon than any other part of the treatment. After strabismus has lasted for a number of years it frequently results in most serious loss of proportionate strength between the internal and external recti muscles. There is very great thickening and contraction of the internal, and corresponding elongation and atrophy of the external, rendering the trouble all the more difficult to remove. Such a course is prevented by operating early in life.

These conclusions are arrived at by actual experience and clinical observation. I am well aware that they are contrary to our usually accepted theories on the subject. But theories and far-fetched attempts at explanation should not be permitted to weigh in the balance with experience. There are many obscure points in ophthalmology which cannot be explained by any theory. We must learn to look such calmly in the face, in order to undertake with courage the task of elucidating them. General medicine, which has for ages been the object of investigation, together with all branches of science, affords many more unfilled gaps than does ophthalmology, which was, so to speak, born but yesterday. Hence, let us not despair, but seek diligently to analyze the many facts that practical experience presents every day, and light will eventually dawn upon us.

DE CERTAINS TROUBLES DE LA MOTILITÉ OCULAIRE ET DES MOYENS D'Y REMÉDIER.

ON CERTAIN DERANGEMENTS OF OCULAR MOTILITY, AND THE MEANS OF REMEDYING THEM.

ÜBER GEWISSE MOTILITÄTSSTÖRUNGEN DES AUGES UND DIE MITTEL ZU IHRER BESEITIGUNG.

PAR LE DR. ABADIE.

De Paris.

Je laisserai de côté à dessein la question du strabisme, affection contre laquelle nous possédons aujourd'hui des méthodes opératoires qu'on peut appeler parfaites, pour m'occuper exclusivement de certains troubles de la motilité oculaire qui semblent avoir échappé jusqu'ici à tous nos moyens d'action et auxquels, néanmoins, on peut remédier ainsi que je l'indiquerai par la suite.

Les troubles de la motilité oculaire que j'ai en vue en ce moment sont :—

1. Le strabisme intermittent.

2. La parésie de certains muscles provoquant de la diplopie sans déviation apparente de l'œil.

3. L'insuffisance des muscles droits internes.

Occupons-nous d'abord des troubles de la première variété.

Le strabisme intermittent, je le reconnais, est souvent la conséquence d'un vice de réfraction et est justiciable de l'emploi des lunettes. C'est même cette variété de strabisme qu'on guérit le plus communément par l'emploi des moyens optiques ou orthopédiques, mais nous devons néanmoins reconnaître qu'il est des cas où le strabisme intermittent persiste même après que nous avons eu recours à ces divers moyens.

Dès lors que faire le plus souvent nous abandonnons les malades à eux-mêmes et nous renonçons à guérir leur difformité.

Digitized by Google

Pourtant ces cas ne sont pas incurables, étudions en effet, leur pathogénie. Le strabisme intermittent est dû à ce qu'à certains moments sous certaines influences, mise en jeu de l'accomodation ou de la convergence un des muscles le droit interne le plus souvent innervé très énergiquement prend une action prépondérante.

Il est trop fort à un moment donné, bien que son insertion au globe oculaire soit normale puisque à l'état de repos le strabisme disparait.

Que faut-il donc obtenir? Il faut affaiblir ce muscle sans toucher à son insertion, sinon, on risque de transformer un strabisme intermittent en strabisme permanent du côté opposé. On atteindra ce but en pratiquant la ténotomie partielle du muscle qui se contracture de temps à autre en suivant les règles que j'exposerai tout-à-l'heure.

2. J'étudierai maintenant un cas rare à la vérité, mais si difficile à guérir dans la pratique où à la suite d'une parésie portant sur un muscle oculaire il est resté une diplopie très génante sans déviation apparente de l'œil. Ces cas, je le reconnais, sont rares. D'habitude les paralysies oculaires guérissent par l'emploi prolongé de l'électricité et les médicaments appropriés à la cause pathogénique du mal, d'autrefois la paralysie persiste reste plus on moins complète et il reste un strabisme permanent qu'on peut guérir par les procédés habituels.

Mais cette règle souffre quelques exceptions. J'ai rencontré des malades chez lesquels une diplopie très génante persistait, diplopie d'autant plus génante que les images étaient très rapprochées et chez lesquels il n'y avait pas de strabisme pas de déviation apparente de l'œil.

En pareille circonstauce l'emploi des prismes qui rapprochent les images et tendent à provoquer leur fusion nous sont d'un grand secours, mais ils ne réussissent pas toujours. J'ai eu alors l'idée de ramener l'équilibre en affaiblissant par la ténotomie partielle le muscle antagoniste du muscle parétique. J'ai obtenu plusieurs fois de cette façon des résultats complets.

Sans modifier l'insertion du muscle je l'affaiblis progressivement de telle sorte que séance tenante en dosant, pour ainsi dire, peu à peu l'opération, amincissant le muscle de plus en plus, je diminue peu à peu l'écartement des images jusqu'à ce qu'elles soient entièrement fusionnées. Étaient-elles tout d'abord à 0.30 centimètre l'une de l'autre, je coupe quelque fibre tendineuse et aussitôt le malade n'accuse plus qu'un écartement de 0 millimètre, 15 centimètres, quelques fibres sont sectionnées encore et finalement la fusion est obtenue.*

3. Enfin, j'aborderai en terminant la question la plus importante, celle de l'application de la ténotomie partielle à la guérison de l'insuffisance musculaire des droits internes et des troubles qui en sont la conséquence.

Personne n'ignore combien est fréquente chez les myopes l'insuffisance des droits internes et quels sont les troubles fâcheux qu'en sont la conséquence. En première ligne nous signalons l'asthénopie musculaire qui rend tout travail de longue haleine impossible. Les lésions si fréquentes de la région maculaire dûes aussi en grande partie aux compressions exercées par le droit externe sur les éléments vasculaires et nerveux de la région polaire postérieure pendant l'acte da la convergence.

Pour remédier à cet état de chose on a recommandé l'emploi des prismes dans les cas légers, la ténotomie complète des droits externes. Mais les cas les plus nombreux sont précisément les cas intermédiaires ceux où les prismes ne réusissent plus, ceux où la ténotomie complète risque de prevoquer du strabisme et de la diplopie. Il faut alors recourir à la ténotomie partielle qui donne des résultats excellents.

La ténotomie partielle avec excision partielle du muscle se pratique absolument

* Soit dit en passant ce rapprochement, cette fusion des images prouve que la ténotomie partielle combinée à l'excision d'une portion du muscle met à néant les objections de ceux qui prétendent que cette opération n'affaiblit pas le muscle.

Digitized by Google

comme la ténotomie complète. Le manuel opératoire reste le même jusqu'au moment où le tendon est chargé sur le crochet. Alors, au lieu de sectionner toutes les fibres tendineuses on en laisse quelques-unes médianes plus ou moins, suivant l'effet à produire. En outre on enlève obliquement avec les ciseaux les fibres musculaires dont l'insertion tendineuse à été détachée de façon à effiler, pour ainsi dire, les muscles le laissant intact vers son insertion orbitaire mais l'amincissant autant qu'on le désire vers son extrémité oculaire.

Cette méthode opératoire qui permet de remédier à des troubles de motilité si délicats de globes oculaires nous parait susceptible d'être généralisée et étendue à la chirurgie générale. Jusqu'ici pour remédier aux difformités ou au troubles de la motilité musculaire, on a presque toujours pratiqué des sections tendineuses ou musculaires complètes et il est évident pourtant qu'il y a des degrés dans la difformité à laquelle on veut remédier.

Mais avant la découverte de l'antisepsie on ne pouvait pas aborder à ciel ouvert les extrémités tendineuses situés dans le voisinage des articulations. On faisait des sections sous cutanées un peu au hasard. Aujourd'hui on pourra peut-être en procédant comme nous l'avons fait pour les muscles de l'œil mieux doser les opérations et obtenir de meilleurs résultats. La chirurgie oculaire aura eu ainsi l'heureux privilège de fournir quelques indications utiles à la chirurgie générale.

INSUFFICIENCY OR INEQUALITY OF POWER OF THE SUPERIOR OR INFERIOR RECTI MUSCLES AND A VERY SIMPLE AND EFFICIENT REMEDY.

L'INSUFFISANCE OU L'INÉGALITÉ DE PUISSANCE DES MUSCLES SUPERIEURS OU INFÉRIEURS DU RECTI ET UN REMEDE SIMPLE ET EFFICACE.

INSUFFICIENZ ODER UNGLEICHHEIT DER KRAFT DER RECTI SUPERIORES ODER INFERIORES, UND EIN EINFACHES UND WIRKSAMES HEILMITTEL.

BY EDWARD O. SHAKESPEARE, A. M., M. D.,

Of Philadelphia, Pa.

I have found in my practice as an ophthalmic surgeon, that there is often an inequality of power, or want of proper balance, between the recti muscles of the two eyes. I have found that in cases of stubborn eye headaches, and other exceedingly annoying phenomena more or less closely connected with the eyes as the initial agent of the disturbance which have not been removed by attention to the focus of the eye and to the state of accommodation and condition of the external and internal recti muscles, there is often a want of equality and harmony of action of the superior recti. In directing the patient to look at horizontal lines placed at a distance, and alternately covering one eye very quickly, there is in these cases a more or less sensible displacement vertically of the projected image of the lines, just as is obtained in a lateral direction by applying the same test for insufficiency of the internal or external recti muscles. If a weak prism, say of one or two degrees angle, is alternately and very quickly placed in front of the eyes, with the base down or up, while the patient is still fixedly regarding the horizontal distant lines, there is a displacement of the projected image which is greater

Digitized by Google

in the one eye than in the other. If, again, an attempt is made to gauge the strength of these muscles, by learning the strength of prism through which the displaced images can be fused, it is also found in these cases that the one superior rectus is stronger than the other, or, which is much the same thing practically, the difference is between the inferior opponents.

Now, when we come to consider the small amount of vertical displacement which can be overcome by energetic contraction of even a vigorous superior rectus muscle, as compared with the great amount of lateral displacement which can be overcome by strong action of the internal recti, and remember what annoying and even serious trouble can arise, through the desperate efforts of abnormally weak muscles to prevent double vision in the case of insufficiency of the internal or external recti muscles, we should have no difficulty in understanding how a want of proper balance between the strength of the two superior recti or two inferior recti can cause serious trouble of a similar nature.

I have been successfully remedying the disturbances due to an inequality of the strength of the pair of superior or inferior recti muscles, in a very simple manner, for the last three and a half years.

If there is an error of refraction combined with this trouble, and I frequently find it so, I simply decentre the glass ordered for the refractive trouble, either up or down, as the case may require and as much as the circumstances may indicate, making use of the action of the decentred lens as a prism.

In cases where there is no error of refraction, or any other necessity to use a lens, I order a prism, with the base up or down as the case may be.

I have sometimes met with this trouble combined with insufficiency of the horizontal recti muscles, and have combined the remedies for each.

DISCUSSION.

Prof. N. Manolescu.—Dans l'état actuel de nos connaissances sur le traitement des granuleux, tous ce qui aide à ces malheureux, mérite notre attention.

La ténotomie du muscle droit supérieur, que je crois être le premier qui la propose, est une intervention chirurgicale qui rend de grands services dans le plus grand nombre des cas de granulations conjonctivales.

En effet, comme vous le savez très bien, le grand danger, dans cette maladie, est le pannus granuleux avec ses conséquences bien diverses, et que ce pannus est causé par le frottement de la cornée contre les granulations de la conjonctive du tarse supérieur.

Eviter ce frottement, c'est éviter le pannus. La ténotomie du droit supérieur permet d'éviter ce frottement en grande partie par l'abaissement de quelques millimètres qu'éprouve la cornée à la suite de la ténotomie du droit supérieur.

J'ai pratiqué cette opération au moins dans 25 cas et toujours j'ai enregistré une amélioration au bout de quelques jours seulement.

J'ai enregistré les malades qui n'ont pas pu échapser à la récidive prochaine du pannus, qu'après leur avoir fait cette petite et innocente opération.

Les résultats les plus éclatants de la ténotomie du droit supérieur se montrent surtout dans ces cas de granulations chroniques qui entretiennent un pannus épais dans toute la moitié supérieure de la cornée.

Pour que cette opération donne de bons résultats et sans aucun trouble optique, il faut *couper le muscle complètement et des deux côtés.*

Alors la diplopie n'apparait pas même dans la moitié supérieure du champ visuel.

Digitized by Google

L'examen soigneux des malades qui jouissaient après d'une bonne acuité visuelle m'en a donné la conviction. L'abaissement de la cornée ne constitue non plus une difformité.

Mr. H. POWER, of London.—I am old enough to remember the operation as it was left by Dieffenbach. The first operation for strabismus I ever saw was performed by Mr. Guthrie, who used a small curved knife, with which the conjunctiva was divided. After the eye had been everted with forceps, a grooved curved director was then passed beneath the tendon and a free division made of it. Good success often followed this simple proceeding. Of late, thanks to the continuous attention that has been directed to the subject by American, French, and German surgeons, great advances have been made. We all use atropine, determine the amount of hypermetropia, prescribe glasses, if required, find out if there be paralysis—and then operate. But I do not think gymnastic exercises of the ocular muscles are very generally practiced, in England, before the operation is undertaken, but I may be mistaken on this point. There is a certain class of cases in which, after the tendon is divided, inversion still remains. The operation is repeated, and still the strabismus is not cured. Is this due to the margins of the tendon being thickened?

Dr. E. O. SHAKESPEARE regretted that Dr. Abadie was not now present in the Section, for in listening to his most interesting remarks concerning his partial operation in cases of insufficiency, or squint, in which the trouble is not greatly marked, by removing, little by little, some of the fibres of the over-strong muscle, a thought arose.

It is, perhaps, the aim of all operative procedures for these troubles that the result be ultimately permanent. Now, it is a fact acquired by many observations, that, in general, or, at least, very frequently, when a small or even a large portion of the muscular fibres forming a muscle are excised, there is, sooner or later, a reproduction of the lost fibres and a renewal of the original power of the muscle.

This leads to the question which I had wished to ask of Dr. Abadie: How long do the beneficial results of this operation remain? Are they permanent? Perhaps Dr. Landolt, whom I see present, may be familiar with the final results of these operations, or, at least, with the experience of Dr. Abadie in this regard, and will kindly reply.

Dr. WHITE remarked that Dr. Landolt's paper was of value in especially calling attention to the great importance of careful examinations and prior treatment of cases of squint before attempting correction by operation—because he is aware that among American ophthalmic surgeons there is a great deal of careless tenotomy for squint without any close previous attention or investigation.

He wished also to refer to the methods of muscular advancement and advancement of Tenon's capsule as means of correcting not only paralytic, but concomitant squint as well.

He was indebted to Dr. Landolt three years ago for a renewal of his faith in muscular advancement, which had been sadly shattered by his failures in performing the operation. Since his adoption of Landolt's method he has had no bad results. He was not prepared to say whether advancement of the capsule or muscular advancement was preferable, as he had gotten exactly the same correction by the two operations, with perfect movement of the eyeball in every direction, but he does not do the advancement of the capsule as De Wecker does. In this way it gives only slight correction. Dr. White not only puts in the stitch above and below, through

Digitized by Google

capsule and conjunctiva, as far as the superior and inferior recti muscles, but also adds a third stitch through the overlapped muscle itself, which helps to set up adhesive inflammation, and in this way gets a much greater correction than by De Wecker's method.

Dr. B. J. BALDWIN, of Alabama.—I am glad that the question of age in the operation for squint has been referred to. I am convinced from my experience that amblyopia may sometimes be prevented by an early operation. Two years ago I believed with those who held that squint was congenital, but I do not think so to-day.

The cases of alternating squint which we all meet with and in which binocular vision has been retained, cannnot be accounted for unless we admit it is due to active use. I have in my list two cases of alternating squint, one forty-six years of age and the other thirty-four. In both binocular vision has been preserved—in one case vision amounting to ^{20} and in the other $\frac{20}{20}$ in each eye; of course there was manifest hypermetropia in both cases. How do we account for binocular vision in these cases if not due to their alternating character.

Dr. FULTON closed the discussion of his paper thus:—Amblyopia, as met with in strabismus, may be either primary or secondary, but the secondary cases can all be prevented or cured by a timely operation. As to the proper time to operate he said that it could not be settled by the age of the patient, but should be done just so soon as it was found that the strabismus could not be relieved by glasses and atropine, or by systematic exercise.

Dr. DUDLEY S. REYNOLDS, of Louisville, said he had found the correction of squint one of the most responsible and, at the same time, difficult duties he had ever been called upon to perform. He had given much study to the subject and had written a great deal about it. He is convinced the sources of failure in attempts to correct squint are numerous; that they lie mainly in the difficulty of securing homonymous fixation. Certainly no operation can possibly succeed where the optical properties of the eyes cannot be made the basis of the correction. He feels satisfied tenotomy is often done where a complete optical correction might be had with a decentred lens, or by the use of prisms. He thinks Prof. Landolt is entirely correct in the statement, the orthopædic exercise of the muscles should, in general, precede any operative interference. He thinks, in many cases, great discrimination is required of the surgeon, as to whether advancement of a weak muscle or tenotomy of the apparently contracted muscle shall be done. In persons under twenty years of age he often succeeds, where perception of light exists in one eye only, in developing considerable increase in the power of perception, often sufficient to enable the person to count fingers and, in some instances, to read large type. This, of course, should always be tried before any operation on the muscles. If the amblyopia can be reduced or overcome by strong concentration of light in the retina, the eye should be exercised in this way until fixation is possible. Then the correction of the muscular defect is comparatively easy.

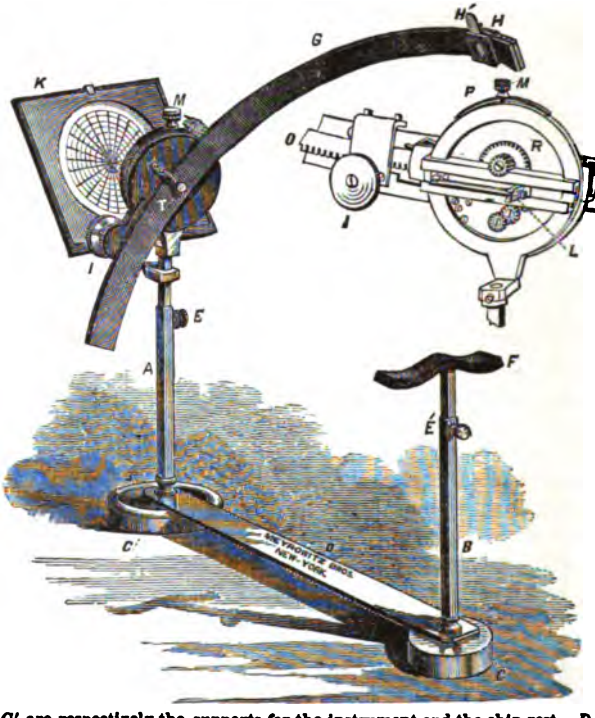
Dr. G. T. STEVENS.—It is my purpose to refer to the paper of Prof. Landolt, read this morning, and to his remarks this afternoon, mainly because he has gone over very important ground in which I am especially interested. With the clearness and directness of purpose so characteristic of the distinguished speaker, he called our attention, this morning, to two grand principles in the subject of strabismus. The first is that the great end to be aimed at in strabismus operations is binocular vision; and second, in order to attain to such a result we are not to sacrifice the usefulness of a muscle

Digitized by Google

essential to binocular vision, in order to obtain an apparently satisfactory result. So long as the operation for strabismus was performed only with respect to the cosmetic effect, when the surgeon cut the tendon sufficiently to satisfy the taste of fond parents, it mattered little how the end was attained, so long as the parties to the transaction were pleased. Now that we aim at a more perfect and useful result, our methods must be proportionately more exact.

We cannot hope to obtain exact results until we know what we are to do. It will no longer answer to place a graduated scale upon the eyelid and measure our devia-

FIG. 1.



A, *B*, *C* and *C'* are respectively the supports for the instrument and the chin rest. *D*, a connecting bar maintaining the requisite distance between the chin rest F, and fixation point T. *E* and *E'*, binding screws to adjust height of instrument. *G*, the sliding arc moved by a rack and pinion O and I. *H*, the carrier, a little brass box into which white or colored papers or the attachment for the measurement of deviations of visual lines may be readily slipped. *H'*, a small tongue of brass connected with the carrier, and serving as a stop in measuring the diplopia of strabismus. *K*, a wooden frame containing the chart. This frame is attached to support A by a hinge, allowing the chart to be pricked by the stylos L, which receives its motion from the arc through the gearing R. *M* is a clamp screw, to hold the arc in any desired meridian.

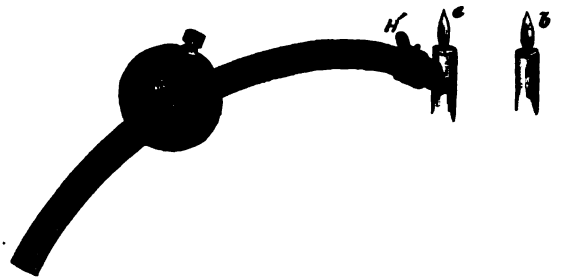
tion in lines. We must know the deviation in degrees, and we must determine the power of all the muscles involved in the deviation. This done, we have a reasonable basis for our procedure.

Prof. Landolt has done well in calling the attention of the profession to the necessity of preserving the function of the tenotomized muscle. If we have a case of considerable converging strabismus, there exists not only a contraction of the internus, but an elongation and relaxation of the externus. If, now, by a very free tenotomy of the internus, we succeed in releasing the eye to the extent of being

Digitized by Google

apparently in its proper relation to the other, we have two weakened muscles; the externus, already weak before the operation, and the internus, weakened by the setting back. We cannot hope that an eye with such muscles can adjust itself in binocular vision with its fellow. We have, by the operation, destroyed all hope of

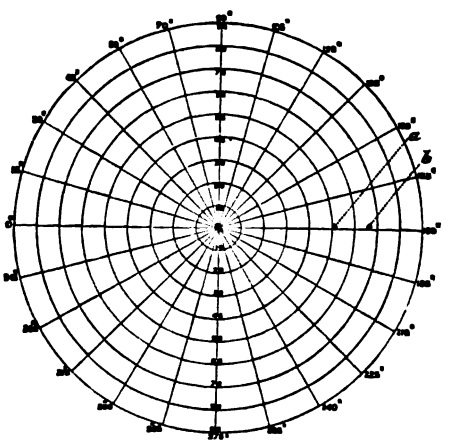
FIG. 2.



The double images of a candle. As the tongue, *H'*, approaches in front of the image *a*, it stops out the flame, when the chart is pricked by the stylos, marking the position of the stop when the first image is encountered. In the same manner, as the tongue encounters the second image, *b*, its position is recorded.

equilibrium in the action of the muscles of the two eyes. If, on the other hand, a moderate tenotomy of the contracted muscle is made, and if the relaxed muscle is moderately advanced, we maintain the physiological action of both, and the balance of energy between the eyes is maintained. A distinguished delegate has called our

FIG. 3.



The chart of the perimeter, showing the two points, *a* and *b*, indicating the degree and direction of separation of the two images of the candle; in this instance about 15° and in a horizontal direction.

attention to a class of cases in which he finds difficulty in effecting a cure of strabismus by strabotomy, and inquires what is to be done. It appears to me that the answer is found in the precept that we must, in such cases, advance the externus at the time that we set back the internus.

Digitized by Google

Permit me, now, to add a few words in relation to Prof. Landolt's very interesting remarks upon his method of determining the degree of diplopia in strabismus. Since the first publication of this method, several years since, it has been of interest to all of us who have cared to perform our work in this department with precision. But if two observers are aiming toward a common object, they may reach the goal by somewhat different routes. I have been accustomed to make these determinations in a somewhat different manner, and I take the liberty of presenting it to you as another, but not necessarily a better, method than that presented by our friend.

At the International Medical Congress at London, and before that, at a meeting of the American Medical Association, I had the honor to exhibit a registering perimeter, an instrument which has been several times invented since that time; the principal modification from the original being in the name of the inventor.

To the movable arc of the perimeter I affix a stop sufficiently large to cover the flame of a candle situated at a distance of twenty feet. (Fig. 1, p. 736.) The patient, having his chin in the rest, looks across the arc and at the candle, which he sees double. (See Fig. 2, p. 737.) The stop is now moved in such a manner as to cover one of the images, and a registration of this position is made upon the perimeter chart. The stop is then carried along until it covers the other image, when the registration is again made, as shown in Fig. 3. This method is extremely simple and easy, and the registration can be transferred at once to the case book.

Dr. Landolt then demonstrated on the blackboard his method of estimating the degree of squint by supposing the patient to occupy the centre of a circle, and the surface of the wall to be divided into radiary lines running from a common point (the object looked at) at 45°. The patient sees the central point in its proper place, and at an angle of so many degrees in any direction within a radius of 45°, according to the squint. The method was explained not long since in the *Archives d'ophthalmologie.*

THE VISUAL AXIS.

L'AXE VISUEL.

DIE SEHAXE.

BY ALBERT G. HEYL, M.D.,
Of Philadelphia.

It is proposed in this paper to define with some precision what the visual axis is. This will be done, not after the laboratory method, by measuring angles or locating points, etc., but the endeavor will be rather to develop the idea that the visual axis is a physiological reality, and not a mathematical fiction. The subject is of great practical importance. There can be no doubt but that in the treatment and etiology of cases embraced under the term Anomalies of Refraction and Accommodation, attention has been directed too exclusively to the eye as a focusing apparatus; this has been well so far as it goes, but it does not go far enough. The eye is not a mere mechanical apparatus, it is a living organ, and the true solution of many difficult and puzzling cases of asthenopia will only be found when clearer conceptions as regards vital, in distinction from mechanical, functions are evolved.

Digitized by Google

It will be well to glance at certain definitions which occur in works on physiological optics in connection with the visual axis.

1. The Gesichtslinie, or visual axis proper. This is defined as a line passing through the nodal points and striking the macula lutea. A brief explanation may make this somewhat clearer. It has been found convenient in studying the properties of lenses to establish certain cardinal points, as they are called, which depend for their position on the curves, refraction indices, etc., of the lenses. One set of these is known as the nodal points, and possess the following property: If a ray of light strike a lens in the direction of one nodal point, it will emerge from the lens as if it came from the other nodal point, and the emerging ray will be parallel to the incident ray. If a diagram be made showing this relation, it will be readily seen that if the two points were caused to approach each other until they coincided, then there would be one single nodal point and the ray of light would pass through it and undergo no deflection. It simplifies matters to consider such a fusion to have taken place, and it often can be done without making any practical difference. Now the eye possesses a lens apparatus, and therefore nodal points. Considering these nodal points to be fused together into one, we can say that rays of light striking the cornea so as to pass through the nodal point will undergo no deflection, but pass right on to the retina. The particular ray which, passing through the nodal point, strikes the macula, will, according to the above definition, coincide with the visual axis. Now, were the eye simply a mechanical apparatus, like a camera obscura, the definition would be correct; but the complex physiological requirements of the eye are not satisfied by it.

2. The Blicklinie. The eye being more or less spherical in shape, rotates about a point within, named the rotation centre of the eye. If a line be drawn from any point in space at which we are looking, to the rotation centre, it will constitute the *Blicklinie*. It is not identical with the *Gesichtslinie* already described. Now here we have another mechanical definition. It would be quite correct to draw or conceive of such a line were we studying the movements of a rounded body, destitute of life, with reference to points in space, but in so doing we do not rise to the level of physiological processes.

3. The Visirlinie, or collimating axis. This axis is made use of when two points in space, one more distant than the other, are brought in the same line with each other and the macula, *i. e.*, the two points and the macula lie on the same line, and this line is the collimating axis. It seems to me that it is also the same axis which is made use of when we look sharply at any object. Now, if we study the mental processes associated with the use of this axis it will be found to be a function essentially of monocular vision; even if both eyes be open when this axis is being used, it will be found that one eye is essentially sighting the points in space, etc. We have here a physiological reality, and the conception of the collimating axis is therefore one which will retain a position in physiological optics. Its peculiarities are that it is a function of monocular vision, that it is associated with conscious exercise of will.

4. There is one more conception to be mentioned. When one is walking along the street, with the mind absorbed in thought, the macula of each eye is pointing at one point in space. In this there is no conscious exercise of will, and yet the maculæ are each directed to the given point. A line drawn from this point to the macula will give what I conceive to be the initial position of the visual axis. Notice, (*a*) that we are not dealing in this case with a mathematical fiction, but with an *innate* mental conception. The collimating axis is likewise a mental conception; just as surely as I know in my own consciousness that I am pointing my finger in a given direction, so do I know when I point my macula in a given direction. One great point of distinction between the mental processes involved in these conceptions is this: The collimating axis is a conception which lies high up in consciousness, while the visual axis lies low down in

Digitized by Google

consciousness. The distinction may be thus illustrated. There is in the consciousness of every human being a fundamental conception of a median plane which divides the body into two lateral halves and extends indefinitely out into space; very likely this conception is developed from the symmetrical positions of the double portions of the body. This median plane has to do with our ideas of about the relative position of things in space, *e. g.*, the ideas of right and left. Now, this innate conception of a median plane is one which does not easily suggest itself to the mind; we have to study, and observe somewhat closely the operations of the mind to find it; in other words, it lies low down in consciousness. So is it with the mental conception which constitutes the visual axis, while the *collimating* axis is a conception much more easily thought about and studied, and therefore lies high up in consciousness. (*b*) The *visual* axis is a function of binocular single vision. Whenever in the process of vision the impressions of each retina are mentally conceived of as single, although each retina receives its own individual impression, we have the phenomenon of binocular single vision. This process I believe to be distinct from that of sharply fixing an object, although it is often associated with it. In this sharp fixation it is the collimating axis which is made use of even when both eyes are open, and there is brought into existence what has been described as the prevailing or dominating eye. It is, so far as the sharp fixation goes, a monocular process. The *visual axis* is brought into play in the process of convergence, as in writing, reading, etc. It involves the binocular use of the eyes *at the same time*, and always stands, in *consciousness*, in relation with the median plane. The two visual axes are therefore always to be considered as inseparably connected together, just as the two eyes may be looked upon as parts of one single organ.

(*c*) It will be seen from this that the nodal points do not come into consideration in the above conception of the visual axis. The truth seems to be that it is not the nodal points which determine the axis, but it is the axis which determines the nodal points. The anatomical point which has essentially to do with the position of the visual axis is the macula lutea; considered simply as a point, without reference to mental processes, it would be insufficient to determine the position of a line or axis. The other determining quantity is the angle made by the visual axis with the median plane. This may be called the axial angle. I believe that this angle is defined by mental processes set in operation as the result of the peculiar position of the macula in the retina, so that after all, from the physiological standpoint, the position of the macula does define the position of the visual axis. From all this the visual axis then may be defined as a mental conception, low down in consciousness, which may be diagrammatically represented by a line extending from the macula lutea to a point on the median plane and forming a certain angle with it termed the axial angle. It is a function of binocular single vision, and its initial position is determined when the muscles of the eye are in what is ordinarily described as a state of rest, *i. e.*, not being acted on by the will. A curious coincidence may be noted here between the initial position of the axis and the state of rest of the muscles. How is this brought about? What is the meaning of the muscles being in a state of rest? These questions bring us face to face with a great many obscure problems, *e. g.*, the physiology of muscle, the development of the macula lutea, etc.

The method of formation of the macula lutea is unknown. Von Baer and Huschke started the idea that the macula is a remnant of the fœtal cleft, and this, in lieu of a better explanation, has been looked upon by embryologists as probable. As Kölliker has shown, however, a great difficulty which confronts this explanation is, that while the macula lies to the outer side of the disc, the fœtal cleft lies downward and inward. Vossius (1883), in some studies on the optic nerve, has arrived at the conclusion that the optic nerve undergoes a rotation in position during gestation sufficient to overcome this objection. By this rotation the point of entrance of the central muscles of the

Digitized by Google

retina into the optic nerve sheath comes to lie in the outer lower quadrant instead of the inner lower quadrant of the cross section of the optic nerve. Therefore, so reasons the author, a rotation of about 90° has taken place in the fœtal cleft toward the macular region. Even if this be granted, however, it does not explain or account for the wonderful precision with which the macula is located and formed. I believe that there is an explanation which can dispense, if need be, with the fœtal cleft, and which accounts for the location and also for the coincidence between the state of rest of the external ocular muscles and the initial position of the axis as already defined. The explanation, then, which I have to offer is that the macula is not formed because of any inherent peculiarity in the embryonic retina, but that it is the result of intraocular pressure which, in the formation of the eye, after the closure of the fœtal cleft, so presses on the tissue which is to constitute the retina that the inner layers split, leaving the layer of cones exposed; that this pressure is perpendicular to the tangent at the macula; that the position of the macula in relation to the corneal axis is due to the antagonisms between the external muscles of the ball.

The principle underlying this explanation may be thus illustrated: Take a circular piece of rubber cloth of given diameter; at every point of the circumference let an equal force be applied, the line of force being perpendicular to the tangent of the point at which it is applied. The rubber will stretch in all directions, one point alone remaining stationary and in equilibrium. This will be the centre, and if the tension be sufficient a rupture will take place at this point. Suppose in place of the rubber we have the embryonic tissue of the retina subjected to a similar tension. Then we can understand how, at the central point, the vitality of the tissue would suffer, the blood supply to it diminish and a defect arise. Such, roughly stated, is the theory. Let us examine a little more thoroughly the details of the process. It will be necessary to briefly allude to several preliminary points:—

1. The recti oculi muscles may be divided into two sets: (*a*) Those supplied by the third nerve. (*b*) The rectus externus, supplied by the abducens. These two sets are antagonistic, so that not only is the externus opposed by the internus, but also, in a measure, by the superior and inferior, by virtue of their power to rotate the eye inward. The consequence is that the externus, in order to maintain equilibrium, has to exert a greater force than it otherwise would.

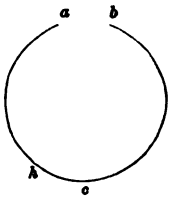
2. Its muscle is not simply to be looked on as a contractile band, but a living structure attached to the organ, which it is to move by a tendon which *is endowed with reflex sensibility.* A familiar illustration of this is the knee reflex. Here a blow upon a tendon causes the muscle to contract, and I think the conclusion, if the matter be studied, will be that there is an unconscious contractility set up in all muscles through the agency of their tendons.

3. Applying this to the eye, we may say that as the recti muscles are formed, a degree of unconscious tension is set up in them corresponding to the reflex demand of their tendons. Suppose the eye, in its process of formation, is converged, by reason of the embryonic state of the externus. As the latter, however, develops through its tendon, contractility is set up in it until the antagonism set up by the third nerve muscles resists further shortening of the externus. Thus, equilibrium between the recti is established, and such are the relations of the parts that the eye assumes the proper position in the orbit. This is altogether a reflex and unconscious act. Nor is this all. Within the eye is the ciliary muscle, and between it and the recti an antagonism exists. As equilibrium is established between the two sets of recti muscles, so, also, is there an equilibrium between the total pull of the recti and the ciliary muscle. Precisely how this happens need not be elaborated at this time. It may be through the pull of the recti tendons on the scleral region anterior to their insertion, or it may be through the effect induced on the intraocular tension by the contraction of the recti.

Digitized by Google

4. What is the effect of all this? We have the third nerve recti muscles antagonized by the single externus muscle; this will result in the sclera being pulled on by the externus tendon to a greater extent than by the third nerve tendons, and, as a consequence, the scleral region *posterior* to the externus tendon will be the most yielding part of the sclera to the intra-ocular tension, and in the processes of growth and shaping of the ball there will be special tendency to bulge in this region. More than this. The total pull of the recti developed in establishing the position of the ball in the socket starts up antagonistic contraction of the ciliary muscle. Through this the sclero-corneal foramen would be drawn toward the corneal axis, *i. e.*, its diameter will be shortened, the choroid stretched, and the ciliary processes advanced. At the same time, the vitreous tension is being developed, the principal element, perhaps, being the tension of the anterior chamber. The result of all of this is that while the embryonic structure which is to constitute the retina is drawn by the ciliary muscle around the vitreous, a pressure within the vitreous is tending to stretch it in every direction.

It will, perhaps, make this rationale clearer if we glance at the mechanical principles involved. The problem is essentially to ascertain the effect of pressure from within on the casing of a body.



Let *a c b* be a section of a sphere filled with water, having an opening at *a b* of a given area. Suppose at *a b* pressure is made on the water in the sphere, through the opening *a b*. The effect will be that each unit of surface of the sphere will be pressed on by the same amount of pressure. Suppose the casing of the sphere to be extensible to the same extent in all directions: increase the tension to an extent sufficient to enlarge the sphere; the spherical shape will be preserved if every point of the spherical case be equally free to move outward from the centre. Suppose, however, *a* and *b* are fixed points. Then, if sufficient force be applied, the spherical form will be lost, a sphenoid will take its place, the long axis being transverse and the short axis running antero-posteriorly through *c*. Suppose the point *a* and *b*, instead of being rigid, are acted on by equal forces tending to bring them closer together. The effect on the point *c* would be essentially the same, *i. e.*, it would still be the point in equilibrium in the line *a c b*. Now, suppose that the casing, *a c b*, instead of being the same in thickness at every point, is thinner in the half *a c*. If, now, the same forces be again applied, the point of equilibrium will no longer be in *c*, but will be moved toward *a*. Let *h* be the point. Suppose, in the above cases, the forces be so applied as to cause a slight rupture in *a c b*. This rupture will take place at the point of equilibrium. Let us now apply this to the formation of the macula. Let us suppose a time in the embryonic development of the eye when the vitreous exists, enclosed by the tissue not yet fully differentiated into the hyaloid membrane and retina; the ciliary muscle is developed, and also the aqueous humor. We have the anterior tension exerted through the anterior scleral foramen; we have the ciliary muscle contracting and drawing what will be later, or what already is, the ciliary processes forward, and thus tightening the hyaloid and retina. The effect of this is obvious. The point corresponding to *c* in the diagram is pushed back, and thus the antero-posterior axis is formed. There is an important principle in this as regards the relation between the sub-tension, the contraction of the ciliary muscle, and the length of the antero-posterior axis of the ball. Let us, however, keep to the subject before us. Were this process of extension to take place evenly in a casing with the posterior end of the antero-posterior axis as the point of equilibrium, the macula would be on this axis, but the conditions are not so. As has already been shown, the ball is more free to spread in the external half, owing to the greater force exerted on the sclera by the externus in its antagonism to the third nerve recti muscles, and hence the macula is shifted to the external side of the point which

Digitized by Google

it would otherwise occupy, and the greater the contraction of the externus, other things being equal, the greater the eccentricity of the point of equilibrium, *i. e.*, the further to the external side of the point pressed by the antero-posterior axis.

One or two questions may arise here :—

1. Granting all this, is there any proof that such a point of equilibrium would lose its vitality and become converted into an opening? In answer to this, we have, I think, the conus as an illustration of what intra-ocular traction and tension can do. Here we have lessened vitality in the choroid adjoining the disc, with gradual disappearance of the elements. There is another thing to be remembered in this connection, and that is, the lower the vitality of the cells in any part of the body, the fewer the blood vessels in it; the reason the cornea is ordinarily destitute of vessels is because the vitality of its cells is low. Stimulate these cells, as by hot water, and blood vessels begin to form. The process, then, by which the macula is formed is that, by virtue of its being the point of equilibrium in the embryonic tissue enclosing the vitreous, its cells become less vitalized, the blood vessels belonging to it shrivel and disappear, and, finally, a slight rupture takes place, and the macula is formed.

2. Why is there not an opening through the whole thickness of the retina, and not merely through a part of it? This cannot be answered until the connection between the different elements of the retina is better understood. Embryology shows this much, that the cones are among the earliest formed retinal elements, and, possibly, at the time of formation of the macula, they are already too strongly developed to be affected by the tension and pressure.

We have thus set before us the mechanical processes concerned in the development of the macula and of the visual axis. But this axis is not simply a mechanical production. Were it so, the axis would coincide with the line of intra-ocular force exerted at the macula, and this would be represented by a line drawn perpendicular to the tangent at the macula. The visual axis, although in part depending on mechanical causes, is essentially a mental conception. A fundamental mental conception in every human being, as has already been shown, is that of a median plane dividing the body into two lateral halves. Through this conception of a median plane we get certain ideas of our relation to things in space. The *visual axis* is a mental conception, just as the median plane is, and stands, doubtless, in some relation to it. This relation, I think, is dependent on the degree of eccentricity of the macula as regards the antero-posterior axis. Were the macula at the point pierced by the posterior pole the visual axis would be perverted to the median plane. As soon as eccentricity of the macula begins to the external side we begin to have an angle formed with the median line, and the greater the eccentricity the greater the angle, other things being equal. This process is, I believe, associated with the *perception*, but not the *projection* of light.

Now, conceive of an eye at the moment of its exposure for the first time to the influence of light. We have before us an organ in which the macula is pointing in a given direction corresponding to the equilibrium already established between the two sets of recti muscles. Ether undulations stream against the organ from the outer world; the first effect will be that upon the sclero-corneal epithelium, which I look upon as a structure intimately associated with the process of sight.* The result of this will be to give a *set* to the ball, or, to express it in another way, the muscles of the ball will grip it more firmly; this is an *unconscious* reflex process. More than this, ether undula-

* *Vide*, "A Form of Epithelial Mycosis," Albert G. Heyl, M.D., *Transactions* Medical Society, State of Pennsylvania, 1884. Also, "Purulent Ophthalmia and its Treatment by Hot Water." *Archives of Ophthalmology*, English edition, Vol. xv, No. 3, 1886. G. P. Putnam's Sons, New York.

Digitized by Google

tions stream into the eye, striking the percipient elements of the retina, and starting in the consciousness the sensation which we call light. One spot in the retina will be affected to a peculiar degree, viz.: the macula, and the sensation thus developed will be located by reference to the median plane, and will constitute the visual axis. The train of phenomena will, perhaps, be better followed by reference to what occurs in another organ, viz.: the leg. When we are lying down the muscles of the leg and foot are said to be at rest. This cannot indicate that they are perfectly flaccid; there is, beyond reasonable doubt, a species of involuntary tension of the tendons. This corresponds to the equilibrium established between the recti muscles before the eyes are exposed to light. Now, let the individual stand on his feet; instantly a state of rigidity is set up in the muscle through the agency of a reflex from the soles of the feet. This corresponds to the reflex of the sclero-corneal epithelium, giving a *set* to the recti muscles. The perception of heat through the proper terminal perceptive elements of the skin and of light through the retina are to be classified together, but the macula seems to have no analogue in the skin, being peculiar to the eye.

I wish, in conclusion, to direct attention to the phenomenon of fusion, which is intimately related to the visual axes. Fusion may be defined as an involuntary process by virtue of which the axis of each eye tends to intersect at the same point in space at the same moment.

Physiologically it is an involuntary process and takes precedence, in point of time, of voluntary binocular vision. That is to say, in a normal condition fusion must take place before conscious binocular vision. Hence, in cases of difficult vision (the reference is not to defective vision from structural lesions, but to functional disorders) it by no means follows that the root of the trouble is a focusing defect—it may be simply a defect of fusion.

Space may be considered as being made of an infinite series of lamellæ, curved, concentric, applied closely to one another. Some of these will be more distinct than others; and we have presented in connection with this the idea of depth, which does not specially concern us at present. Select a given lamella in front of us. It will be intersected by the median plane of the body in a vertical line. Conceive of a plane at right angles to this median plane and coinciding with the plane passing through the origins and insertions of the recti interni and externi (it is supposed that these recti planes are on the same level). The intersection of this plane in the space lamella will be a horizontal line. Now, both of these planes, the horizontal and the vertical, are, I believe, innate mental conceptions, not diagrammatic fictions. Suppose the lamella chosen at a distance at which the initial position of the axis pierces the median plane. Then two distinct phenomena are to be noted; one relates to the division of the lamella made by the horizontal plane. This has to do with indirect vision, involving the retinal surface in its general extension. It probably is not essentially connected with the lens apparatus of the eye. The point in connection with it to be specially noted is that the plane must be horizontal and include the horizontal recti planes of both eyes. If the horizontal plane of one eye lie below the other, or inclined to each other, then difficulty in fusion may arise. It is to this category that the cases belong in which the patient walks with his head inclined to the shoulder, and some cases of this kind, at least in my experience, can be met by oblique prisms, base up before one eye, base down before the other, the axes being parallel. This phenomenon of binocular retinal fusion does not seem to be connected with voluntary vision, and I suppose exists in creatures below man.

Distinct from this retinal fusion is the axial fusion. Conceive of a plane passing through the axis in its initial position. It will intersect the median plane in a vertical line, which practically may be considered to be identical with the intersection of the

Digitized by Google

space lamella already referred to by the median plane. The axial plane must be perpendicular to the horizontal plane, otherwise it would not intersect the median plane in a vertical line. The axial plane is diagrammatic, not psychological, as I believe the median and horizontal plane to be, and simply is a help in our conception of the axis. The angle made by the initial position of the axis is the axial angle, and the abnormalities in the size of this angle lead to the axial anomalies proper. Sometimes the angles are too large, and the necessity occurs for the use of adducting prisms. Sometimes the angles are too small, and then abducting prisms are indicated. The whole clinical course of these cases, the relation of abnormalities of the axial angles to the refraction, need to be studied and arranged so as to be available for use in the daily routine of practice. One thing more can only be added here, and that is with reference to the muscles of the ball in reference to these fusion phenomena. Retinal fusion requires that the horizontal recti planes coincide, and this coincidence must be preserved. This is done through the agency of the superior oblique. To briefly explain this: From the physiological standpoint I believe that three sets of voluntary muscles attached to the ball exist, one supplied by the motor oculi or third nerve, composed of the recti and the inferior oblique. These muscles in their unconscious reflex tendon contraction take the initiatory in determining the position of the ball. Opposed to these muscles are two others. One, the superior oblique, supplied by the fourth nerve, which is the special antagonist of the inferior oblique. The initiatory action of the inferior may be supposed to have taken place, disturbing the horizontality of the horizontal plane. Now the superior oblique begins to act, involuntarily by its tendon reflex, let it be understood, until the horizontality is restored. It will be seen from this that it is the superior, not the inferior oblique which is the regulator of the horizontal plane. As this is an unconscious process, and not voluntary, it may be asked, How is it that the superior performs this function? It may be because it is relatively the weaker muscle, by reason of its line of traction being altered by its tendon bending around its pulley. The initiatory would then be made by the stronger muscle. It may be that there are cases of abnormality in the horizontal plane due to too great or too little action of the superior oblique, and it may be that cases which I have observed in which the image of one eye is projected behind the other find their explanation in such an abnormality.

The other muscle which stands in antagonism to the third nerve muscle set is the externus. This has been alluded to in the earlier part of the paper. It remains only to say, that in this case, as in the preceding, it is the stronger set which take on the initiatory in determining the position of the ball in the socket, while the weaker muscle—the externus—contracts until the equilibrium is established and the macula formed.

DISCUSSION.

Dr. Ole Bull, of Christiania, Norway.—I should like to know how Dr. Heyl would explain the existence of two maculæ. As we all know, many birds, which have but a restricted field of binocular vision, have two maculæ.

Dr. Heyl said it would be difficult to answer.

Prof. Keyser, of Philadelphia, remarked that he has been very much interested in and instructed by the excellent paper of Dr. Heyl, and, in illustration of one of the abnormities of the visual axis and its correction by prisms, related the following case: In May last a physician from the interior of the State of Pennsylvania brought to him a young man, 28 years of age, for examination in relation to a peculiar diplopia. The man said that from a little child he never could see like other persons; that he always saw two objects, one diagonally above the other, and he was obliged

Digitized by Google

to shut one eye to see and distinguish anything near or far. In this way he went through school. On careful examination all the muscles of both eyes were found in perfect action, but there was persistent diagonal diplopia of 60°. Vision either eye $\frac{6}{6}$ and no specially perceptible astigmatism. The diplopia was completely corrected by holding a prism of 10° at an angle of 60°, base downward. He was given a pair of prisms of 5°, with the bases in opposite direction, which made the corrrection, and with which he reads and works without any difficulty with both eyes open.

Digitized by Google

FOURTH DAY.

ON THE CURABILITY OF THE DETACHMENT OF THE RETINA AND ITS TREATMENT BY ASPIRATION OF THE FLUID UNDER THE RETINA.

SUR LA CURABILITE DU DÉTACHEMENT DE LA RETINE ET SON TRAITEMENT PAR L'ASPIRATION DU FLUIDE SOUS LA RETINE.

ÜBER DIE HEILBARKEIT DER NETZHAUTABHEBUNG UND IHRE BEHANDLUNG DURCH ASPIRATION DER FLÜSSIGKEIT UNTER DER RETINA.

BY DR. GALEZOWSKI.

Notwithstanding the progress made in the pathological anatomy of the eye, the actual acquaintances upon the pathogenia of the detachment of the retina are still very few; we have no certain knowledge of its etiology, nor about the manner it produces itself.

There is a positive fact, that the detachment of the retina, in the greatest majority of cases, is with myopia, but it can be found also in hypermetropic and emmetropic eyes, as I have mentioned in the statistics which I published on the subject in the year 1883, (*Recueil d'Ophthalmologie*, 1883, page 701). At that date, on 649 detachments observed at my dispensary, I found 51 hypermetropics or emmetropics, therefore, I repeat that myopia is not the only predisposing cause of the illness, but is surely one of its principal agents. Sometimes the illness breaks out suddenly, without any provocation. Sometimes, also, it is the consequence of traumatism, of a blow on the eyeball or on the head, of a contusion in the neck region, of a general commotion of the organism, produced by a fall.

In regard to the ophthalmoscopic alterations, there are a great many sorts, relatively to the atrophie choroidea, or posterior staphyloma, and to the place it occupies, but there are also cases in which there are no appreciable alterations of the choroid.

Leber's opinion was that all the detachments of the retina are sometimes preceded by a rupture of the retina, easily perceived with the ophthalmoscope, and he makes conclusion that the fluid sub-retinian is just the same as that formed in the fluid corpus vitreum, which introduced itself between the retina and the choroid. I am on this question of a different opinion, and believe that the laceration of the retina is far from being a general fact of the detachment. In my statistics established in 1883, on 649 cases, I have only observed 31 lacerations of the retina.

I have seldom regarded this class of diseases among constitutional illnesses. So, I have met it once in an albuminuric patient who I examined with Dr. Noël Gueneau de Mussy, and a second time in a man who suffered with neuro-retinitis albuminuria, and whom I saw with Professor Peter, but these are exceptional cases.

Syphilis constitutes also a cause, relatively rare, of these diseases; I have seen it but 13 times. Sometimes also, there is a sympathetic cause.

If I complete my statistic with the cases I have observed since that epoch until now, I find the following numbers :—

Digitized by Google

On 124,000 new patients treated at my clinic since 1867, and 28.000 patients I have examined in my own house (total 152,000 patients), I have constated :—

Detachment of the retina	784	(532 men, 252 women.)
Both eyes affected	57	
Emmetropia and hypermetropia	63	
Caused by wounds or traumatism	194	
Sequel of cataract extraction	13	
Syphilis	18	
Sympathetic affection	4	
Tumor of the interior of the eye	10	

A most important fact, upon which permit me, Gentlemen, to direct your attention, is that myopia is the most frequent cause of the detachment of the retina. But the vitreous liquid is liquefied before the detachment; that iritis and irido-cyclitis invariably make their appearance during a well-known period of the illness, and that those two last affections cause, at a certain moment, a cataract and the atrophy of the eyeball.

If now I pursue this idea, and seek to solve the question of the possible curability of the disease, I find myself face to face with the opinion shared up to the present by the majority of practitioners, that the detached retina cannot resume its functions. I had held this opinion for a long while, when I was struck by a very curious fact. I have found two patients with the same kind of lesions. In the infero-external region of the retina I found, as it were, folds of the detached retina, with scotoma in the corresponding visual field. Both patients had stated that they had lost vision of one eye for some time, that vision had afterward returned, to a great extent. There had undoubtedly been detachment of the retina cured spontaneously.

These facts led me to hope for obtaining a cure of detachment of the retina. I have resorted to the antiphlogistic treatment, to horizontal position, to iridectomy, to posterior sclerotomy with suture, and finally to aspiration of the sub-retinal liquid by an operative procedure, which I desire to bring to your notice to-day.

I have by these different methods obtained 15 cures.

Antiphlogistic treatment in connection with horizontal position, 7.

Antisyphilitic treatment, 3.

Treatment by aspiration without suture, 2 in 19.

Treatment by aspiration with suture, 2 in 6.

Treatment by posterior sclerotomy, 1 in 4.

Iridectomy has not afforded me any cures, but it has permitted me to arrest the inflammatory accidents on the part of the iris and choroid.

I aspirated by means of a small aspirating syringe, which I had made by M. Collin, surgical instrument maker, 6 rue de l'École, de Médecine. This syringe is constructed like that of Prof. Dieulafoy, of Paris, which he employs for pleuritic effusions. I pass the small trocar into the sclerotic, beyond the ciliary ring, and open the stop-cock. The liquid then pours into the syringe, its quantity ordinarily being from 1 to 2 grms. The effects of this operation are of little consequence.

I have applied this method 25 times; 19 of these cases were simple aspirations. Out of this latter number I have had two successes; in the other cases the liquid was reproduced. I thought at the time that in making the suture across the sclerotic of the detached portion of the retina, one would be able to maintain the retina in position and promote its adhesion. In six cases I have obtained two cures, but in the other four cases sight was not reëstablished. In two operated upon, hemorrhages into the vitreous supervened a few days after the operation.

Digitized by Google

For the suture I employ five threads of catgut No. 000, prepared and preserved in carbolic acid, hence antiseptic.

These, gentlemen, are merely the first attempts. I recommend to you an application of this method, and to see whether, by discarding the suture after the aspiration, and combining the latter with an injection of an antiseptic astringent liquid, we cannot bring about the cicatrization of the detached portion of the retina, and thus save the rest of this membrane.

DISCUSSION.

Dr. ABADIE, of Paris.—In order to treat in a scientific manner a subject so difficult and complicated as retinal detachment, first of all it is necessary to classify its causes. In some cases it is undoubtedly due to causes either rheumatic or syphilitic in nature, and here we may bring about an amelioration by a constitutional treatment. But this is by no means the rule. In the immense majority of cases it is due to myopia, and generally speaking without a single inflammatory symptom which would act as a precursor. And to what is this attributable? It seems rational to admit the mechanism of the sclerotic in progressive myopia. We know that it is gradually distended, the antero-posterior diameter of the eyes becoming greater, till finally the retina is separated from the other coverings. It explains to us the momentary improvement that the puncture of the sclerotic gives. The fluid ebbs out and the sclerotic retracts and the retina takes its proper place, but only in a few days to resume its first position and give us just the same condition of detachment as we had before the operation. The whole subject is remarkable for the insufficiency of its treatment.

Prof. KEYSER remarked—There is nothing so interesting to the ophthalmologist, and the treatment of which is such an enigma, as detachment of the retina. He was with v. Graefe when he first suggested and made incision of the retina to let the fluid out into the vitreous; also with de Wecker when he introduced his trocar through the sclerotica; and with Bowman and Critchett when they opened the sclerotic with the knife to let the effusion pass out under the conjunctiva. All of these operations he has tried many times, without any permanent success. In some cases there seemed to be an improvement at first, which soon passed away. The absorption by injections of pilocarpine, and the prostrate position with compress bandages, have all failed of permanency in his hands. In the operation of opening the sclerotic the vitreous does not give pressure enough to force the liquid out, and as the instrument of Dr. Galezowski draws it out by gentle action in vacuum, he thinks it deserves a fair test. He thinks there may be some benefit derived in very recent cases, but cannot see how there can be any adhesion of the retina again to the choroid in old detachments of months and years standing. He agrees entirely with the remark that there is always inflammatory action going on in all cases where detachment of the retina takes place. He has frequently noticed well marked choroiditis extending throughout the eye in such cases; and has had the separation to take place suddenly in cases of choroiditis while under his care. He says that all such patients should be treated for choroiditis at once, and any operation proposed be made soon after the detachment has taken place and absorption has not occurred under proper treatment. It is a well known fact that when detachment of the retina takes place there is softening of the vitreous, so in the operation suggested care must be taken not to draw the vitreous off instead of the effusion behind the retina. The operation as proposed by Dr. Galezowski is one requiring great care and delicacy to prevent more serious trouble. Attention must be called to the fact, however, that effusions take

Digitized by Google

place in and behind the retina, which are absorbed in a short time under proper treatment, so that too great haste in operative influence should be guarded against until internal remedies and other methods have been unsuccessfully tried.

RICHARDSON CROSS, Esq., of Bristol—I have watched one case where imperfection of the visual field agreed with a segment of the retina which lay anterior to the rest of the fundus. There was no apparent retinal degeneration; the condition suggested sub-retinal inflammation, and I expected detachment to develop. The patient was energetically treated by diaphoretics with absolute rest. No change occurred; and even some months afterward no alteration was noticed except that the retina was less prominent, as judged by direct examination. Here the exudation seemed to have organized, but had it increased detachment would have resulted.

I have had one successful result by sclerotomy under the detachment. A girl aged seventeen came in, August 1886, with well marked gray detachment in the lower segment of each eye, which floated up across the visual axis. She said she had always been short-sighted, and for some time had noticed a floating film in front of the left eye. In May 1886 the right eye became suddenly blind, and soon after a marked impairment of vision followed in the left. I found the vision R. not fingers; L. $\frac{2}{60}$, $\frac{3}{5}$, J. 10. at 8 inches. I exposed the sclerotic between the external and inferior rectus, where I considered the main detachment lay, and carefully dissected through with a Graefe knife. A thin tinged fluid escaped. The eye was kept closed six days, the patient being recumbent. On the ninth day vision of R. was $\frac{4}{60}$ $\frac{1}{11}$, Jaeger 10.

She left the hospital and returned on January 7th, 1887. The right saw $\frac{1}{60}$ and J. 4 close. Fine thin synchysis in vitreous. Disc pale; no white floating membrane. Position of detachment + 1. Fundus — elsewhere. The left eye saw as above. At her request I operated by the same method on this eye. The result was even more satisfactory than in the former case. I have no record of the vision with me, but it markedly improved, and I heard in August of this year that she sees very well.

I admit such cases are fortunate exceptions, but I believe the operation was the sole cause of the improvement of sight.

I am of opinion that, while Professor Galezowski's method is a real addition to the rational means at our disposal, incision of the sclerotic under the detachment, as practiced by Wolff and others, remains the best surgical procedure.

Every case, however, must be treated upon a full consideration of its pathological details.

Prof. LANDOLT, of Paris—A few words, gentlemen, on the subject that now occupies our attention. I have divided it into three forms: first, detachment due to choroidal exudation; second, that due to the very high degrees of myopia; third, that due to traumatism. And now as to the first, that due to choroidal exudation, a condition frequently disappearing under suitable internal medication and when an operation, indeed, would do positive harm. Often, indeed, that which we take for a retinal detachment with the ophthalmoscope is nothing more than an exudation in the tissue of the retina, without having separated the retina from the adjacent membranes. I have seen, very recently, a case in point. As regards the second form, I cannot understand how the theory of Graefe per se can hold for an instant. It is physically impossible for the sclerotic to so distend itself that the retina resting upon it shall become detached. It is necessary to search for a more satisfactory explanation, and I am only too sorry that I am unable to give it. Those forms of

Digitized by Google

detachment of the retina, in all probability due, as Leber says, to changes in the choroid or alterations of some sort in the nature of the corpus vitreum, in this form of the disease I regard an operation as indicated, that is to say when the detachment is of recent date and not complicated, and after I have decided to operate I do it with the greatest prudence.

As regards the operation with the syringe, I look upon it as exceedingly dangerous. A detachment which has been produced by an exudation is by no means comparable to a pleuritic exudation, in so far as its operative treatment is concerned. In the latter case we obtain a pathological liquid; in a detachment of the retina we run the risk of drawing off some of the corpus vitreum and so complicating the difficulty. Finally, as regards those cases due to traumatism, it is impossible to speak of them in a general manner, the circumstances attending the injury are so different. I wish to relate a case in point. A small boy came to me, accompanied by his father. He had just received a blow from a sharp stick in one of his eyes. I made out a detachment of the retina, near the macula lutea. I advised absolute rest, the greatest care and gentle pressure. Nevertheless, the father started out on a journey of fourteen hours in a railroad coach, with the child. Gentle pressure was, however, kept up, and, notwithstanding the father's imprudence, the retina re-attached and sight was completely restored.

Dr. Smith, of Detroit—The operation of Prof. Galezowski strikes me favorably, particularly if the needle is not passed through the retina, thereby endangering the escape of the usually fluid vitreous. I have made the operation of tapping with a paracentesis needle with exceedingly brilliant, immediate results, but, as Prof. Abadie remarked, the detachment returns anew and the ultimate result is *nil*. I have several times made the operation of my friend, Prof. Wolff, of Glasgow, who has reported a number of successes. As you know, it consists in making a somewhat free incision in the sclera and choroid, the idea being that the inflammatory exudate tended to hold the retina in place after the escape of the effusion. I have made this operation several times, but, I must admit, with only temporary amelioration.

Dr. Wm. Fred. Holcombe, of New York—The word "*cure*," used to-day by Dr. Galezowski, should be taken with *allowance*, due to his speaking English, while, in using French yesterday he said vision *never returned* in the part detached, but that all future detachment was prevented and present sight saved, and *this* is what he calls a "cure" and claims as most important. It is well to recall that the elder Sichel and other oculists always performed tapping of the eye with a long, thin knife, similar to von Graefe's cataract knife, and allowing the patient to go about and the oozing of any fluid to go on freely. Gradually, benefit became more and more apparent, and a so-called cure was effected, as fair sight was preserved. In the use of the catgut seton as a means of drainage, I conceive it may be of great benefit. The retina undoubtedly is more or less paralyzed, according to the pressure upon it, unless *rupture* has occurred. Any means of removing and relieving the pressure of the fluid without more injury than it affords, is very important. But let us not forget that sclerotomy proved successful in the hands of ancient surgeons, whose patients were thus greatly relieved, before they had any knowledge of the appearance of the optic fundus with the ophthalmoscope, and they only judged of results by return of fair vision.

Dr. Calhoun, of Georgia, asked Dr. Galezowski what advantage he expected to gain in making the operation upon chronic cases of detachment of the retina? He had made the operation a number of times and had been disappointed in the

Digitized by Google

results. In one case of recent detachment the effusion broke through the retina into the vitreous and was absorbed. The retina replaced itself with tolerable vision. In another case (traumatic) the foreign body was removed and the retina resumed its natural position, but there was no improvement of vision. But in chronic cases, as long as the eye remained free from secondary inflammation, he was of the opinion that no attempt should be made to replace the retina, but that the eye should be let alone, unless other complications manifested themselves.

Mr. H. Power, of London.—In detachment of the retina the situation of the fluid has not, so far as I know, been quite accurately ascertained. Is it entirely subretinal, and what relation does it hold to that pigment layer which probably fulfills important functions in regard to vision, and which, in its development, may be said properly to belong to, and to form an integral part of, the retina? I wish to ask M. Galezowski whether he is of opinion that that portion of the retina which is actually detached ever resumes its functions, whether the removal of the fluid be effected naturally or by operation? I think I can afford a partial explanation of improvement in vision after such removal of the fluid in some instances. A case occurred to me in out-patient practice, in which a woman in middle life applied for relief, in consequence of great impairment, though not total loss, of vision. The case was examined ophthalmoscopically, and extensive detachment of the retina was found to be present. She was directed to attend again in order that students might have an opportunity of seeing the eye. Three days after she reappeared, stating that her vision had greatly improved. On examination, the fluid was found to have been entirely absorbed and the retina to have become reapplied. Now, I presume that the retina here formed a sac which hung over and obscured the macula. When the fluid was reabsorbed, the sac disappeared and light once more fell on the region of the macula.

Dr. Galezowski closed the discussion of his paper with the following words:—I do not agree with Dr. Abadie that distention of the sclera has any relation to retinal detachment. With Dr. Keyser I agree. The operation I propose is a new and, perhaps, a more promising method for chronic and bad cases. In some cases I find arrest of the morbid action and restoration of sight in other portions of the retina. If I succeed in arresting the detachment, I feel that the eye is saved by the operation.

ON THE PREDISPOSITION TO GLAUCOMA.

DE LA PRÉDISPOSITION AU GLAUCÔME.

ÜBER DIE PRÄDISPOSITION ZUM GLAUCOM.

BY DR. BRUGSCH-BEY,

Ophthalmic Surgeon, Cairo, Egypt.

Since von Graefe declared glaucoma to be a disease with increase of intra-ocular tension and with a certain reaction on the functions of the optic nerve and the retina, increased tension is now considered by the greatest number of authors, with only a few exceptions, as the most characteristic symptom of this disease. As soon as the intra-ocular tension is increased and the sight begins to fail, we are justified in diagnosing

Digitized by Google

glaucoma, either primary or secondary, according as there is any other disease in the eye that may cause it. As the eyeball is formed by a dense membrane enclosing a liquid interior, the tension can increase in two ways—either by a larger secretion of fluids, or by the retention of liquids which have been used to nourish the eye. The theory of retention has already found some anatomical facts proving it. Kines, as it is well known, first drew attention to the occlusion of the region of the ligamentum pectinatum in a certain number of cases of typical glaucoma, by the growing together of the cornea and the ciliary portion of the iris. It is evident that after an occlusion of a part in this way a temporary, and in case of a perfect occlusion, a continuous increase of tension, *i. e.*, glaucoma can be provoked. According to this consideration, the author of this article conceived the idea that not only an occlusion, caused by inflammation or by any other pathological process, but even a natural short periphery of the cornea, would be able to produce the same effect. Therefore, eyes with a relatively small cornea would be more exposed to glaucoma than those with a large one, provided that in both eyes the length of the diameter would be the same.

According to this fact, we see only a very small number of children's eyes attacked by glaucoma. It is perfectly exceptional to see this disease in men younger than twenty years. Also, it is well known that the smaller an animal is the larger relatively is his cornea. I may be allowed to illustrate these statements by some calculations. We find in Jaeger's (an eminent late professor of Vienna) "Ueber die Einstellungen des dioptrischen Apparates im menschlichen Auge," pp. 12 and 13, the following data in connection with the eyes of children and adults:—

The diametrical proportion of the diameter of 20 children's eyes younger than 40 days is 17.4 millimetres. Von Reuss, in Graefe's "*Archiv für Ophthalmologie*," vol. XXVII, part i, p. 27–53, gives the middle length of the corneal diameter of young children as 9.5 millimetres. In the same author's writings we find for adults, middle length of the diameter of the eyeball, 24.3 millimetres; middle length of corneal diameter, about 11.6 millimetres. Hence it follows that during the time of growth the diameter of the eyeball increases from 17.4 to 24.3 millimetres. The length of the corneal diameter on the other side is only lengthened from 9.5 to 12.5 mms. The greater volume of the adult's eye relative to its corneal diameter and to the proportions, as we find in children's eyes, is very evident.

By the classical experiences made by Schwalbe, Leber and others, it is stated that the way in which the used liquids leave the eyeball is the region of the ligamentum pectinatum all around the cornea. A diminution of this way, *i. e.*, a small cornea with a short periphery, must throw great obstacles in the way of the affluent liquids.

The following calculations will serve to illustrate this:—

As it is not our purpose to state either the exact volume of the eyeball or the exact length of the corneal periphery, we may consider the corneal periphery as a circle with the radius of half the diameter and the eyeball as a globe with the radius of half the diameter.

By the formulas $d\pi$ and $\frac{4}{3}r^3\pi$, we find for children the proportion between corneal periphery and volume of the eyeball as about 1 : 100 (more exactly, 1 : 99), and for adults the proportion is 1 : 200 (more exactly, 1 : 203).

By this we see that for children 100 parts of volume have one part of way, and for adults, 200 parts of volume have only one part of way at their disposal. The greater difficulty on the side of adults is therefore evident.

A similar predisposition to glaucoma, as between adults and children, we see also between large and small animals. For instance, a peculiar ophthalmia of horses is presumed to be of a glaucomatous character. On the other hand, rabbits, mice and such like are almost never affected by glaucoma. Also by experimenting on them a

Digitized by Google

transitory increase of tension can be provoked, but never a continuous tension, a glaucoma, as we find it in human eyes.

If these more theoretical considerations should be true, a greater number of men suffering from glaucoma would be among a population showing the above-mentioned anatomical peculiarity than ordinarily is to be found. So, indeed, I find it in the city of Cairo, where also a good many of the patients from all Egypt pass under my notice. While in Europe the number of glaucoma patients is about 1 per cent., this number in Egypt amounts to about 4 per cent. 1½ per cent. are secondary cases. These figures are taken from 2000 patients. The predisposition of the Semitic race for glaucoma is observed in Egypt, and has also been noticed by other authors Also among Algerian Arabs glaucoma seems to be a very common disease. In the beginning of my medical practice in Egypt, I was greatly surprised at finding children attacked by glaucoma, principally of a secondary form. Very often after a perforating ulcer with anterior synechia of the iris, caused by a horrible Egyptian blennorrhœa, began to close, and the denser cicatricial tissue not allowing any more the aqueous humor to flow out, the tension slowly increased and a typical glaucoma resulted, which necessitated the operation of iridectomy. The immediate consequence of increased tension shortly after the healing of an anterior synechia is a most surprising fact in Egyptian ophthalmic practice.

The smallness of the cornea was first observed in finding that all artificial eyes sent from Europe were furnished with too large corneas. Even eyes with a cornea of 11.5 mm. are met with but very rarely. Attentive to these facts, I began to measure the corneal diameters of men of the pure Egyptian race. I found, by the examination of forty-six recruits in the Government Hospital at Kasr il Aïnee, the mean proportion of the corneal diameter to be 11 millimetres. I am very thankful to Dr. Mohamed Bey Off, who gave me the opportunity of measuring a number of these men of high stature. Then I measured nineteen adult patients on whom I operated for glaucoma. I only found a diameter of about 10.5 millimetres. These observations are very exact, because I made them during the narcosis of the patients.

The question has still to be studied whether the eyes of pure Egyptians are not smaller than the eyes of other races. The number of eyes I examined for this purpose is, indeed, not yet sufficient, because it is very difficult in the hot climate of Egypt to get eyes fresh enough. Already two hours after death the eye is too putrefied in the summer time, and cannot serve any more for measuring purposes. Nevertheless, I found in the cases I examined, under favorable circumstances, that the diameters were not smaller than 23.5 millimetres.

If it be true that eyes having small corneas are more disposed to glaucoma than eyes with large corneas, then a good many symptoms may be lightly declared, and thus overlooked.

So in many instances the sudden appearance of glaucoma acutum in its form "foudroyant" will lose its mystery. Such eyes have been long ago in danger on account of their anatomical construction, and as occasion arises, such as congestion, emotion, etc., an attack of glaucoma foudroyant is the result.

As both eyes generally have the same anatomical construction, it will not be surprising to see the other eye attacked by glaucoma after the emotion caused by the operation on the first eye.

Hypermetropic eyes, which are very often attacked by glaucoma, are very short eyes; but the vertical diameter is proportionally longer than in other eyes; so their form resembles more a globe, which possesses a great volume; whether hypermetropic eyes have also small corneas is a question still to be studied.

The healing effect of iridectomy in glaucoma will be easily understood on the basis

Digitized by Google

of this theory and in the following manner. If a glaucomatous eye remains without any help of art, it slowly degenerates and attempts to take the form of a globe. The first sign is the known disappearing of the sulcus corneo-scleroticalis. The cornea becomes flat. The iris, spread out like a compact membrane with a little hole for a pupil, hinders a further expansion of this part.

After a piece of iris has been cut out, the expansion is freely permitted. Indeed, we see colobomas of iridectomy in a good many cases enlarged some time after operation. A small, artificial pupil made for an optical purpose appears as a large coloboma made for glaucoma. Both sides of this coloboma remain parallel. Such enlargement can only be caused by an enlargement of the base of the cornea. If a coloboma is too small or too little peripherally done, it does not modify, in the least the glaucoma, because the desired expansion cannot take place.

I am well aware that the data I can give are too restricted to prove everything sufficiently, but since I conceived this idea, I see daily facts proving it, and therefore I wish to ask the illustrious members of this International Medical Congress whether similar facts have happened in their experience.

Till now I have only found one observation similar to mine, made by Professor Rimpoldi, who saw glaucoma more frequently in "small eyes" than in large eyes. Perhaps in Italy there may be similar pathological peculiarities producing the same effect as in Egypt.

DU GLAUCÔME DANS LA RACE NOIRE.

ON GLAUCOMA IN THE NEGRO.

ÜBER DAS GLAUCOM BEI DEN NEGERN.

PAR LE DR. MOURA.

Dans une communication faite à l'Académie de Médecine de Rio de Janeiro au sujet de la tension intra-oculaire dans les races noires, mixte et blanche, j'ai démontré au moyen de statistiques que cette tension variait beaucoup d'une à l'autre race. J'y ai fait remarquer que celle-là était bien plus exagérée chez la plupart des nègres dont l'état de la vision était pourtant physiologique, de façon qu'on pourrait considérer un grand nombre de ces individus sous l'imminence du glaucôme prodromique si, par hasard, quelques autres manifestations fussent constatées. Chez les métis l'accroissement de la tension oculaire se montre à un moindre degré ; enfin chez les individus de la race blanche elle est encore plus faible. Ainsi s'explique la fréquence considérable du glaucôme chez les sujets de la race noire ; ce fait est parfaitement confirmé par notre déjà assez longue observation soit en ville soit à mon service à la Policlinique où les sujets de cette race se présentent en plus grand nombre. C'est ainsi que sur un total de 5458 malades de tous les âges enregistré dans ce service et dont 3466 de la race blanche, 1379 métis et 513 noirs, j'ai rencontré le glaucôme dans les proportions suivantes : 1.36 pour 100 chez les blancs, 2.39 pour 100 chez métis, 10.72 pour 100 chez les nègres.

Cela montre donc combien varie d'une race à l'autre la fréquence proportionnelle du glaucôme ainsi que la prédominance marquée de l'affection chez ces derniers. Ce qui du reste semble expliqué par cette augmentation de la tension oculaire dûe probablement chez eux à des conditions particulières à la race.

Digitized by Google

D'abord deux facteurs principaux donnent lieu, chez ces individus, au défaut d'équilibre entre la sécrétion et l'excrétion des liquides de l'œil. Les nègres ayant le fond de l'œil fort pigmenté, ont-ils besoin de recevoir une plus grande quantité de rayons lumineux pour la clarté de l'image, et pour cela sont-il obligés d'avoir la pupille plus dilatée, d'où il résulte un embarras considérable au cercle de filtration antérieure des adhérences multiples se produisant à l'angle irien. En deuxième lieu l'endurcissement de la sclérotique débute plus tôt chez les nègres, de façon qu'il n'est pas rare de retrouver chez eux le glaucôme entre vingt et vingt cinq ans.

Il est bien probable que cette même cause agisse également sur la filtration postérieure, en l'empêchant même dans certains cas. D'ailleurs on observe rarement chez les nègres l'anneau sclérotical ; si l'on admet en même temps l'hypothèse très judicieuse et très acceptable du savant Prof. de Wecker que cette partie du pôle postérieur du globe oculaire joue un rôle marqué pour la filtration des humeurs de l'œil, on aura un élément de plus pour expliquer l'accroissement de la tension intra-oculaire dans cette race.

Le glaucôme se présente chez les nègres sous la forme chronique simple ; jamais je n'eus l'occasion d'observer aucune autre comme manifestation primitive, mais lorsque l'affection est arrivée au degré le plus avancé, il n'est point rare de voir le glaucôme prendre la forme irritative.

La marche du glaucôme des nègres est plus rapide par rapport à celle qu'on observe chez les sujets de la race blanche ; l'excavation de la pupille est plus profonde dans un délai relativement plus court chez les premiers.

L'iridectomie ainsi que la sclérotomie réussit parfaitement chez eux, bien qu'il ne soit point rare de voir la reproduction du mal de façon à réclamer la repétition de l'opération.

BI-LATERAL GLIOMA OF THE RETINA—ENUCLEATION OF THE RIGHT EYEBALL—EXTIRPATION OF THE CONTENTS OF THE LEFT EYE.

GLIÔME BI-LATÉRAL DE L'ÉNUCLÉATION RÉTINALE DU GLOBE DE L'OEIL DROIT EXTIRPATION DES MATIÈRES DE L'OEIL GAUCHE.

BILATERALES GLIOM DER NETZHAUT—ENUCLEATION DES RECHTEN AUGAPFELS—EXSTIRPATION DES INHALTES DES LINKEN AUGES.

BY A. G. SINCLAIR, M. D.,
Memphis, Tennessee.

V. B., male, æt. nineteen months, was brought to me in November, 1881, with the following history: In July of the same year vision of the right eye began to fail, and soon afterward also that of the left, and total blindness in both rapidly supervened. Following this, in a short time, a whitish reflection from the pupils attracted attention. This soon acquired an appearance which the father of the child described as "ridged" or "lumpy," and aptly compared with the convolutions of the brain. Coursing over this uneven surface, dark lines (blood vessels) were observed. The pupils were apparently somewhat larger than in the normal condition when the disorder was first noticed. There was no redness of the eyes, and apparently no pain until about the end of

Digitized by Google

September. At that time an attack of indigestion, lasting a day or two and attended with more or less fever, was followed by inflammation in the eyeballs, which continued up to the time of presentation, accompanied with, apparently, almost constant pain. On examination of the right eye, there was found considerable congestion of the conjunctival and episcleral blood vessels, haziness of the cornea in its central portion, diminished depth of the anterior chamber, the crystalline lens lying nearly in contact with the cornea and having on its anterior surface dots and patches of pigment derived from the posterior surface of the iris, the lens still transparent, enlargement and immobility of the pupil, except that a slight additional enlargement was obtainable by the employment of mydriatics, dullness and alteration in color of the iris and extensive posterior synechiæ. Through peripheral portions of the pupil, where unobstructed by pigmentary deposit, a yellowish-white reflex appeared, which, on close inspection, was found to proceed from the surface of an intra-ocular mass reaching nearly to the posterior surface of the crystalline lens. The tension of the ocular globe was somewhat above the normal degree. The movements of the eye were unrestricted. Vision had been entirely destroyed.

The foregoing description of the right eye applied also to the left, except that there was a space about one line in diameter near the centre of the cornea which was abraded and hazy, and that the intra-ocular growth was less advanced and was obscured by blood extravasations, both old and recent.

Diagnosis.—Bilateral glioma of the retina.

Prognosis.—Very unfavorable; enucleation of both eyes with possibly evisceration of the orbits was proposed to the parents as the only means of affording relief from pain and prolonging life. They eagerly accepted the proposition, and accordingly, on September 7th, I enucleated the right eye and removed with it about half an inch of the optic nerve. Entire relief from suffering followed the operation, and extreme debility was speedily replaced by strength and vigor. A few weeks later I removed the contents of the left orbit as thoroughly as it was possible to do it without the employment of cauterization of its walls. From this operation, also, the child made a rapid recovery.

For microscopical examination the first specimen was submitted to Dr. T. Mitchell Prudden, Director of the Physiological and Pathological Laboratory of the College of Physicians and Surgeons, New York, whose report is as follows :—

NEW YORK, Nov. 23d, 1881.

My Dear Doctor :—The microscopical examination of the eye sent me confirms your diagnosis. I examined individual sections of the whole eye with the following result :—

The cornea shows few new-formed blood vessels in the limbus. There is a small quantity of granular fluid in the anterior chamber. The iris, ciliary body and portions of the choroid are sparsely infiltrated with pus. There is a broad, vascular, cyclitic membrane closely enclosing the lens behind. There is a partial detachment of choroid from sclera, and nearly complete separation of retina from choroid. The retina in the parts examined is almost completely involved in larger and smaller nodules of closely aggregated, small, spheroidal and variously shaped cells. With a nominal amount of intercellular substance the nodules are moderately vascular, and are, in some cases, aggregated into fungiform masses. The largest of these nodules projects forward from the optic nerve entrance, while the numerous smaller ones are irregularly scattered over the retina. There are also small similar nodules in the choroid, and also at the sides of the cyclitic membrane upon the ciliary body.

The new growth has infiltrated back into the optic nerve about 1.5 mm. beyond the cribriform lamella.

The anatomical diagnosis would, accordingly, be "irido-cyclitis and choroiditis, with glio-sarcoma of the retina extending into the optic nerve beyond the cribriform lamella."

The tumor cells, so far as could be detected with the microscope, extended only to within 7 mm. of the cut end of the optic nerve.

Digitized by Google

The microscopic examination of the second specimen was made by Dr. Carl Heitzman, of New York, who reports as follows, under date of September 25th, 1882:—

DR. A. G. SINCLAIR :—

Dear Doctor :—I have examined the tumor of the eyeball and the extra-ocular mass, with the following results :—

The tumor is a primary glioma of the retina which grew into the vitreous body, almost completely replacing it, and reaching the posterior aspect of the lens without having led to an increase of the diameters of the eyeball.

The tumor consists of a large number of globular elements characterized by a large, often double, nucleus. Between the groups of the glioma elements there is scanty, fibrous connective tissue. The change into glioma tissue pervades a great portion of the choroid, the pigment layer between the retina and choroid being, at least in some places, still recognizable. Broad bundles of fibrous connective tissue between the groups of glioma elements characterize the former tissue of the choroid. Anteriorly, there are remnants of the vitreous body nearly intact, still even here a beginning transformation of the vitreous body into glioma tissue can be traced.

The crystalline lens is, in portions, transformed into homogeneous lumps, replacing the ribbons, which, as such, retained their shape; scanty plastids, partly lying in vacuoles, are scattered throughout the ribbons of the crystalline lens, representing a transformation of the bioplasm of the tissue into glioma tissue. The peri-orbital tissues are more or less transformed into glioma. The lachrymal gland exhibits glioma elements in its connective tissue portion, while the epithelia are not markedly changed, except an augmentation of the nuclei and a coarse granulation. The calibers of the acini are not noticeably altered.

Confirmed by the results of these examinations, the correctness of the diagnosis made prior to the operation is, of course, beyond question.

With the subsequent history of the case I have kept myself familiar, and under date of August 6th, 1887, that is to say, nearly six years after the operations were performed, the child's father reports him in excellent health and with no sign of a return of the disease. In the history of the case nothing indicative of a hereditary tendency to the disease could be discovered. The parents have one other child, now four years old, whose eyes are thus far unaffected.

I regard the case as interesting, in that it tends to show that the prognosis in glioma is, perhaps, somewhat less unfavorable than we have usually considered it. In regard to bilateral glioma, Knapp, in his masterly work on intra-ocular tumors, says (page 121): "The uncertain result of this remedy can alone influence the physician not to request permission for the bilateral operation of extirpation."

Dr. D. Tench, in a valuable report on enucleation for various causes, which appears in the last (June) number of the *Archives of Ophthalmology*, says, in regard to some of the cases operated on for glioma: "The optic nerve was affected, thus dooming the patient, with almost absolute certainty; while in still two more cases the other eye had also become affected, making death only a question of time;" while Meyer, in his latest edition (Diseases of the Eye) says: "If the optic nerve is invaded by the degeneration, and especially if there are traces of it in the peri-orbital tissue, it is certain to return after a very brief interval."

In the subject of this report not only was the disease far advanced in both eyeballs, but also the optic nerve, of one side at least, and the peri-orbital tissues of the other, were in process of transformation into glioma, conditions all supposed to point to a speedy relapse, and yet we have here six years following the operations without a sign of recurrence.

DISCUSSION.

Prof. KEYSER remarked that it has always been taught that the enucleation of an eye with retinal glioma in a child is not sure to result in saving life, but only in prolonging it, as the disease is sure to attack the brain sooner or later. In most

Digitized by Google

instances death ensues in from four to eighteen months. It is not every case that looks like glioma of the retina which is true glioma. He has seen and operated in several cases apparently of undoubted glioma, most of which have succumbed in some months after, two of which, however, are still living, in which six and seven years have elapsed after the enucleation without relapse. The one of six years had a sister who died five months after enucleation for glioma, from its extension to the brain, just twelve months previous to the operation on his patient. Yet it may be that the diagnosis in these two successful cases was not certainly correct, although the microscope showed the characteristic cells etc., found in glioma. He advised operation with great caution as to diagnosis.

Mr. POWER, of London, said he doubted exceedingly whether genuine cases of glioma were relieved by operation.

Dr. DUDLEY S. REYNOLDS said he thought where the disease originates in the brain or at any point in the optic nerve, enucleation could, of course, do no good; but where the disease originates in the retina, the case is different. There are many well-authenticated instances of permanently successful results from enucleation. Out of a large number of cases—perhaps twenty—he knows of but one instance in which the disease appeared in both eyes simultaneously. This was the case of Anna Lewis, a girl nine years of age, who, being blind and having a golden reflex from the pupil, was said to have cataract. She was sent to the Indiana institution for the education of the blind. In June, 1887, her aunt brought her to Louisville. She had for two or three weeks suffered severely from headache, and the blindness, which was limited to the central part of the field at first, had recently become complete. Both eyes were sensitive to pressure, tension very great. She was put under the influence of chloroform; and when I enucleated, it was found the disease did not extend—at least, apparently—beyond the globe. A little more chloroform was administered, and the other eye enucleated before the patient recovered consciousness. It was found precisely like the one first removed. Sections through the projecting end of the nerve near its point of contact with the sclerotica were made, and nothing abnormal observed. The retinæ in both eyes had been almost entirely incorporated in the morbid growth. Sections of this growth were made; and I still use them as typical examples of glioma. Miss Lewis has never had any return of the disease, and has ever since enjoyed excellent health. I take this to be an extraordinary case, both in its nature and the result of the operation. It is sufficient to decide my mind in favor of enucleation in every case where I have reason to believe the disease does not extend beyond the orbital cavity.

Dr. GALEZOWSKI remarked, I have seen perhaps four cases of retinal glioma in my practice. In one of these cases I enucleated, and the patient is now living, after six years. I have seen one case of glioma in both eyes, but here I did not operate, and the child died.

Dr. RANDOLPH, of Baltimore, Md., said, *apropos* of Dr. Reynolds' remarks as to the origin of glioma, whether in the retina or optic nerve, I had an opportunity not long since of examining a glioma which evidently had its origin in the optic nerve, the entire structure of the nerve being virtually destroyed, the nerve fibres being pressed apart and densely infiltrated with round cells, the characteristic cells of this strictly sarcomatous growth.

Digitized by Google

ERGOT OF RYE IN OPHTHALMIC PRACTICE.

L'ERGOT DE SEIGLE DANS L'OPHTHALMIE PRATIQUE.

MUTTERKORN IN DER OPHTHALMOLOGISCHEN PRAXIS.

BY H. CLAY PADDACK, M.D.,

Of New York City.

This valuable agent was not used by the ancients. The most complete of all our earlier writers on materia medica note none of the medical virtues of ergot of rye. We are now all familiar with the use of it in obstetrics, but popular tradition had then taught a few empirics its virtues in the practice of Midwifery, but this remedy, destined to become one of the most valuable, was not understood by science. It has been thought, of all the properties of ergot, the most important and incontestable was certainly that of producing contractions in cases of uterine inertia, even at present, in spite of the obstinacy of a few physicians who deny to ergot the possession of properties almost as evident as those of cinchona. As the value of this remedy is generally admitted in inertia of the uterus, it is indeed as certain to play as important a part in the practice of ophthalmology. I shall not take up the physiological action of the drug; it is too well known, as far as it acts on the unstriped muscular fibre of the uterus, but shall add, it has lately been proved beyond a doubt that it also acts on the blood vessels in general, and those of the brain and spinal cord, as well as the uterus, in particular. Full doses unquestionably diminish the frequency of the pulse slightly; but even large doses fail to reduce it below sixty. The chemistry of ergot is not at present understood. Those who have made ophthalmic practice a study will without doubt welcome any remedy that will have an influence over some of those diseases due to congestion and over-distention of the arteries, capillaries and veins; a per cent. of those affections we know are only slightly if at all controlled by any local or general treatment, in the present state of our knowledge. Dr. Sœlberg Wells says of "episcleritis:" The disease "is very protracted and obstinate in its course, also very little influenced either by the general or local treatment." All of those diseases of the eye that are due to congestion and inflammation are speedily controlled by the use of ergot of rye. Where this remedy is most valuable is in diseases of the conjunctiva, cornea, sclerotic and retina, having as a prime symptom congestion, of an acute or chronic character. Ergot not only has an influence on the maternal and placental circulation, but it has also as positive an influence on the blood vessels of the brain and spinal cord.

Dr. William A. Hammond says:—

"As is well known, ergot possesses the property of constricting the organic muscular fibre. This property has for several years past led to its successful application in the treatment of those diseases of the spinal cord in which it is desirable to lessen the amount of blood in its vessels. It is only lately, however, that this agent has been employed in similar affections of the brain. From my own experience, as well as for the consideration of the investigation of others, I am entirely satisfied that ergot does contract the cerebral vessels, and hence that it diminishes the quantity of intercranial blood; it causes contraction of the arteries of the retina, and loss of capillary tint in the disc."

Now, it must be admitted that the chief difficulty in many of those obstinate diseases of the eye, especially in promoting resolution, is the congestion and over-distention of the blood vessels, that fail in many cases to reduce their calibre, often where internal medication is administered, with the local application of the various astringents. There are, indeed, many cases that fail to respond even to heroic treatment. In ergot we have, we believe, the essential factor to contract the over-distended blood

Digitized by Google

vessels, giving them tone and also acting as a tonic to the nerve supply of the organ, thereby promoting resolution in many cases so speedily as to appear almost magical in its effect. From what has been said it will be easily seen that there is nothing empirical in the use of this new remedy in those affections of the eye that apparently have so little tendency to improve under other methods of treatment. Certainly this must be admitted as a rational remedy, and it has been used quite long enough, at least, to have its action on certain blood vessels established beyond a doubt, and nothing more is claimed than the therapeutic action of the drug, that is well known in obstetrical practice. Its action on the cranial vessels is just as apparent to an observer. I will give you a few cases to show what result I have obtained from its use since I first discovered its value in diseases of the eye, seven years ago, this having been the first, as far as I know, of its being used in the treatment of the eye troubles. I now offer it as an addition to the large list of new remedies.

CASE I.—C. D., age thirty-five, a blacksmith, was struck in the right eye, at the margin of the cornea and sclerotic, with a small piece of hot steel from an anvil. The effect was not severe at the time, but it produced a slight irritation. Thinking it better to consult a physician, he applied to an oculist, who used for two sittings, of one hour each, alternate applications of water as hot as could be tolerated, and then immediately applying it ice cold. This was used for two consecutive days. At the end of that time there was a fully developed attack of episcleritis, with the most severe ciliary neuralgia I ever witnessed. The eye was so congested that it was crimson in color, then there appeared small, dusky-red and yellowish-red elevations on the sclerotic a short distance from the edge of the cornea. The suffering was so intense that powerful anodynes were resorted to to quiet the pain. I treated this case for five months according to the latest authorities. At the end of that time there was no improvement. I had able counsel to see the case, but all we tried was of no avail. The only relief for the time was by opiates and leeches to the temple. There was such an intolerance of light that the patient was compelled to be confined in a dark room most of the time. After trying everything known to the profession I concluded the only way to cure the case, and I, indeed, had no faith in its being cured at all, was, by some heroic means yet untried, to reduce the enlarged vessels. This, astringents and other remedies, locally, failed to do, with tonic and alterative treatment internally. I conceived the idea of using ergot, as a last resort, thinking possibly it would do something toward improving the symptoms for a time, at least, even if there was no permanent benefit. I gave the patient four grammes three times daily, suspending all other treatment. On the second day there was marked improvement. Indeed, considering the severity of the case, there was a miraculous change for the better. After the fourth day only two grammes were given at a dose, three times a day, and then discontinued at the end of ten days, and the patient was given an elixir of iron, quinine et strychnia, also cod-liver oil as a tonic. The neuralgia abated. No more opiates were now given, and in one week's time the patient was able to go about town and see his friends. In six weeks from the first dose of ergot the disease was completely arrested and he was able to resume work.

CASE II.—Mrs. Van A., age thirty, was suffering from chronic conjunctivitis, iritis with ulcers of the cornea and ciliary neuralgia. She had been treated by several different physicians with the usual treatment, for six months before I saw the case. I gave her first alteratives and tonics internally and applied three-grain solution of atropia locally, with mild astringents. She was much reduced in flesh and strength. The case improved slowly for some weeks and then came to a stand. I tried other lines of treatment with no better result. I then determined to again try ergot but to continue the tonics also. There was an improved condition of the eyes on the following day, and with this treatment the congestion was speedily reduced. I stopped the ergot on the tenth day, and with the atropia I soon finished successfully this unpromising case.

Digitized by Google

CASE III.—Mrs. B.; age twenty-five; she had first an attack of conjunctivitis for some months, having been treated by her family physician with a solution of sulphate of zinc et morphia, also argenti nitrate. When I saw the case there was conjunctivitis, iritis with retinitis and ciliary neuralgia. She was also very anæmic. I gave her syr. ferri iodidum, fifteen minims, with cod-liver oil and local applications of a three-grain solution of atropia, twice daily, and a generous diet. There was marked improvement at first, although it was gradual for four or five weeks. Then there was no further advancement to be observed; and the patient feeling somewhat discouraged, I used the ergot as before, and the change for the better after its use was daily visible. In a few weeks the patient was discharged cured.

In all those cases there was congestion and over-distention of the blood vessels, which we frequently find so difficult to cure. I have since continued daily to use the ergot in my ophthalmic practice, with a success in treating many cases that I have been unable to receive from any other line of treatment. I will close with a few directions regarding its use.

1. A reliable preparation of the drug must be obtained. We certainly could not expect success from an inert article.

2. It should be given in maximum doses, and in bad cases for several days, and then diminish the quantity or stop it for a time.

3. In cases accompanied with anæmia and general debility also give tonics, with cod-liver oil, a generous diet and fresh air.

4. Atropia must be used when cases are complicated with iritis,

5. Cases of a syphilitic origin, or complicated with, must have treatment also for that disease.

Mr. H. POWER, of London, read a paper by Mr. P. H. MULES, of Manchester, England, on—

USE OF THE ARTIFICIAL VITREOUS AFTER EVISCERATION.

L'USAGE DES VERRES ARTIFICIELS APRÈS L'EVISCÉRATION.

ANWENDUNG DES KÜNSTLICHEN GLASKÖRPERS NACH DER EVISCERATION.

The battle between Evisceration, Enucleation and Neurectomy yet remains to be ought out, and a large mass of facts must be accumulated before either the one or the other can defy deposition.

My object in this paper is—so far as we have at present accumulated these facts—to lay before you the result of present experience of the permanent impaction of the glass globe into the denuded sclera.

We may take for granted, that if no ill effects follow the use of this globe, and if the pain and discomfort after the operation are no greater than after enucleation, the result obtained is immeasurably superior. To quote from an article in the *British Medical Journal*:* "No one who has performed this operation or seen its results, can fail to appreciate the immense advantage it gives from a cosmetic point of view." Apart from this, the cavity remains clean and free from muco-pus, with numerous other advantages already treated of in papers on the subject.†

---

* June 4th, 1887, p. 1230.

† *British Medical Journal*, Dec. 19th, 1885.

Digitized by Google

Do we gain these advantages from evisceration alone, without the use of the glass globe?

Partly, yes.

The cavity is clean and free from discharge. No cicatricial bands form, but as time progresses the orbital fat shrinks and a cavity is formed, not one whit less deep than after enucleation, projecting from the centre of which is a button of sclera the size of a large pea. I have taken careful measurement of the orbital cavity of a man whose sclera, eleven years ago, was apparently perfectly eviscerated, how I do not know, and I find it as above stated; it is very easy to understand this; first the optic nerve shrinks; secondly, there is obliteration of vascular and lymph channels; thirdly, there is, the most important part of all, "absorption of orbital fat from the alteration of muscle insertions" and secondary contractions depending on scleral shrinkage. With the insertion of the glass globe you avoid all this; instead of changes of shape progressing in the cavity for an indefinite time, necessitating constant alteration of artificial eyes, you have in the course of three months the form as it intends permanently to remain, and a beautifully rounded stump, two-thirds the size of the normal, is presented for the adaptation of the artificial eye.

There remains the degree of pain and distress of evisceration as contrasted with enucleation.

This was undoubtedly a formidable fact; we have records of some eighty cases of all ages, and here I include the simple eviscerations of which we have done few, but as far as pain is concerned, it makes no difference whether the globe is introduced or not, the pain and constitutional distress being severe for three or four days. In none have we had suspicion of meningitis, or indeed any cause for uneasiness, yet it is this that has militated against the universal adoption of the operation. But having lately established the "raison d'etre" of the pain, as a pressure, not an inflammatory one, the remedy was not far to seek, and I found it by deeply draining the sub-scleral space with antisepticized horse-hair loops introduced through the conjunctiva to the outer side of the globe, and allowed to discharge through a divided canthus, and the application of an ice bag. *Vide British Medical Journal,* June 4th, 1887, p. 1213. By this means all pain and constitutional disturbance is avoided and, if necessary, the patient can be discharged on the third day.

We have, as before stated, performed evisceration in some 100 cases, of all ages, and have modified the operation in a few points.

Silk sutures should be used instead of catgut; even then a small percentage will fail to unite primarily; in these I remove the glass globes, treating them as simple eviscerations.

One sclera only reopened after four months completion of union: this was in a woman of drunken and immoral habits. Twice I have seen sloughing scleræ from want of care in antiseptics.

My first case, operated upon two years and seven months ago, is still happy in a perfectly fitting glass eye.

I do not know one instance of sympathetic irritation after the removal of the globe contents which could be fairly attributed to it, and I still hold by the view that I have published elsewhere*, that in evisceration we have an absolute prophylactic against sympathetic disease.

It only remains for me to urge those who have not adopted this treatment to a trial thereof, and an insistance that the artificial eye makers shall properly second their efforts by a well fitting shell.

In July of the present year, Cross, of Bristol, brought before the Ophthalmological

‡ *British Medical Journal,* Feb. 6th, 1886.

Digitized by Google

Society of Great Britain two cases—the only ones on record—entitled "Sympathetic Ophthalmitis occurring after Evisceration." He classified them with those where sympathetic ophthalmitis commenced after enucleation. Holding the opinions expressed above, I agreed, that if sympathetic in their character they would come under the class named, but I dissent most strongly from the view that they were necessarily sympathetic, solely because a simple, easily cured iritis followed an accident to the fellow eye; holding it to be illogical that an accident to one eye shall alone determine whether or no simple inflammation of its fellow be sympathetic; both the recorded cases recovered rapidly and perfectly, one within ten days. Cross also expressed an opinion, in which I fully concur, that the worst eyes for evisceration are those lost from slow irido-choroiditis with tendency to flushes and recurrent inflammatory attacks; these always give trouble, more or less pain and chemosis; in these the drain must be allowed to remain at least a week, as we find early withdrawal followed by rapid swelling and pain.

I need say nothing to those who have given their patients the advantage of this operation, for I believe they will be loth to return to the less satisfactory methods of enucleation or of neurectomy: and lastly, we have opened a road for interesting physiological research by establishing the tolerance of foreign bodies, when judiciously used, in the cavities of the human body.

DISCUSSION.

Mr. Cross, of Bristol, Eng.—The operation devised by Mr. Mules must be admitted to possess many of the advantages claimed for it; it retains a large and prominent stump which is controlled by the muscles in a natural way. The muscles, therefore, with their nerves, are kept in a state of functional activity, and the general atrophy of the contents of the orbit that follows any other operation of this class is prevented. The movements of the glass eye upon the stump, however, are not commensurate with those of the stump alone, and some shrinking of the orbital fat and tissues occurs.

The operation is admirable in conception and will, I believe, be increasingly useful in suitable cases. I protest, however, against the view that it is a prophylactic measure against sympathetic ophthalmitis, or that it can be compared with enucleation of the globe as a safe procedure where the reason for operating is to save the sight of the fellow eye. Admitting that septic uveitis is the prime factor in the occurrence of sympathetic ophthalmitis, yet the researches of Brailey, Snellen and Leber show that the septic elements soon pass into the sclerotic and the lymph spaces outside it. The cases of sympathetic ophthalmitis that have occurred some time after the eye has been enucleated indicate that at the time of the operation the morbid process had already passed beyond the piece of optic nerve removed with the eye. In the question of the prevention of sympathetic ophthalmitis, the character of the stump left by the operation goes for nothing. The sole consideration is for the remaining eye.

Enucleation gives the most complete removal of the irritating tract, with immediate physiological rest, and thus tends, without delay, to resolution of the morbid process, where this might have advanced beyond the part removed.

Evisceration is likely to leave behind particles of the uveal tract at the vena vorticosæ or around the disc, and leaves danger behind with the sclerotic. *The introduction of the artificial vitreous* is usually followed for a few days by considerable inflammatory reaction, which, by reducing the vitality of the sclera favors the progress of the special morbid conditions which may have been left behind, or it may be a

Digitized by Google

direct danger in itself. The report of Mr. Mules' operations, however, shows that unfavorable results rarely occur. In my two cases* the healing had resulted in a small sinus, and there followed, seventeen days after the operation in one case, and twenty-one days after it in the other, a condition of the remaining eye that was considered by my colleagues in the Society to be sympathetic ophthalmitis. The removal of the artificial vitreous was at once followed by improvement, and both cases recovered after several weeks of what I feel sure was definite (though not malignant) sympathetic ophthalmitis. I did not directly attribute this to the operation, but consider that had enucleation been performed instead, no such circumstance would have occurred. The possibility of a sinus becoming established later in the thin covering that lies between the artificial vitreous and the glass eye, ought to be recognized, and cannot be entirely devoid of danger. In such cases as progressive buphthalmos, and in recent wounds that require removal of the eye, where no inflammation is present that threatens its fellow, the operation may be performed, but where the damaged eye is threatening to affect its fellow it should be looked upon as a morbid centre that requires thorough and radical removal.

Professor GALEZOWSKI, of Paris, said—I think evisceration is not the best proceeding. The artificial vitreous should never be introduced except in successful enucleation. The presence of an artificial eye upon the wounded structures lying about it must be a cause of irritation. I think enucleation is far more simple and advisable. I recall two cases. One operated upon several years ago by Prof. Richet. A violent inflammation ensued but the patient recovered. The stump, however, remained always red and injected, and finally, the patient, being the entire time a sufferer, came to my clinic. I cut out the stump and the patient never had any uncomfortable sensations afterward. A few months ago another patient presented himself in my clinic. He had undergone an evisceration some time previous. The stump was intensely sensitive and contained a fistula. I was not many moments in deciding to get rid of the stump, which I took out. Immediate and permanent recovery followed. In short, with this operation we have very much the effect of a foreign body introduced into the cavity of the eyeball. I advise and practice enucleation in every instance.

Dr. BAKER mentioned a case where he introduced a glass button with the most satisfactory result. The button, after remaining in for some time, fell out, from an accident, and the eye was lost.

Dr. KEYSER mentioned a case of sympathetic ophthalmia following an enucleation which left a small piece of the sclerotic attached to the optic nerve. He thinks that evisceration should never be done in eyes lost from inflammation.

Dr. EUGENE SMITH said that he had seen the happiest results from Mr. Mules' operations. Whenever evisceration had been done he always favored the introduction of the artificial vitreous.

Mr. POWER, of London, said—I have performed or have seen the operation performed about twelve times. I eviscerate, wash the cavity of the sclerotic with strong boracic acid, and wait till all bleeding has ceased. Then introduce the glass ball, over which the sclerotic is brought and the edges of the cut sclerotic carefully sutured with carbolized gut. There should be no tension. The edges of the sclerotic should fit accurately. The ball should be replaced by a smaller one if there is the least strain on the sclerotic in bringing the edges together. The conjunctiva, which

* Opthalmological Society *Transactions*, London, 1887.

Digitized by Google

has been first separated a little way from the sclerotic, is then brought together over the sclerotic wound, so that this wound is rendered subconjunctival; before closing the lids the whole conjunctival sac should be carefully washed out with boracic acid. I have seen one case of suppuration of the globe, one of separation of the lips of the conjunctival and scleral wounds and consequent exposure and necessary subsequent removal of the glass globe; and two or three attended with severe inflammation, but I think in my latest cases, performed with more care, these results have been much less frequent.

FURTHER CONTRIBUTIONS TO THE STUDY OF SYMPATHETIC OPHTHALMIA.

NOUVELLES CONTRIBUTIONS À L'ÉTUDE DE L'OPHTHALMIE SYMPATHIQUE.

WEITERE BEITRÄGE ZUM STUDIUM DER SYMPATHISCHEN OPHTHALMIE.

BY HAROLD GIFFORD, M.D.,
Of Omaha, Nebraska.

That the bearing of this paper may be better understood, I take the liberty, at the outset, of recapitulating the main points which I have previously advanced in this connection. These are—

1. That in several of the lower animals a lymph current passes out from the vitreous chamber along the sheaths of the retinal vessels, leaving the optic nerve, for the greater part, with these vessels, and passing with them to the posterior end of the orbit, there, presumably, enters the cranial cavity through the sphenoidal fissure. This current I have called the posterior vitreous stream.

2. That in rabbits, anthrax bacilli, when introduced into the vitreous of one eye, take the paths just described in passing to the cranial cavity, and from there are washed down between the optic nerve sheaths to the peri-choroidal space of the second eye, by the current which flows from the cranial cavity into the sub-vaginal space of the optic nerve.

3. That leaving the disputed phagocytosis out of consideration, the choking up of the lymph channels, which, in rabbits, frustrated my attempts to confirm the results which Deutschmann* obtained with ordinary pus cocci, probably explains the non-occurrence of sympathetic trouble in many cases where one eye is injected, especially where the primary inflammation is violent, as in panophthalmitis.

4. That in many, if not all, normal eyes the conjunctival sac contains pathogenic bacteria, capable of producing severe inflammation when introduced into the cornea, anterior chamber and vitreous of rabbits, and that the ordinary antiseptic precautions, such as washing out the sac with sublimate solutions, even in the strength of 1 to 2000, by no means suffice to eradicate these bacteria from the sac.

I. THE EFFECT OF OPTIC NEUROTOMY AND NEURECTOMY ON THE POSTERIOR LYMPH STREAM.

That a posterior lymph stream exists in man, as in the lower animals, is altogether probable; whether, however, the bacteria which cause sympathetic ophthalmia leave the optic nerve with it, as do the anthrax bacilli in rabbits, is by no means so certain, but until the question is decided in the negative it will be necessary to take account of it in estimating the value of some of the substitutes for enucleation. To estimate the

---

* *Arch. of Ophthal.*, 1886, 2 and 3.

Digitized by Google

value of optic-ciliary neurectomy on this basis, therefore, the following experiments were performed:—

On five rabbits, after cutting the superior rectus, the optic nerve of the right eye was cut, about 4 mm. back of the eye, and a piece about 1 mm. long was then cut off from the peripheral stump, the wound in the conjunctiva being closed in the usual way. On one rabbit simple optic neurotomy was performed on one eye; on another on both eyes. After from nine days to three weeks a drop of sterilized suspension of India ink was injected into the vitreous of three of the neurectomized eyes, through a scleral incision; in the vitreous of the other two a fine glass rod, carrying a small clump of anthrax spores covered with several dried layers of flour paste, was broken off. Of the neurotomized eyes two received a drop of India-ink emulsion; the other an anthrax rod. The anthrax rabbits died, five, six and seven days respectively after the inoculation; the India-ink animals were chloroformed from twenty-one to forty-one days after the injection into the vitreous.

The results of these experiments were so uniform that it would have been simply a waste of time and material to increase the number. In the India-ink rabbits the pigment grains were observed during life collecting on the papilla, just as occurs in normal eyes. Microscopically, the cut ends of the nerve were found one to two mm. apart, with no indication of any attempt at reunion, even in those eyes where the nerve was simply cut. The India ink was found to have passed out from the vitreous through the central canal into the orbit, precisely as in normal eyes. In the anthrax rabbits, also, the bacilli took the same course through the nerve into the orbital tissue, and in the case of one neurectomized rabbit they had passed over into the sub-vaginal space of the second optic nerve; the only difference between this and the other cases of transmigration, which I have previously reported being that in the latter the bacilli were carried down from the cranial cavity into the sub-vaginal space of the infected eye, as well as into that of the second eye; whereas, in the neurectomized rabbit the central end of the cut nerve was free from bacilli. Whether this was an accident or a reliable indication that the current from the brain to the eye does not continue after a section of the nerve, future investigation must determine. The point demonstrated by these experiments, therefore, is that in neurectomized rabbits the posterior vitreous stream still persists; and whether we eventually accept the sheaths of the optic nerve, or the sheaths of the orbital vessels as the path taken by the microbes from the eye to the cranial cavity of man, the fact that in rabbits optic neurectomy is no bar to the passage of bacilli from one eye to the other should, until we have some positive evidence that this cannot occur in man, restrict the field of the operation to those eyes where there is no active infection.

II. THE ANTERIOR LYMPH OUTLETS AS CHANNELS FOR THE TRANSMISSION OF SYMPATHETIC TROUBLE.

The experiments of Deutschmann and my own have hitherto considered only the passage of microbes backward from the vitreous; but in a large proportion of cases sympathetic ophthalmia results from wounds which do not infect the vitreous at all. With this in mind, the writer has made a number of experiments on the anterior lymph outlets, of which I reserve for another paper such details as are of no immediate interest for our subject.

The experiments of Knies,* Weiss,† and Ulrich‡ have gone furthest in following the anterior outflows, but they are open to certain theoretical objections on account of their having used diffusible solutions. Those of Brugsch,§ on the other hand, were made with suspensions of India-ink, but they were carried out under the discouraging

* *Arch. f. Augen- und Ohrenheilk.*

† *Arch. f. Ophthal.*, XXV, 2.

‡ *Arch. f. Ophthal.*, XXVI, 3.

§ *Arch. f. Ophthal.*, XXIII, 3.

Digitized by Google

influence of Leber's idea that there were no important outlets for the aqueous beyond that into Schlemm's canal, and they led only to the conclusions that there might be an outlet to the sub-conjunctival tissue along the anterior scleral vessels, and that leucocytes take up the pigment granules and carry them into the tissues of the iris, and, to a slight extent, far into the choroid.

My experiments were made on white rabbits, and consisted in the injection into the anterior chamber of suspensions of India ink, of cinnabar, and of anthrax spores. The injections of pigment, instead of being made directly through the cornea, as in Brugsch's experiments, were made by passing a fine glass tube into the anterior chamber through an incision entering the posterior chamber through the ciliary body, the advantages of this incision being that but little aqueous is lost and that whatever is put into the anterior chamber stays there, the iris preventing any outflow through the wound.

The results of the pigment experiments were invariably as follows: The mass of pigment passed quickly into the spaces of Fontana, from here passing out (1) into the tissues of the iris; (2) in a fine line just posterior to the membrane of Descemet, into the cornea; (3) in large quantities along the anterior scleral vessels to the sub-conjunctival tissue; (4) in very considerable quantities into the loose tissues of the ciliary processes, and from here passing back into the choroid; in some cases showing a tendency to collect along the layer next the sclera, but in others occurring all through the thickness of the choroid. In the anthrax experiments the courses taken by the bacilli were essentially the same as those just indicated, with the exception that in the case where they had penetrated furthest back they were not scattered through the whole thickness of the choroid, but lay almost entirely in the peri-choroidal space. The tissues of the iris, moreover, were entirely free from bacilli, except such as lay within the vessels. The loose tissues of the ciliary processes and of the sub-conjunctiva seem to offer the bacteria an especially favorable breeding-ground, for here they were crowded together in immense bunches. Whether the pigment and bacilli were carried into these channels by any decided stream I cannot positively decide. My impression is that from Fontana's spaces to the choroid, and along the vessels leading to the sub-conjunctival tissue, there are streams, while the non-occurrence of the bacilli in the tissues of the iris indicates that the pigment occurs there simply through the agency of wandering leucocytes. Be this as it may, the important point for the present subject is that a comparatively free path for the microbes exists between the anterior chamber and the ciliary processes and choroid on the one hand, and the sub-conjunctival tissue on the other. From the choroid the microbes can, of course, pass into the pial sheath, and reaching from this the sub-vaginal space, they fall under the influence of the posterior vitreous stream, which, it should be remembered, sucks out the contents of the sub-vaginal space as it passes out of the nerve with the retinal vessels. With regard to the communication between the anterior chamber and the sub-conjunctival tissue, it might at first thought seem hardly worth considering in the present connection. Yet it is worth noting that the current in Tenon's space is sharply toward the cranial cavity, as the experiments of Knies with diffusible solutions indicated, and as I have proved in a number of experiments on rabbits, dogs, guinea pigs and rats. In all of these animals I have brought semi-solid India ink under the conjunctiva, with the invariable result that the granules that did not remain fixed at the point of introduction were swept back to the posterior part of the orbit, where they were joined, in cases where the vitreous had been injected as well, by the particles brought through the nerve by the posterior stream. There remains to be considered the possibility of the anterior lymph channels being infected by microbes passing forward from the vitreous chamber. A current forward around the lens undoubtedly exists, and, as Ulrich* has shown in one case of a

* *Arch. f. Ophthal.*, xxx, 4.

Digitized by Google

rabbit with an adherent leucoma, and as my own experiments have shown me in normal eyes, this current will carry pigment particles through between the meshes of the zonula into the posterior chamber. The results which I have obtained by injecting bacteria into the vitreous indicate, however, that except when the zonula is broken down, either by violence or the intensity of the inflammation, no passage of microbes occurs from the vitreous anteriorly.

In some forty cases in which I have injected anthrax spores into the vitreous of rabbits the bacilli were frequently found crowded against the posterior surface of the zonula, but the only case in which they passed forward around the lens was one in which the lens had been dislocated at the time of the injection. The pus cocci also seem to be held back by the zonula as long as the latter remains intact.

Turning now to more theoretical considerations, I appreciate fully that in sympathetic ophthalmia the passage of microbes to the cranial cavity along the orbital vessels is nothing more than a possibility which the facts observed in rabbits make not altogether improbable. The main difficulty in the way of this, as of the other modifications of the germ theory, is the non-occurrence of brain symptoms, and it is rather less difficult to understand why bacteria entering the cranial cavity through the sphenoidal fissure should be kept from spreading over the base of the brain by the strong current flowing down between the optic sheaths than why they should not keep right on into the tractus, and over the whole pia mater, if they pass up through the substance of the nerve and in the pial sheath, as is held by Knies and Deutschmann,* for in these tissues the current, if any of importance really exists, seems to be toward the brain.

The other important objection to accepting the passage of microbes along the sheaths of the orbital vessels, namely, the non-occurrence of orbital symptoms, I have answered elsewhere. One swallow does not make a summer, nor does one coccus make an abscess, nor do many hundreds, unless the conditions are favorable. The well-known work of Wyssokowitsch † shows that large quantities of pus cocci can circulate in the blood and be stowed away in the liver, spleen, and bone marrow, without causing any disturbance, unless some trauma or derangement of the circulation give them a starting-point; and Grawitz has shown that relatively immense quantities of these microbes can be absorbed safely by the subcutaneous tissue of the lower animals, unless some favoring condition, such as a trifling bit of necrosis in the injection wound, give an impulse to their development. We need not wonder, therefore, if microbes, in small installments, wander harmlessly through the orbit, although the same organisms, when collected in the uveal tract, may cause serious inflammation.

In conclusion, let me urge that in our haste to get rid of the old ciliary theory we should not lose sight of the probability that while genuine sympathetic inflammation is only caused by germs, its progress may be hastened or favored by reflex irritation from the other eye. The germ theory alone can hardly account for the numerous cases of immediate improvement of a genuine inflammation, far advanced, which have followed enucleation of the first affected eye. Just as the vasomotor disturbance caused by a chill may give the bacteria in the nose a chance to cause an acute coryza, or the pus cocci in bone marrow an opportunity for an osteo-myelitis, so, in all probability, can the reflex influence of an irritated eye favor the starting up or the progress of sympathetic disease in the fellow eye.

Finally, it seems worth while to correct a prevalent misconception of the position of Mackenzie to the current theories of sympathetic disease. He is commonly referred to as the author of the optic nerve theory, and this theory, as at present held, seems to be regarded as simply a revival of his, with the addition of germs. How far, however, Mackenzie's idea was from the theory of Kniess, who, so far as I know, was the first

* *Zeitschr. f. Hygiene*, I, 1.

† *Virchow's Arch.*, 108, 1.

Digitized by Google

to advocate the spread of the inflammation by the continuity of tissue, is best shown by his own words. In his "Diseases of the Eye" (Am. Ed., p. 597), he says: "It is extremely probable that the retina of the injured eye is in a state of inflammation which is propagated along the corresponding optic nerve to the chiasma, and that there the irritation which gives rise to inflammation is reflected to the retina of the opposite eye, along its optic nerve." It is thus evident that Mackenzie's was but an unscientific substitute for the later theory of Arlt and von Graefe.

RETINOSCOPY.

RÉTINOSCOPIE.

ÜBER RETINOSCOPIE.

BY F. RICHARDSON CROSS, M.B., F.R.C.S.,

London, England.

A large majority of the patients who apply to the ophthalmic surgeon are suffering from errors of refraction, and there is no more important subject in the whole range of ocular therapeutics than the proper application of spectacles.

My apology for venturing to make any remarks upon so well worn a theme is, that one of the subjects suggested for consideration in the scheme framed for the work of this Section was, "The relative value of Retinoscopy in the diagnosis of refraction cases."

Accuracy is the main point required, but saving of time is important, especially in hospital work.

In order to make a scientific diagnosis of refraction, an *objective* method of testing is essential; but the correcting lens which is suggested by a mathematical deduction from the conditions of the eye as an optical instrument is not always the one that produces the best acuity of vision, nor that gives the greatest comfort to the wearer. Allowance must be made for the individuality of the eye, and subjective testing with types and lenses must control the results of the objective method; the patient's judgment must be exercised also, as to which is the best glass for his purpose; so that the surgeon may have a variety of evidence upon which to base his conclusion in prescribing spectacles.

In very young children we are entirely dependent on the *objective* method, and with stupid or illiterate persons, or with very amblyopic eyes, its advantages are self-evident.

For estimating astigmatism (which is nearly always dependent on an error of the cornea) Javal's optometer is an excellent instrument.

Direct examination of the eye by the ophthalmoscope and retinoscopy are much more general in their application, and require no special apparatus. The first of these two is a most valuable test for estimating refraction, but I think it is difficult to apply in the lesser degrees of ametropia and in astigmatism.

The *shadow test* is based upon the fact that under ophthalmoscopic examination the appearance of the illuminated pupil alters in a certain and definite way, according to the position and movements of the mirror. (Bowman, Cuignet).

The eye which is being examined must look well into distance, so as to relax accommodation; this condition is not fulfilled if the pupil varies in size; tonic or clonic spasm requires the use of atropine; if the pupil is merely small, cocaine is sufficient.

On rotating the mirror the light shifts, and it is seen to be edged by a shadow,

Digitized by Google

which crosses the pupil in a direction that is constant, either with or against the mirror.

Whatever be the refraction of the eye, there is always produced at its fundus an inverted image of the light in the focus of the mirror; the image is brighter as the retina is more nearly in the conjugate of the light, and it must move in an opposite direction to the light and to the focal point of the mirror. The illuminated image at the fundus of the eye is *directly* seen on the retina of an eye which is either emmetropic, hypermetropic, or slightly myopic; it moves as the mirror is rotated, and is followed by its shadow, which goes in the same direction as a plane mirror, in the opposite direction to a concave mirror.

In its turn the illuminated retina reflects the rays of light which pass through the pupil and emerge at the surface of the cornea in a direction which depends upon the refraction of the eye.

The rays are parallel in emmetropia; in hypermetropia they diverge, more so as the refractive error is greater. But in myopia they converge, and thus form in the air at the far point of the eye a real inverted image of the illuminated retina. The production of this image at the far point of a myopic eye is the essential unit in retinoscopy. It gives rise to definite phenomena which must be briefly considered.

When the patient's myopia is of low degree, and his far point lies behind the observer's eye, the illumination of the retina *itself* is seen at the fundus of the eye; just as is the case in emmetropia and in hypermetropia, and it passes with its shadow in a converse direction to the focal light from the rotating mirror. If, however, the far point of the patient's eye lies in front of the observer a shadow is seen passing over the pupil just in the opposite direction to the former case. The shadow now seen is no longer within the observed eye but lies at its far point, around the real inverted image of the illuminated retina which is formed here in the air. The shadow obscures the illumination at the fundus by which it has been produced, and it necessarily moves in the opposite direction. (If the mirror used be concave the shadow goes with it, but it goes against the direction of rotation of a plane mirror.)

When, therefore, in myopia the examination by retinoscopy is made at a greater distance from the patient than the far point of his eye, the shadow around this far point is seen to cross over the pupil in the same direction as the focal light of the mirror. Under all other circumstances the shadow is seen at the fundus of the eye, moving contrary to the focal light.

Under one condition no appreciable shadow can be detected, but the retinal illumination seems very bright and stationary. This is when the surgeon's eye is close upon his myopic patient's far point and the one retina is at the conjugate focus of the other.

By the aid of a suitable lens any eye may be rendered short-sighted to any required extent, the distance of the far point or the degree of convergence of the rays being the resultant of the reparative index of the eye together with that of its modifying lens.

A method thus based upon sound optical principles—for the elucidation of which we are indebted to Landolt, Parent and Charnley—cannot but be of great practical value, and it has proved itself an easy, rapid and precise test in the hands of a large number of oculists.

Various methods, by *measuring from the eye under examination*, have been suggested by Chibret, Loiseau, Schmidt Rimpler, but retinoscopy is usually practiced at some constant far point to which the eye can be focused by trial lenses.

A distance frequently chosen is a little over a metre, giving a standard myopia of 1 D; this is not convenient if the ordinary box of trial sights is used; because every fresh adjustment requires the observer to leave his place. The use of Doyne's optometer meets this difficulty, and by means of Couper's new instrument, recently shown at

Digitized by Google

Dublin, the oculist can work at a distance of two metres and shift the lenses himself, thus reducing the standard ametropia to half a diopter. In reducing it further to —.25 D., at a distance of four metres, both illumination and trial lenses present difficulties. At or beyond a metre a plane mirror has advantages. I find it quite convenient to work at 80 cm., so as to hold the trial lens in front of the eye (13 mm.) and to be able to alter it from the box without shifting my seat. At this distance the ordinary ophthalmoscope mirror is the best; and the trial lens, which is neither too convex to turn the shadow with the mirror, nor too concave to give it an opposite direction, leaves a myopia of 1.25 D. (which is, of course, calculated in prescribing the spectacles).

The macula is the proper spot for estimation evidently, but this is found impossible without atropine.

The easiest is the disc, but it is unreliable, mainly because of the uncertain depth of the physiological cup; Morton has shown it to be approximately correct. I find the space between the disc and the macula practicable and accurate.

Like many others, I have employed retinoscopy very largely. I have no confidence in ordering spectacles without it. In private practice I always use it. At hospital every patient is tested by an assistant, with types and trial lenses, and he brings me the result written down. Presbyopia and the simpler cases are at once prescribed for, but any that are at all complicated I estimate by retinoscopy without atropine.

In hypermetropia a good deal that was "latent" is at once made manifest. Astigmatism and the direction of its meridians is detected.

The lenses suggested in myopia are frequently found to be too strong, etc.

As a rule, now, with the subjective test balanced by the objective, lenses can be safely ordered. But when it is thought necessary atropine is also used, and under its influence both retinoscopy and trial-sighting are again applied.

I find as a result that the lens, whether spherical or cylinder, which is based upon retinoscopy (an allowance of about —1.50 D. being made in each meridian) is quite satisfactory to the large majority of patients, so far as can be judged through those who remain under observation or who report themselves from time to time.

Out of a very large number I have kept short notes of *462 cases of retinoscopy* from which some deductions may be fairly drawn.

In *ten per cent.* the result of the test was *not quite* satisfactory; *i. e.*, the lens decided on for spectacles did not completely or fairly coincide with the deduction from retinoscopy.

This seems a large number, but the notes were made for no special purpose; *nearly 400 were examined without atropine;* in some the observation may have been inaccurate; others can scarcely rank as errors; moreover, many of the notes were kept because the cases were unusual.

In the remaining 413 the lens ordered for spectacles was the one that most nearly neutralized the shadow $\bar{c}$ — 1.25 D. (or $\bar{\bigcirc}$ — 1.50 D., or occasionally — 2D.) This lens often differed materially from that which seemed to give best sight at the earlier subjective testing. The patient's tendency is to choose a lens too — or not enough +.

In these days of "short-sighted education" it is important to separate excess of ciliary tone from myopia, and to give full help in ciliary strain. I believe retinoscopy may be fully relied on, and without atropine, even for the detection of slight errors of ametropia, where subjective testing alone may be very misleading. It is, however, especially valuable in the detection of astigmatism, together with the meridian of greatest error. A cylinder will usually be found of benefit in any case where retinoscopy indicates that astigmatism is present, and yet my notes show that out of the whole number—49—of unsatisfactory cases, 29 had reference to the use of cylinders. With few exceptions, however, the errors were very slight in extent.

In *101 cases of hypermetropic astigmatism*, as judged by retinoscopy, a cylinder lens

Digitized by Google

was of no help in *12* (in one of them the cylinder suggested was +2 D.). In *eight* the cylinder required to be slightly stronger or weaker than had been judged. In one a cylinder + 1 was of value though not estimated by the shadow test, — $\frac{21}{101}$.

The cylinder was also slightly misjudged in eight cases out of *90 patients with myopic astigmatism*, — $\frac{8}{90}$.

No rule can, I think, be drawn as to the value of using the weakest cylinders. Very frequently they may be disregarded, but they should always be ordered if they aid acuity of vision, or when the eye is hyperæsthetic. A colleague of mine is quite uncomfortable in far sight without —.50 D. cylinder. A cylinder —.25 makes all the difference to a lady, combined with sphericals, one spherical being only +.50. Slight hypermetropic astigmatism in children gives rise to the symptoms of short sight, and as concave lenses are found to improve distant vision, they are sometimes ordered instead of what is really needed—a convex cylinder. Retinoscopy prevents any such mistake. As to the use of atropine in retinoscopy, it can usually be dispensed with. It is of little help in aiding in the detection of astigmatism. No doubt it shows a certain amount of hyperopia beyond what is manifest to retinoscopy, but the latent defect is spherical and common to all meridians; there is no change in the estimation of the cylinder required. This fact is legible enough, for atropine affects the muscle of the lens, while astigmatism is an error of the cornea.

Atropine was used in *72* of the cases noted, usually in hypermetropia, and in some for a week or more.

In *eight* cases a modification of the strength of the cylinder resulted; in *two* a slight difference was shown in the meridians which before were similar, and in *one* astigmatism that had been noted disappeared. All these differences from the results of retinoscopy without atropine were slight in degree.

Astigmatism after atropine $\frac{1}{12}$.

As may be expected, a latent defect was shown by retinoscopy under atropine compared with retinoscopy without the drug. The latent hypermetropia thus exposed, however, was not marked in extent; it was *spherical* in increase, whether the eye was astigmatic or no.

In *23* cases the increase was 1 D.; in *five* cases it amounted to 3 D., and in *12* cases to 2 D. The difference was barely a diopter in *14* cases, and in *three* it amounted to 1.5 D.

In one pair of eyes dissimilar in refraction atropine showed an increase of hypermetropia alike in both eyes, of 3 D. in the horizontal meridians and of 2 D. in the vertical.

Atropine would seem, therefore, to be of little value in retinoscopy as compared with the importance it possesses in estimating refraction by subjective testing only; for without its use retinoscopy exposes a much higher degree of hypermetropia than is manifest to testing with trial lenses, and moreover, after its application no marked latent hypermetropia is, as a rule, found—no more, indeed, frequently, than represents the normal tone of the ciliary muscles.

The highest degrees of hypermetropia in my list were a pair of eyes estimated at 12 D. which required lenses + 10 D., and a pair of eyes 7 D. × 13 D., requiring a + 5 cylinder combined with + 5 spherical.

In twenty of the cases quoted a lens of at least + 5 D. spherical or cylinder was required.

In myopia I have estimated several cases over — 20 D. and have patients wearing — 20 lenses.

In one case estimated — 17 D. ⌒ — 21 D. lenses — 14 ⌒ — 4 cylinder are constantly worn, and pincenez lenses — 4.50 D. over these for improving distant vision, the combination being 22.50 × 18.50.

There were *243* myopes, of whom *90* had astigmatism.

Digitized by Google

Allowing for correction, retinoscopy may understate the concave lens which is required in order to produce perfect acuity of vision. A glass slightly too weak is, perhaps, an advantage. In four of my cases, however, I found that I had over-estimated the lens which gave the best sight.

In four cases of astigmatism a slight error occurred in the value of the cylinder. In four other cases the test did not seem satisfactory; the concave lens required for distance, through which also the patient read small type, was much in excess of that suggested by retinoscopy, but by this means considerable ciliary spasm was detected, and the value of the test still further shown.

Thus, scarcely two per cent. of failures occurred in testing myopia by this method.

It is well known that after iridectomy flattening of the cornea occurs, requiring the aid of a convex cylinder whose axis is at right angles to the direction of the coloboma.

In my records of several such cases the estimation of the astigmatism by retinoscopy is quite in accordance with the lens required for best acuity of vision.

RETINOSCOPY.

RÉTINOSCOPIE.

ÜBER RETINOSCOPIE.

BY A. R. BAKER, M.D.,
Of Cleveland, Ohio.

It would be an interesting study to determine some of the causes which lead the Medical profession to accept a new drug upon the most meagre testimony, when originating from the most obscure sources, while a new surgical procedure or instrument of precision in diagnosis is summarily rejected, and if ever brought into general use it is only after a long and persistent fight against the prejudices of the profession. This may be illustrated by the many long years which abdominal surgery took to gain the sanction of the Medical profession, compared with the almost universal acceptance and use of cocaine as a local anæsthetic within a few weeks. Or even with Jequirity, an agent quite as potent, and liable to do very great harm when used improperly, which the merest tyro in medicine does not hesitate to employ, and is used even by those who never dream of using the ophthalmoscope, of which it has been remarked, that with this instrument it is "like walking into nature's laboratory and seeing the Infinite in action," since by its means we are enabled to look upon the only nerve in the whole body which can ever lie open to our inspection under physiological conditions, and to follow in a transparent membrane an isolated circulation from its entrance into the eye through the arteries to its exit in the veins. We are further enabled to watch and study daily, or even hourly, morbid processes in each and every phase, from simple hyperæmia to absolute stasis, and from passive œdema to the most violent inflammation; while ofttimes through its agency also we get the first intimation of disease in remote and seemingly unconnected organs, so as to read, as if in a book, the written trouble of the brain, the heart, the spleen, the kidneys, and the spine.*

Although Retinoscopy has been used very extensively by general practitioners and specialists in England and France, it has been almost entirely ignored by German and

* Virchow. *Transactions* International Medical Congress, London, 1881.

Digitized by Google

American practitioners. While Cuignet, Parent, Chibret, Forbes, Morton, Hartridge, Juler, Landolt and a host of others, have written articles upon this subject, extolling its value and urging its superiority and adoption over other methods of determining errors of refraction, no less an authority than Dr. Hirschberg, of Berlin, remarked in a laughing manner, when the writer asked his opinion of the value of retinoscopy, "Oh, that is a lazy English method, and don't amount to much."

Dr. Loring, in his excellent text-book says, "that it is, in his opinion, the most difficult and least satisfactory of any of the methods of determining the refraction of the eye, and contributes nothing which cannot be more easily and more expeditiously performed by the upright image."

It is unfortunate for any profession to permit national or race prejudice to bias the discussion as to the merits of any scientific discovery, but the manner in which retinoscopy has been received shows that the medical profession have not risen entirely above these considerations. Retinoscopy was first brought to the notice of the profession by the French, and received by the English and used extensively by them. It was summarily rejected by the Germans and Americans without investigation.

It was a favorite theory of Bayard Taylor that the Americans were growing more like the Germans in their literary and scientific work and methods of thinking, if not in physical appearance.

This has been particularly true of the medical profession in this country, who have, in a manner at least, seemingly followed the profession in Germany, accepting what the German professor accepts and rejecting what he rejects. He rejected retinoscopy, and, true to the prevailing fashion, the American rejected it, without examining the subject for himself.

But this is not the only element which decided the question in the minds of such acute observers as Prof. Hirschberg and Dr. Loring. Prof. Hirschberg (if he will pardon the personal allusion) is one of the most accurate men in the use of the ophthalmoscope I have ever met. I remember, while in his clinic, in Berlin, in 1882, his examining a number of patients in the dark room and sending them out for me to prove his ophthalmoscopic findings with trial lenses and test type. The accuracy of his ophthalmoscopic readings, especially in astigmatic cases, was a revelation to me; especially as I had always been rather skeptical as to the ability of any one to determine accurately the amount of error in cases of astigmatism by means of the ophthalmoscope alone. But granting that Professor Hirschberg can determine the amount of error of refraction accurately by means of the upright image, it is not accomplished by every one; in fact, very few can do so. The same remarks will apply, to a certain extent, to Dr. Loring, together with the fact that his name is associated with one of the best refraction ophthalmoscopes made. This method of determining refractive errors has become somewhat of a hobby with him, and he could not easily be persuaded that there was a better and easier method.

I believe one of the great objections to the introduction of retinoscopy has been the belief that the present methods are good enough. It has been the aim of nearly every oculist in this country and Germany to learn to determine errors of refraction by means of the upright image. Theoretically, this would seem to be the most rational method, but practically, it has been found that few, if any, under forty years can practice it successfully, and that no one can do so unless he has a large amount of clinical material to keep him in constant practice. Another reason why retinoscopy has not been better received by the profession in America has been the unfortunate manner in which the subject has been presented to American readers. The only articles which have been published in this country (Dr. Swan M. Burnett's work on astigmatism has appeared since this was written) were that by Dr. Jackson, in the *American Journal of Medical Sciences*, June, 1880, and a brief paper in the *American Journal of Ophthalmology*, July,

Digitized by Google

1884, by the writer. Dr. Jackson commits the grave error of recommending a plane mirror, and instead of neutralizing the shadow by means of lenses, he judges of the amount of error by the distance he is obliged to go from the patient in order to neutralize the shadow. The method recommended by him is not retinoscopy as taught and practiced by the French and English, but merely a modification of the old method known as the "fundus image" test. It has always been recognized, and utilized to a certain extent, that in emmetropia, at a distance of several feet, when a light was directed into the eye from an ophthalmoscopic mirror, nothing but a red reflex could be distinguished. If the eye was myopic or hypermetropic in a high degree, the disc and blood vessels could be seen. When a plane mirror is used, these vessels will move in direction against that of the mirror in myopia, and with it in hypermetropia, and by gradually approaching the patient the vessels will become invisible, and as you come closer will be reversed. This point of reversal corresponds to the far point of vision, and by measuring the distance from the eye, the amount of myopia or hypermetropia, by first making the eye highly myopic with a concave lens, may be approximately determined. It is to be regretted that Dr. Jackson has presented this somewhat difficult and, at best, uncertain method of determining errors of refraction in place of the simple, uncomplicated and definite method of retinoscopy as taught in the English ophthalmic hospitals. I like the term retinoscopy, and believe it will be retained as the proper one. At one time I was greatly in favor of substituting the name of "shadow test," as I thought that would convey the proper meaning of the method of examination a little more definitely and prevent confounding of it with other mirror tests, as Dr. Jackson has done; but I see he uses, as the title of his article, the very term that I thought would prevent this mistake.

The examination by this method is so simple that I sometimes think much valuable time has been wasted in explaining the method of its performance. I fear that the descriptions have often served to render the subject more obscure. With a few trials I have been able to teach students with no special skill in the use of the ophthalmoscope to use it satisfactorily and with benefit to patients. I know one spectacle peddler who, making no pretensions to a knowledge of refraction and accommodation, uses it successfully in his business.

In teaching the use of retinoscopy, if possible, I select for examination a person with emmetropic eyes—usually a fellow student.

For beginners, it is better to dilate the pupil with cocaine. My ophthalmoscope is furnished with two mirrors attached by means of a screw; one large mirror of about twenty-four inches focus for making examinations by the indirect method and for performing retinoscopy, and one mirror (small) placed at an angle of the usual focus for direct examinations. This is known as the Johnsons' ophthalmoscope, and is made by Crouch, of London.

I seat the patient with his back to the light, and the student opposite at the distance of about three or four feet. I then direct him to reflect the light into the eye of the patient, who is requested to look past the opposite ear of the one making the examination. If the observer has had a little experience in using the ophthalmoscope he catches the red reflex from the fundus directly. With beginners, I find this the most difficult part to teach. I then direct the examiner to rotate his mirror from side to side slowly and watch the red reflex carefully, and ask him whether he sees a slight shadow moving across the red disc. Most observers see it easily, others require some little time. Beginners often make the mistake of moving their heads instead of rotating the mirror. I then direct them to rotate the mirror in the vertical meridian until they see the shadow again. I am careful to see that they *rotate* it, not move it up and down. I then place a + 1 D. in a spectacle frame, in front of the eye under examination. The examiner will soon tell you he can see no shadow. I then put in a + lens (having the + 1 D. in

Digitized by Google

place, which I do not change during the entire examination) and he again sees the shadow. I direct him to observe closely the direction in which it moves. I alternate plus with minus lenses, and the examiner will soon tell you that with the plus lenses the shadow moves opposite to the direction in which the mirror is moved, and with the minus lenses it moves with the mirror.

By leaving the + 1 D. in the frames we are not obliged to make the additions and subtractions which are necessary if it is removed, and we go on with the examination just as if it were not there, making the calculations as simple as with a plane mirror.

One of the first questions asked by the student is, why do we have this shadow moving in the same direction as the mirror, in low degrees of myop a? And why is it necessary to use this + 1 D. to neutralize the shadow in emmetropia? This is because with a mirror of twenty-four inches focal distance the rays of light do not cross before reaching the observer, and an erect image of the illuminated and shaded portion of the retina is obtained, the same as when a plane mirror is used.

The following rules are of great practical importance in making these examinations.*

1. If the image of the shadow appear to move in the same direction in which the mirror is moved, and if the rapidity of movement and curvature of the shadow are the same in all meridians, we have to do with a simple myopia.

2. If the shadow appear to move in the opposite direction to that in which the mirror is moved (remember in all of these examinations the + 1 D. is to be left in situ), and if the rapidity of movement and curvature of the shadow are the same in all meridians, we have a simple hypermetropia.

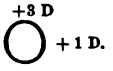
3. The slower the movements of the image the feebler the illumination; and the more crescentic and narrower the shadow, the higher the hypermetropia or myopia.

4. A difference in two opposite meridians, either of direction or rapidity of movement, or of the curvature of the shadow, indicates astigmatism. These two dissimilar shadows, moving at right angles with each other, either one vertically and the other horizontally, or both obliquely, indicates the meridian of greatest and least refraction.

5. If the shadow moves with the mirror in one meridian and against it in another, we have a case of mixed astigmatism.

By means of ordinary trial lenses it is easy to measure the amount of error of refraction. If the shadow move with the mirror, we place concave lenses in the spectacle frame until the shadow is neutralized, and the number of lenses required will indicate the amount of myopia.

If the shadow moves opposite to the direction in which the mirror is moved, convex lenses are used until the shadow is neutralized and the lens required to neutralize the shadow will indicate the amount of hypermetropia. In cases of simple astigmatism no shadow will be seen in one meridian, and a shadow moving with or against the mirror in the opposite. By neutralizing this shadow with the appropriate lens it will indicate the number of the cylindrical glass required to correct the astigmatism. In cases of compound astigmatism, it will be necessary, first, to correct the meridian with the least error of refraction. This will indicate the spherical lens required; and then by correcting the meridian with the greater error, and deducting the lesser from the greater the cylindrical lens required will be found. Suppose we have a case in which it requires a + 1 D. to neutralize the shadow in the horizontal meridian, and a + 3 D. to neutralize it in the vertical. To correct the error would require a spherical + 1 D. ⊂ + 2 D. Cylindrical. Axis 90.



* Morton. "Refraction of the Eye."

Digitized by Google

Cases of myopic compound astigmatism may be corrected in a similar manner, using concave lenses. Cases of mixed astigmatism can be corrected in the same way. Supposing a given case requires a + 2 D. to neutralize the shadow in the vertical meridian and a minus 1 D. in the horizontal

+ 2 D
○ — 1 D.

The following formula would indicate the lens required to correct the error of refraction: Spherical + 2 D. ⊂ — 1 D. Cylindrical. Axis 180.

I think one reason why retinoscopy has not come into more general use is the fact that, in order to determine the amount of error of refraction, it is necessary to resort to the use of trial lenses.

One of the great objects to be attained by the numerous optometers presented to the profession, and this has been almost as fruitful a field for the inventive genius of the oculists as the pessary has to the gynæcologist, has been to do away with trial lenses. This has also been one of the great advantages claimed for the direct method of examination with the ophthalmoscope.

But, whatever other means are made use of to determine the amount of error of refraction, I do not believe we will ever be able to discard the practical test with trial lenses. Often the lenses which are theoretically correct are not the ones practically best adapted to the needs of the patient. I presume my experience has been similar to that of most ophthalmologists, when I say that I frequently find patients wearing simple spherical lenses selected by themselves, or some traveling spectacle peddler, who have discarded the use of compound lenses which have been fitted with great labor and skill by the oculist.

There is in all these cases an unknown quantity, the accommodation, and the ophthalmologist who keeps this fact in mind, and proves his readings either with the ophthalmoscope, or by means of retinoscopy, or any of the optometers, by an actual test with trial lenses, will often save his reputation as an oculist, and render the best service to his patients.

But as it is often necessary to prescribe spectacles when we cannot have the aid of trial lenses, I have made use of the following simple device which occurred to me in an emergency some years since. As already stated, I make use of an ophthalmoscope with two mirrors attached by means of a screw. Simply by detaching these mirrors I have two instruments. First, a retinoscope with a good handle, and second, a series of lenses conveniently arranged for determining the amount of error of refraction, which answers every purpose of a trial case. This latter I give to the patient, directing him to look past my ear through the small hole. I then proceed to make the examination in the usual manner, directing him to turn up the proper lens, as required.

I have made use of this simple procedure for a number of years and have found it to serve me admirably when away from my office, where I could not have access to trial lenses.

I have found the concave mirror much superior to the plane one. It concentrates the light so that we secure better illumination, and makes it possible to sit at such a distance from the patient that it is convenient to change the lenses without moving. It has been urged that the exercise necessary in walking backward and forward each time a lens has to be changed was of benefit to a physician of sedentary habits. This has not impressed me of very great advantage. I would still prefer to have retinoscopy characterized as a "lazy English method," and keep my seat.

The advantages of retinoscopy are many. It does not require any expensive apparatus or paraphernalia. It is easy of execution; any one capable of making an ophthalmoscopic examination can learn to use it in a few hours. It saves valuable time.

Digitized by Google

Cases which formerly took from one to a dozen sittings, as long as the patience of my patient and myself would permit, are now disposed of usually in one sitting of short duration.

Although it is necessary to use atropia occasionally to paralyze the accommodation, especially when there is spasm of the ciliary muscle, I find that since I have become accustomed to using retinoscopy it is not necessary to use atropia once where I formerly used it ten times. There is one point on which many observers are at fault. That is in directing the patient to look at the mirror. If, instead, the patient is directed to look at the dark wall beyond, the accommodation is relaxed, the pupil dilated, and the examination made comparatively easy, and the use of a mydriatic seldom necessary. With a little practice the physician soon learns to distinguish when the accommodation is relaxed, by the condition of the pupil.

A retinoscopic examination is more easily performed than an ophthalmoscopic one by the indirect method. It is more accurate as a means of correcting errors of refraction than by the upright image, and the examination is made as quickly as with the optometer.

We are enabled to examine cases and correct the error of refraction without the assistance of the patient, a very important matter with children, ignorant people, and those of feeble intellect. We are enabled by means of retinoscopy to fit spectacles accurately in cases of amblyopia as the result of the excessive use of tobacco, alcohol, etc., and especially as the result of squint. In cases of nystagmus, in which it is impossible to fix the eye, its use is invaluable.

I have purposely omitted all allusion to the optical principles involved, as they have been fully discussed in the many papers written on this subject. I have endeavored to present this subject in a plain, practical manner, and if this incomplete paper should be the means of creating enough interest in retinoscopy to lead you to give it a fair trial, I am sure you will be surprised with what ease difficult cases of error of refraction can be corrected.

DISCUSSION.

Dr. Galezowski, of Paris, said: The work of Mr. Cross is very interesting and his method worthy of your serious attention. It is probably the most mathematical of the methods of estimating errors of refraction. But a word as regards the history of this method of examination. Mr. Cross has not made mention of the fact that Monsieur Cuignet discovered and practiced this method fifteen years ago, and one of my assistants later on wrote several descriptions of the same method of examination. And to these two gentlemen is due the credit of retinoscopy. Thanks to Mr. Cross, it is now known in America. And another point, allow me to say that I am completely against the name Retinoscopy. One, on giving this name, would think immediately that it was a method for diagnosing the condition of the retina. Other authors wish to give it the name of pupilloscopy, but this, on the same principle, would not hold, for it would seem to indicate the condition of the pupil. In the last edition of my "Atlas of Ophthalmoscopy" I have suggested the name Dioptroscopy, as coming nearer the idea of the discoverer of the method, inasmuch as the degrees of the refraction are expressed in dioptres.

Dr. Swan M. Burnett.—Though a rose by any other name perhaps would smell as sweet, an indiscriminate use of a number of names for the same thing should not be permitted in a science whose nomenclature is supposed to assist in definition and to convey to the learner, by the word used, some precise idea of what the thing is or is supposed to be. Prof. Galezowski has shown that neither "retinoscopy" nor "pupilloscopy" at all describe the method, which takes its name from the

Digitized by Google

object seen by the observer, and I am forced to reject his own term, "keratoscopy," on the same ground. Both Mr. Cross and Dr. Baker have constantly used the word "shadow" in their papers when they wished to indicate the thing seen, and whose changes of form and position characterize the procedures. That being the case, "skioscopy," it seems to me, is the only term that we can accept as descriptive of this method of examining the refraction of the eye. If we wish to use an English word we can say "shadow test." I have shown my belief in the method by giving it a chapter in my work on astigmatism. I use myself a plane mirror, but the choice of the plane or concave form will depend upon the one with which the operator has been most accustomed to work.

Mr. Cross incidentally, in his remarks, states that astigmatism is corneal, and that the action of the ciliary muscle only affects the general ametropia and but rarely the astigmatism.

A considerable experience with the keratometer of Javal has shown me that in a number of cases the astigmatism of the eye is not accurately represented by the corneæ, but that there exists a lenticular form, due to partial contraction of the ciliary muscle, and generally such as to neutralize the corneæ in whole or in part.

Dr. Fannie Dickinson, Chicago, would like to ask Mr. Cross if, in all cases, where, after the use of atropine, he found the latent amount of refractive error after retinoscopy to be a spherical correction of 0.50 D. to 1.50 D., did he find points of similarity in these cases, as age, occupation, general muscular tone, or other points which modify and vary the proportion to be corrected of the entire error of refraction found after the use of atropine? She desired to make a comparison between the amount of error left uncorrected by retinoscopy, which Mr. Cross suggests may be considered as the "tone of the ciliary muscle," and the tone of the ciliary muscle which is taken into consideration and allowed for after the use of atropine in many cases.

Mr. Cross.—Besides our deductions from examination by retinoscopy (as several speakers have suggested), varying accommodation of the eye, age, occupation, *individuality, in fact*, must be fully considered in prescribing spectacles. As regards the various names that have been suggested for the method, it is not only the shadow that is watched but the light of which the shadow is the edge; its movements are seen not only in the pupil but through it upon the retina. The term retinoscopy is too inclusive; it might also be applied to other forms of retinal examination, but for the present I think it is the best we can use. Keratoscopy is wrong and misleading, for the cornea certainly is not the observed part. I agree with Dr. Burnett that *asymmetrical* action of the ciliary muscle may occasionally produce a lenticular astigmatism.

Digitized by Google

FIFTH DAY.

TREATMENT OF ABSCESS AND ULCERATION OF THE CORNEA WITH JEQUIRITY.

TRAITEMENT D'ABCÈS ET D'ULCÉRATION DE LA CORNÉE AVEC LA JÉQUIRITÊ.

ÜBER DIE BEHANDLUNG DER ABSCESSE UND ULCERATIONEN DER HORNHAUT DURCH JEQUIRITY.

BY EUGENE SMITH, M. D.,

Of Detroit.

We have been taught by experience that, in extensive infiltrations of pus in the cornea, there is small hope of its absorption; and we are all aware of the tendency of the pus to diffuse itself widely. This tendency to spread in all directions is found in closed abscesses—not infrequently in abscesses which have opened internally or externally—and occasionally in open ulcers.

The progressive destruction in and around the original point of disturbance is due to the proliferation, to the solvent property of the infiltration, and to the pressure on the infiltrated tissue. Possibly, or probably the presence of microbes play a not unimportant part in the progress of the disease.

How best to check and suppress the suppuration and arrest the progressive destruction of the cornea, are questions which have long agitated the minds of ophthalmologists, as has also the best means of getting rid of the more or less extensive opacity so invariably left if the patient recovers.

These somewhat general remarks bring me to the subject under consideration, the treatment of abscess and ulceration of the cornea with Jequirity.

In September, 1883, in an article published in the *Journal of the American Medical Association*, I called attention to its action in a case of asthenic ulcer of the cornea in a decrepid man of seventy-three years of age, where the eye was rapidly going to destruction in spite of all treatment. As I was using jequirity in several cases of trachoma, its peculiar effects upon the circulation led me to test it, by way of experiment, in this case. A 3 per cent. solution was applied to the conjunctival sac three times in twenty-four hours, and much to my surprise, the progressive necrosis was checked, and in two weeks the patient was discharged *well*, and the resulting cicatrix was surprisingly slight.

In October, 1883, I again called attention to the use of this remedy, in an article published in the *Medical Age*, citing several cases of obstinate herpetic trouble, vascular ulceration, etc., which were speedily relieved by its use when prolonged treatment by the usual methods had failed.

Since that time I have frequently resorted to its use in cases of ulcus cornea serpens, chronic vascular ulcer, asthenic ulcers and abscesses, and in one case of herpes zoster seu ophthalmicus. In several cases the extent of the purulent collection and the character of the case seemed to forbid all hope of saving the eye, or even a sufficiently large

Digitized by Google

portion of the cornea for an artificial pupil, yet, *in each case recovery* has taken place, with the resulting opacity so thin as not to call for operative interference.

To what is this very remarkable absence of scar tissue due?

I have not sought to study the physiological action of the drug, but have occupied myself with noting its clinical effect, though I have wondered if the jequiritic bacillus had anything to do with the demise of the microbes said to be present in ulceration of the cornea. I incline, however, to the belief that the proliferation of the corneal corpuscles and their segmentation is checked, while nutrition is increased by the numerous blood vessels, a fine network of which, more or less abundant, and apparently quite thin, develops on, or in the cornea, for, coincidently with their development is seen a marked change in the character of the abscess or ulcer; we soon observe the opacity to be disappearing and the transparency of the diseased portion of the cornea returning, most positive evidence of improved nutrition.

I do not practice, nor would I advise pushing the remedy to the point of getting the degree of jequiritic inflammation necessary in the treatment of trachoma, but find that a 3 per cent. solution applied two or three times in twenty-four or forty-eight hours—or one application of a *very small* amount of the powdered seed, keeping the eye cleansed with a 2 per cent. solution of boric acid after the jequiritic effect is established, answers the purpose. The inflammation of the conjunctiva is seemingly catarrhal in character, with, in some cases, but a slight degree of the membranous characteristic.

DISCUSSION.

Dr. GALEZOWSKI, of Paris, said—Allow me to make a few remarks on the subject of Dr. Smith's paper. In the several years that it has been used, I have seen jequirity produce such grave results, that I question the advisability of using such a substance in ulcers of the cornea. Certainly much depends upon the strength of the solution employed, and, as I have never used the solution in the same strength as that employed by Dr. Smith, naturally I can say nothing of its results. Apropos of ulcers of the cornea, allow me to make some remarks, the result of my own experience during the past few years. I have met many cases where corneal ulcers have resisted every kind of treatment, and I have searched for the reason in vain. Finally I came to the conclusion that in a great number of cases the ulcers were only herpetic in nature and were provoked by an intermittent fever. In another case I observed a carious tooth had given rise to persistent neuralgia, and again, as a cause, spontaneous neuralgia of the fifth pair of nerves. Of course, in such cases the cause must be treated.

Locally I prescribe iodoform powder, or the steam douche is allowed to play upon the eye, and for the pain I generally instill cocaine. The older Desmarres was accustomed to use atropine in corneal ulcers, while at the same period Velpeau employed nitrate of silver, and indeed recommends it highly in his treatise on "Eye Diseases." This latter remedy I have tried, and I am happy to say in a number of cases with great success. I always immediately neutralize with a solution of chloride of sodium. To sum up. It is important to get at the cause of the trouble and treat that. Locally I use iodoform powder, steam douche and the cauterization of the ulcers with a two per cent. solution of nitrate of silver applied with a camel's hair brush and immediately neutralized with a solution of sodium chloride, and through such treatment I have had the happiest results.

Dr. Smith closed the discussion by saying that he always tried first a three per cent. solution, and if that did not act favorably he stopped its use immediately.

Digitized by Google

NECESSITY FOR REFORM IN THE MANNER OF DESIGNATING LENSES.

NÉCESSITÉ DE RÉFORME DANS LA MANIÈRE DE DÉSIGNER LES LENTILLES.

NOTHWENDIGKEIT EINER VERBESSERUNG IN DER METHODE ZUR BESTIMMUNG DER AUGENGLÄSER.

BY DUDLEY S. REYNOLDS, A. M., M.D.,

Of Louisville, Ky.

The greatest difficulty in determining errors of refraction in the eye lies in the faulty manner of constructing the instruments used in testing. The lenses in our trial-cases are graded according to what is known as "the metrical system," or by the radius of curvature, according to the plan of determining astronomical refractions. The lenses sold in the shops are graded roughly by the approximate length of the refracted rays from the nodal point in the lens to the focal point of the light. Now, the attempt to grade a system of refracting lenses by the measurement of the focal lengths only, is, of itself, unfortunate. It induces the optician to attempt the impossible; and it is just here that the metrical system is especially faulty. In this system, the glass which focuses light at the distance of one metre from its nodal point is called "a Dioptre," and this is the unit of refraction. According to this system, the lenses are graded by 0.25 D., 0.50 D., 0.75 D.; and below the unit, in whole numbers, halves and quarters. Now, since the focal length of the unit is a metre, and since it is necessary in any attempt to establish the focal point of a lens at half this distance the radius of curvature shall be considered, it seems unreasonable to go near this point in the choice of the unit, and, besides, it is not easy to establish focal lengths of a series of lenses by the means ordinarily employed in measuring. In addition to all this, 1.50 D. should have a focal length of about thirty inches; 2 D. about twenty inches; while 1.75 D. would fall at about twenty-five inches. So that no intermediate focal point may be reached by this system between twenty and twenty-five inches, or between twenty-five and thirty inches. To every experienced ophthalmic practitioner, this, at times, proves unsatisfactory. Now, according to the old system of astronomical refractions, the unit is obtained by the maximum angle of refraction in a spherical body. Thus, a transparent sphere, highly polished, and made of what is known as optical crown glass, will interrupt the passage of light to the full extent of its own diameter, while light falling upon the surface of the sphere would be refracted toward the centre from a portion of the circumference equal to 90°. As the whole circumference represents 360°, 90° is, of course, equivalent to a quadrant of the sphere. If the sphere be about two inches in diameter, the quadrant focuses light near the centre of the sphere, or, by actual linear measurement, at $\frac{7}{8}$ of an inch from the nodal point of the segment representing the quadrant. Now, the quadrant, being the unit, represents a definite radius of curvature, no matter what the size of the sphere; and the series of refracting lenses derived from the division of this quadrant is what Dr. Robert Smith employed to designate the varying degrees of refraction in his "Compleat System of Opticks," published at London, 1738. It was employed by Dr. Wm. Porterfield in his treatise on "The Manner and Phenomena of Vision," published at Edinburgh, 1759. In fact, it was the only system of grading lenses recognized by scientific writers and experimenters down to the time of the publication by the new Sydenham Society, of London, of Donders' great work on the "Anomalies of Accommodation and Refraction," in 1864. Donders has been the text-book since that time for most of us. In recent years, however, the metrical system has been brought forth and made popular by the labors of Landolt, Snellen, and the German writers on accommodation and refraction.

Digitized by Google

What is commonly called "the inch system" of measuring lenses has never had any place in science. It was simply the method used by the common peddlers of spectacles, and by those engaged in producing the cheap manufactures intended for the common people, whose defects being for the most part symmetrical, permitted them to employ such lenses as afforded apparent relief by increasing the angle of vision, often at the expense of the sharpness of the image.

In 1878, Prof. Snellen devised an instrument for measuring lenses by determining the character of refraction of the points of light thrown upon an opaque screen. Carefully constructed, this instrument, known as "Snellen's Phakometer," is of great value. The want of precision in its measurements has been almost entirely met by the placing of a dark, bellows-shaped tube between the clasp which holds the lens to be measured and the registering disc upon which the refracted points of light are collected.

This excludes all interference of either diffuse or artificial light in the line of the refracted rays between the lens to be measured and the focal point. With a series of minute points of light passing through a lens, in vertical and horizontal lines, intersecting at the centre, the central point may be employed for centring the lens in the phakometer. If asymmetry of grinding exist, the points will not be perfectly round, or there may be noted at one side of the point a blue margin, and when this is removed by sliding the disk, it may be noted that the opposite side of the same point is red or blue, according to the direction in which the disk is moved. I esteem this instrument, with the improvements I have added, and which are known to the profession, of great value. It affords the most accurate method known of testing the refracting powers of lenses. Employing this instrument for the purpose of testing lenses in trial cases, made by the best manufacturers, I have been astonished to find that many lenses are not designated properly according to their refracting power. These, graded by what may be known as the old scientific system of dividing the quadrant of a sphere, do not represent the exact fractional parts of this quadrant that the figures etched on the margin would indicate. In 20 cases, graded by the metrical system, I found but little difference in those lenses marked 1 D.; while all those of greater focal length, and correspondingly less refracting power, were quite defective. It happened in several that the lens marked 0.25 D. was a perfectly plain piece of glass. Of the negative lenses graded by this system, scarcely ten per cent. of the whole number were even approximately accurate. Those of American manufacture are superior in the accuracy with which they are graded, especially the lower powers. Now, in order to do away with some of this class of defects in graded lenses, I propose that no attempt shall be made to designate them by focal lengths, and that we shall either return to the old system of astronomical refractions, taking the quadrant of a sphere for the unit, and dividing it into any desirable number of fractions having a common multiple, according to the radius desired to represent a stated angle, which is the method I use, or that we shall have our lenses graded by the angle of refraction only. This seems to me in every way more desirable, as we should then be able to designate the lenses we desire with greater precision. We should recognize 90° as the maximum angle, not for purposes of division, but for the purpose of establishing the highest angle of refraction which any spectacle lens might possess. We might begin with an angle of 1° or 5°, and proceed in this way to construct a series of lenses for ordinary use; and for very nice testing of sensitive eyes, we might employ lenses with an angle of 5″, 15″ or 30″. We might even grade lenses with any refracting angle desired, provided we could secure a uniform quality of crown glass, and this substance has been determined to be best adapted for spectacle lenses. Its index of refraction having been established, a skilled optician should have little difficulty in constructing such lenses as might represent precisely the amount of refracting power desired. I have consulted two practical opticians, who inform me this scheme could easily be carried out. I

Digitized by Google

submit it for the consideration of this Section of the Congress, believing, were it adopted, we should have a more scientific grading of the lenses than could be secured by considering the angle of the radius.

Being asked by Prof. Landolt to give an example in writing for a compound cylinder after the manner stated, Dr. Reynolds said: "I use the old system of gradation by the radius," and wrote on the blackboard the following:—

$$\overset{\text{O. D.}}{+\tfrac{1}{200}\,90^\circ \supset \tfrac{1}{400}} \;\|\; \overset{\text{O. S.}}{+\tfrac{1}{800}\,75^\circ \supset +\tfrac{1}{800}} \;|\; \overset{\text{Bis. C.}}{2\tfrac{1}{2}''}.$$

DISCUSSION.

Dr. LANDOLT requested Dr. Reynolds to explain in detail.

Dr. BURNETT, of Washington, said—I think that the system proposed by Dr. Reynolds, which does great credit to his erudition, can never come into general use, from the fact that in dealing with the eye we have to do always with the focal point. In fact, the chief and important property of any optical system is its focal length, negative or positive. What we have needed is some uniform distance of focal length as a unit or standard, and this is furnished us in the metre, which is now adopted by all civilized nations, either by statute or permission.

Dr. ALLYN, of Pittsburgh, opposed return to inches and advocated the metre system.

Dr. THOMPSON, of Indianapolis, regarded the method of Dr. Reynolds as scientific, but he himself uses both methods.

Mr. CROSS, of Bristol, England—Dr. Reynolds' scientific communication is worthy of full consideration, as an attempt to improve our method of grinding and of designating lenses. In considering refraction we are constantly making reference to distance, "far point," "range of accommodation," etc. Our standard lens must also bear a similar designation. *It must have a standard focal length.* If we adopt Dr. Reynold's suggestions, we are at once in the difficulty of requiring a constant material for making the lenses; this is impossible. Moreover, our calculations must be made in vulgar fractions. I consider the metrical system, as now universally used, to be as near perfection as possible.

THE DESIGNATION OF PRISMS BY THE ANGULAR DEVIATION THEY CAUSE, INSTEAD OF BY THE REFRACTING ANGLE.

LA DÉSIGNATION DES PRISMES PAR LA DÉVIATION ARTICULAIRE QU'ELLES CAUSENT AU LIEU DE L'ANGLE DE RÉFRACTION.

DIE BESTIMMUNG DER PRISMEN DURCH DIE DURCH SIE VERURSACHTE ANGULÄRE ABWEICHUNG, ANSTATT DURCH DEN REFRACTIONSWINKEL.

BY EDWARD JACKSON, A. M., M. D.,

Of Philadelphia, Pa.

The angle included between the refracting surfaces of a prism is called its *refracting angle.* The angle between the direction of a ray as it enters a prism and its direction on leaving it is the *angular deviation*, a direct measure of its *refractive power.* The refractive power of the prism depends on its refracting angle, and the indices of refrac-

Digitized by Google

tion of the substance composing it and the substance surrounding it. For our purposes the substance surrounding the prism is always air, with a practically constant refractive index. But the index of refraction for optical glass, the substance composing the prism, may vary from 1.51 to 1.72, and is liable to change in the future with changes in its composition or processes of manufacture. When I order a prism of three degrees by the present system of numbering prisms, just so the shape is right, it may deflect the light rays 1.53° or 2.14°, and still be exactly the prism ordered. It is no just ground of complaint that the prism furnished happens to have 40 per cent. more refractive power than the prism in the trial case. That such an extreme case is unlikely to occur may be freely admitted. But the fact remains that quite appreciable variations in the strength of prisms designated by the same number do constantly occur from this variability in the index of refraction of the glass used. And this inexact, unscientific method of designating prisms constantly covers inaccuracies of carelessness quite as serious as the case supposed. Some of these are revealed in the results obtained by testing the prisms in seven trial cases representing four well-known makers, two American and two European. These are given below in tabular form, the first column giving the number of the prism; the second the deviation it would cause if properly made of glass having a refractive index of 1.54; the third the weakest of that number tested; and the fourth the strongest.

Number.	Proper Strength.	Weakest.	Strongest.
1	0.54	0.40	0.60
2	1.08	0.65	1.38
3	1.62	1.45	1.65
4	2.16	1.95	2.20
5	2.70	2.22	2.82
6	3.24	2.78	3.10
7	3.78	3.35	3.85
8	4.33	3.85	4.45
9	4.88	4.65	5.
10	5.43	5.05	5.45
12	6.52	6.15	6.60
15	8.18	7.70	8.10
20	11.01	10.18	10.50

Every set examined showed at least one prism varying a whole number from its proper standard of refractive power. And in but one set did they all come within 16 per cent. of the proper standard.

Now prisms are used solely for their power of refracting light. If we order a certain prism, we want, not one made from the same lot of glass as the prism in our trial set, not one having the same shape or thickness, but one having the same power of refracting light. Identity of refractive power is the only identity we must always demand in the prism furnished. Would it not be better to designate in the prescription exactly the refractive power desired, instead of prescribing one of a certain shape or refracting angle which may or may not possess that refractive power?

Again, we are often compelled to speak of the deviation of the visual axes as measured in degrees; and the introduction of another kind of degrees (degrees of refracting angle), not necessarily corresponding with degrees of deviation, is a very frequent cause of confusion and obscurity in discussions concerning prisms and the conditions and purposes for which they are used. Take this sentence from a recent paper,* in which the writer is alluding to "hyperphoria," as he calls the tendency to vertical deviation of the visual axes. "An actual deviation of a single degree would result in a separation of images at a distance of one-half metre, approximately of 6.4 mm." Possibly the

* "Anomalies of the Ocular Muscles," G. T. Stevens, *Archives of Ophthalmology*, 1887, p. 161.

Digitized by Google

author could tell whether he meant what he said, an actual deviation of one degree, or a deviation such as would be produced or corrected by a prism having a refracting angle of one degree. But no one else can feel certain about it. Would it not be best to give up a system of notation which requires us to deal with two different kinds of degrees where one would better answer every purpose?

Then, as we measure convergence in metre-angles its comparison with deviations estimated with prisms entails an extra labor of reduction to common terms so long as we designate prisms by their width of refracting angle; and the calculation of the prismatic effect of a certain decentring of lenses is complicated in the same way. So that it seems to the writer entirely worth while to encounter the inconveniences attendant upon a change of the system of numbering prisms to one based directly and solely on their refracting power: Lenses were formerly numbered according to their radius of curvature, but the change to the basis of refractive power has been readily effected, and with prisms such a change would be in some respects even easier.

My proposition is, *a prism shall be designated by the number of degrees light rays are deflected from their course by passing through it in such a way that within the prism their direction is perpendicular to the plane bisecting its refracting angle; that is, the minimum deviation the prism can produce.*

To avoid confusion between the old and new systems, the number of a prism according to the new might always be written or printed with a circle surrounding it. Thus prism (2) would indicate a prism whose refractive power, minimum angular deviation, is just two degrees, whatever may be its refracting angle.

DISCUSSION.

Dr. LANDOLT said—The work of Dr. Jackson seems to me exceedingly meritorious; his conclusions in every way logical, reasonable and tending to supply a genuine want; I make the motion, then, that the proposition of Dr. Jackson concerning the designation of prisms be submitted to the consideration of ophthalmologists throughout the world, that it be supported by this Section, and that it be discussed and eventually adopted at the next International Congress.

Mr. POWER proposed that a committee be appointed to bring this matter formally before the Ophthalmological Section of the next Congress.

The President appointed Drs. E. Landolt, S. M. Burnett and E. Jackson.

AMETROPIA IN OUR SCHOOLS.

L'AMÉTROPIE DANS NOS ECOLES.

ÜBER DIE AMETROPIE IN UNSEREN SCHULEN.

BY F. B. TIFFANY, M. D.,
Of Kansas City, Mo.

What I have to say to-day on the subject of ametropia is based entirely upon data collected from examinations made a few months ago in the public schools of Kansas City and vicinity. These examinations included the *white*, *red*, *black* and *mixed races*, embracing several distinct nationalities. Before beginning the examinations I arranged a record, with spaces for name, nationality, age, sex, color of eyes, if the pupil himself

Digitized by Google

or his brothers or sisters or parents wore glasses; space for vision of either eye—emmetropia, myopia, hypermetropia, manifest or latent astigmatism, spasm of accommodation, strabismus, convergent or divergent, and a space for remarks—all of which facilitated me in obtaining complete and practically accurate statistics. I not only examined for myopia, but for hypermetropia, astigmatism, spasm of accommodation and strabismus. When I began the examinations I had in mind ten thousand, but I stopped with two thousand and forty. The examinations were quite carefully made, examining most of the pupils separately, many with the ophthalmoscope and some by the aid of mydriatics, so that I think, in the main, the true diagnosis was made; in some cases I may have mistaken spasm of accommodation for myopia, and *vice versa*. As a result of the examinations I have the accompanying formulated table, showing the whole number examined, the average age of each school, the number and per cent. of affected in each school, number of males examined and the number and per cent. of those affected, the number of females examined and the number and per cent. of those affected. Then the number of different nationalities examined, with the number and per cent. of those affected; a per cent. of each sex. Then comes the colors of the eyes—blue, gray, brown, hazel and black, with the whole number of each, and the number and per cent. of those affected of each. Under the next head we have the affections—strabismus, myopia, hypermetropia, astigmatism, spasm of accommodation; and following this comes hypermetropia and myopia, compared one with the other; and, finally, the per cent. the school is to the whole number, per cent. of the affected in the same school; and, lastly, latent hypermetropia, and at the foot a general summation of the columns severally.

That perfect vision, perfect eyesight, be enjoyed by our progeny depends largely upon the recognition of any defect (slight though it may be) in early life, timely correction of the same (if possible), and care and proper use of the organ while in the school-room. That many a boy or girl with some anomaly of refraction or accommodation has been allowed to suffer day after day in order to keep pace with his class, and eventually develop more serious conditions, or even blindness, is too evident; whereas if the trouble had been recognized in time and proper steps taken, much suffering as well as serious consequences could have been averted. The object of these investigations is to ascertain, if possible, the condition of the eyes of the youth, the effect of use of the organ for near and small objects, and the final consequences; and if possible to correct the evils arising before irremediable damage has been done. In Dr. Cohn's report only one anomaly of refraction (myopia) is considered. Dr. Cohn thinks that near-sightedness increases in the higher grades, finding more myopes in the second year than in the primary department—more in the third than in the second, and so on through the grammar school to the high school, where there is more among the last year students than among those just entering the high school, and still more in the normal schools and universities. He attributes the increase of myopia to improper use of the eye in the school room, as well as bad light and improper heights and structure of seats and desks. This, to a certain extent, is evidently true. In my friend Dr. Ernest Fuch's report, which has been translated and appears in the "Chicago School Board Report," 1886, the whole attention is directed to myopia alone. Now, in our investigations, not only myopia but hypermetropia, astigmatism, spasm of accommodation and strabismus were looked for. Every eye that was not up to the standard of vision, (that of $\frac{20}{20}$) was not dubbed myopic. A careful examination was made with the trial glasses, or optometer, and a more accurate diagnosis made. Our results show a much larger per cent. of hypermetropia, and astigmatism, and spasm of accommodation than myopia; there being nearly four times as many hypermetropic pupils as myopic. Of the whole number examined, 2040, in the different grades and schools, 99 or 4.8 per cent. had spasm of accommodation. Now spasm of accommodation (or simple hyper-

Digitized by Google

metropia, even) if not corrected by glasses or relieved by rest from near and small objects, is likely to develop into myopia, or possibly intra-ocular disease; hence the importance of these examinations. In our examinations the increase of anomaly of refraction as we ascend in the grades is not marked—in fact, in some schools it seems to diminish, but this would not be a definite proof one way or the other, from one examination, as a different set of pupils are examined in each grade and in each school, and from the one examination there is no means of knowing but what those of the higher grades started with the same degree of anomaly in the primary departments as they are found to have in the sixth year of the grammar school or the high school, as the case may be; in fact, it seems to be true that pupils with anomalies of refraction, both myopes and hypermetropes, are closer students and fonder of books and study than the emmetropes or those of perfect vision, and hence these students remain in school while many of those with perfect vision drop out before they reach the higher departments. In our examinations we find a greater percentage of anomaly among the normal schools of Kirksville and Warrensburg than any other, but it does not necessarily follow that this is the result of a longer course of study and close application with the eyes, although it is conjecturally true. The only way to gain a positive knowledge that study is the cause, or otherwise, is to watch the same set of pupils from the primary department up through the different grades, and see if John, Mary and James, starting out at six or seven years of age in the primary department with perfect emmetropic eyes, finally, later on, develope myopia. In *our* examinations this can be done, as we have the name in full, color of each eye, present amount of vision, whether there is any hereditary defect or not; and now should the examinations be continued and the same pupils be called for as they enter the higher grades, year after year, a very definite and satisfactory idea can be gained.

As to the lighting and ventilation of our school rooms very little need be said, as so much attention has been given to this subject by our efficient Board that the Kansas City school room is almost perfection in those particulars. In the Washington school, where we find the largest per cent. of defective eyes in the lower grades, I noticed first and second year pupils (for want of room) were crowded into a room calculated for older pupils, and hence the distance between the desk and the seat was too great, as well as that from the seat to the floor, compelling the little pupil to hang, as it were, upon the desk, his feet not touching the floor; this position of necessity brought his face too near his book or slate, and hence taxed the power of accommodation of the eyes to a great degree; and besides, this school, from its location (on the north side of the hill), is not as well lighted and ventilated as others. In the Nevada school (adjoining town) I found a less degree of anomalies among the same grade of students than in the Kansas City schools. This I attributed to the fact that the school is in a small country town, where the children have more freedom of the field, where they are not crowded as much as our city children are, and perhaps have better ventilated school rooms. Pages 790, 791, 792 and 793 show a tabulated form of statistics, as gathered during the months of April and May, 1887, of the schools of Kansas City, Nevada, Mo., Warrensburg, Kirksville, State Normal School and the State Universities of Missouri and Kansas.

Digitized by Google

TABLE SHOWING THE RECORD OF TWO THOUSAND AND FORTY PUPILS, OF EYES AS TO ERRORS OF REFRACTION AND SPASM OF AND DARK EYES, AND IN

	Average Age in Years and Months.	Whole Number Examined. / No. having affected vision. / Per Cent.	No. Having Affected Vision. Females.	No. Having Affected Vision. Males.	NATIONALITY. Upper numbers indicate how many of each nationality. Middle numbers show number affected. Lowest row of numbers show the per cent. of each nationality affected in each school. American.	German.	French.	Scotch.	Irish.	English.	Swede.	Mixed.	Per cent. of Females Examined.	Per cent. of Males Examined.	COLOR OF EYES. First row of figures in each square represents total number of eyes of that color examined. Middle row of numbers indicates number having defective eyes of that color, respectively. Third row shows percentage. Blue.	Gray.	Brown.	Hazel.	Black.
Lawrence University	19.8	69 15 21.7	22 5 22.7	47 10 21.2	59 13 22.0	2 0 00.0	2 0 00.0	0 0 00.0	4 2 50.0	0 0 00.0	0 0 00.0	2 0 00.0	31.8	68.2	33 7 21.2	10 3 30.0	15 3 20.0	7 1 14.2	4 1 25.0
Columbia University	17.2	138 34 17.1	40 12 30.0	98 22 22.4	99 27 27.2	6 2 33.3	6 2 33.3	2 1 50.0	2 0 00.0	21 2 9.5	0 0 00.0	1 0 00.0	28.9	71.1	60 11 18.3	29 8 27.5	22 4 18.1	20 8 40.0	7 3 42.8
Kirksville Normal School	20.1	172 30 17.4	106 21 19.8	66 9 13.6	141 25 17.7	8 2 25.0	2 0 00.0	2 0 00.0	5 0 00.0	2 0 00.0	1 0 00.0	11 3 27.2	61.6	38.4	53 7 18.2	45 9 20.0	45 5 11.2	17 4 23.5	12 5 41.6
Warrensburg Normal School	16.9	170 44 25.8	106 24 25.6	64 20 31.2	157 37 23.5	4 3 75.0	2 2 100.	2 0 00.0	0 0 00.0	0 0 00.0	1 8's 0 00.0	4 2 50.0	62.3	37.7	45 15 33.3	38 12 31.3	63 9 14.2	20 8 00.0	4 0 00.0
Kansas City High School	16yr 10m	395 104 26.3	264 74 28.0	131 30 22.9	291 72 24.7	24 9 37.5	3 1 33.3	2 1 50.0	25 10 40.0	4 1 25.0	3 2 66.6	43 8 18.6	66.8	33.2	144½ 35 24.2	104 31½ 30.4	129½ 32½ 25.9	14 3 21.2	3 1 33.3
Fourth Year	17.8	58 17 21.2	34 11 32.3	24 6 25.0	50 15 30.0	1 0 00.0	1 1 100.	0 0 00.0	2 1 50.0	0 0 00.0	0 0 00.0	4 0 00.0	58.6	41.4	22 3 13.6	18 6 33.3	14 8 57.1	3 0 00.0	1 0 00.0
Third Year	17.3	64 18 28.1	53 15 28.3	11 3 27.2	42 8 19.0	4 1 25.0	0 0 00.0	0 0 00.0	7 3 42.8	1 0 00.0	0 0 00.0	10 6 60.0	82.8	17.2	18 6 33.3	16½ 2½ 66.6	24½ 9½ 38.7	5 0 00.0	0 0 00.0
Second Year	16.6	156 25 16.0	87 16 18.3	69 9 13.0	113 20 17.7	6 1 16.2	1 0 00.0	1 0 00.0	7 2 28.5	2 0 00.0	2 1 50.0	24 1 4.1	55.7	44.3	66½ 10 15.3	41½ 9 21.6	47 6 12.7	0 0 00.0	1 0 00.0
First Year	16.	117 44 37.6	90 32 35.3	27 12 44.4	86 29 33.7	13 7 53.8	1 0 00.0	1 1 100.	9 4 44.4	1 1 100.	1 1 100.	5 1 20.0	76.9	23.1	38 16 42.1	28 14 50 0	44 10 22.7	6 3 50.0	1 1 100.
Lincoln School	13.6	220 57 25.9	169 47 27.8	51 10 19.6	African or Negro.								76.8	23.2					
High, 1, 2, 3 and 4 Years	14.10	57 21 36.8	46 21 45.6	11 0 0.0									80.7	19.3	0 0 00.0	8 4 50.0	37 14 37 8	0 0 00.0	12 3 25.0
Seventh Grade	13.10	56 16 28.5	46 14 30.4	10 2 20.0									82.1	17.9	3 1 33 3	0 0 00.0	37 9 24.3	0 0 00.0	16 6 37.5
Fifth Grade	13.1	38 11 28.9	27 9 33.3	11 2 18.2									71.0	29.0	6 0 00.0	2 1 50.0	29 10 34.4	0 0 00.0	1 0 00.0
Fourth Grade	13.8	32 4 12.5	19 2 10.5	13 2 15.3									59.3	40.7	1 0 00 0	1 0 00.0	20 4 20.0	0 0 00.0	10 0 00.0
Third Grade	11.4	37 5 13.5	31 1 3.2	6 4 66.6									80.7	16.3	0 0 00.0	0 0 00.0	25 4 16.0	0 0 00.0	12 1 8.3
Morse School	12.6	207 34 16.5	108 18 16.6	99 16 16.1	138 24 17.3	32 3 9.3	3 0 00.0	2 0 00.0	9 3 33.3	6 1 16.6	2 0 00.0	15 3 20.0	52.1	47.9	95 18 18.9	38 4 10.5	57 9 15 7	6 2 33.3	11 1 9.0
Fifth Grade	13.	178 28 15.7	90 14 15.5	88 14 15.9	120 19 15.8	25 3 12.0	3 0 00.0	2 0 00.0	8 3 37.5	6 1 16.6	2 0 00.0	12 2 16.6	50.5	49.5	82 15 18.2	30 4 13.3	49 6 12.2	6 2 33 3	11 1 9.0
Fourth Grade	12.	29 6 20.6	18 4 22.2	11 2 18.1	18 5 27.7	7 0 0.00	0 0 00.0	0 0 00.0	1 0 00.0	0 0 00.0	0 0 00.0	3 1 33.3	62.0	38.0	13 3 23.0	8 0 00.0	8 3 37.5	0 0 00.0	0 0 00.0

Digitized by Google

WHITE, BLACK AND RED RACES, GIVING THE PERCENTAGE OF AFFECTED ACCOMMODATION, IN BOTH MALE AND FEMALE, IN LIGHT DIFFERENT NATIONALITIES.

AFFECTIONS. Middle row of each indicates full number of each affection, while the under figures in each square show the percentage of all of the males and of the females.															Hypermetropia Compared with Myopia.						
Strabismus.			Myopia.			Hyperopia.			Astigmatism.			Spasm of Acc.			Myopia.		Hyperopia.				
Females.	No. of each Affection.	Males.	Females.	No. of each Affection.	Males.	Females.	No. of each Affection.	Males.	Females.	No. of each Affection.	Males.	Females.	No. of each Affection.	Males.	Females, per cent.	Males, per cent.	Females, per cent.	Males, per cent.	Per cent. that the school is of the whole No.	Per cent. of the Affected in same school.	Latent Hypermetropia.
0	0	0	3	9	6	2	5	3	0	1	1	0	0	0							
00	00.0	00.	4.3	13.0	8 6	3.8	7.2	4.3	00	1.4	1.4	00	00.0	00	33.3	66.6	40.0	60.0	3.3	3.3	10
0	1	1	2	6	4	6	14	8	3	9	6	1	4	3							
00	.7	00	1.4	4.3	2.9	4.3	10.1	5.8	2.3	6.5	4.5	.7	2.9	2.2	33.3	66.6	4.28	57.2	6.7	7.6	5
0	0	0	2	4	2	7	10	32	6	9	3	6	7	1							
00	00.0	00	1.15	2.3	1.15	4.0	5.8	1.8	3.4	5.2	1.8	3.4	4.0	.6	50.0	70.0	50.0	30.0	3.4	6.6	11
0	1	1	5	9	4	12	22	10	2	2	0	5	10	5							
00	.5	5	2.9	5.3	2.3	8.2	12.9	5.8	1.1	1.1	00	2.9	5.8	2.9	55.5	54.5	45.4	55.4	8.3	9.7	13
2	2	0	16	22	6	33	48	15	8	11	3	15	21	6							
.5	.5	00.0	4	5.5	1.5	8.3	12.1	3.8	2	2.7	.7	3.8	5.3	1.5	72.7	27.2	68.7	31.2	19.4	23.1	2
0	0	0	1	1	0	4	7	3	1	2	1	5	7	2							
00	00.0	00.0	1.7	1.7	00	6.8	12.0	5.2	1.7	3 4	1.7	8.6	12.0	3.4	100	00	57.1	42.7	2.8	3.7	
2	2	0	4	4	0	6	6	0	3	4	1	0	2	2							
3.1	3.1	00.0	6.2	6.2	00	9.3	9.3	00	4.6	6.2	1.6	00	3.1	3.1	100	00	100	00	3.1	4	1
0	0	0	2	5	3	9	13	4	8	4	1	2	3	1							
00	00.0	00.0	1.2	3.2	2	5.7	8.3	2.4	1.9	2.5	00	1.2	1 9	.6	40	60	69 2	30.7	7.6	5.5	
0	0	0	9	12	3	14	22	8	1	1	0	8	9	1							
00	00.0	00 0	7.6	10.2	0	11.9	18.3	6.9	.8	.8	00	6.8	7.6	.8	75	25	63.6	36.3	5.7	9.7	1
0	0	0	7	9	2	12	16	4	1	1	0	27	31	4							
00	00.0	60.0	3.1	4	.9	5.4	7.2	1.8	4	.4	00	12.2	14	1.8	4	1.8	77.7	22.3	10.8	12.6	0
0	0	0	3	3	0	4	4	0	1	1	0	13	13	0							
00	00.0	00.0	3.2	5.2	00	7.	.7	00	1.7	1.7	00	22 8	22.8	00	100	100	100	00	2.8	4.6	
0	0	0	1	1	0	5	5	0	0	0	0	8	10	2							
00	00.0	00.0	1.7	1.7	60	8.9	8.9	00	00	00	00	14.2	17.8	3.6	100	00	160	00	2,7	8.5	
0	0	0	3	5	2	1	1	0	0	0	0	5	5	0							
00	00.0	00.0	7.8	13.1	5.2	2.5	2.5	00	00	00	00	13.1	13.1	00	60	40	140	(0	1.8	2.4	
0	0	0	0	0	0	2	3	1	0	0	0	0	1	1							
00	00.0	00.0	00	00	00	6.2	9.3	3.1	00	00	00	00	3.1	3.1	00	00	66.6	33.3	1.5	.8	
0	0	0	0	0	0	0	3	3	0	0	0	1	2	1							
00	00.0	00.0	60	00	00	00	3.1	3.1	00	00	00	2.7	5.4	2.7	00	00	00	160	1.8	1.1	
0	1	1	5	8	3	4	13	9	2	2	0	5	10	5							
00	.4	.4	2.4	3.8	1.4	1.9	6.2	4.3	.9	.9	00	2.4	4.8	2.4	62.5	37.5	30.7	69.2	10.1	7.5	1
0	1	1	4	7	3	4	10	6	2	2	0	4	8	4							
00	.5	.5	2.2	3.9	1.7	2.2	5.6	3.4	1.1	1.1	00	2.2	4.4	2.2	57.1	42.8	40	60	8.7	6.2	1
0	0	0	1	1	0	0	3	3	0	0	0	1	2	1							
00	00.0	00	3.4	3.4	00	00	10.3	10.3	00	00	00	3.4	6.8	3.4	100	...	...	...	1.8	1.3	

Digitized by Google

TABLE SHOWING THE RECORD OF TWO THOUSAND AND FORTY PUPILS, OF EYES AS TO ERRORS OF REFRACTION AND SPASM OF AND DARK EYES, AND IN DIFFER-

	Average Age in Years and Months.	Whole Number Examined. / No. having affected vision. / Per Cent.	No. Having Affected Vision. Females.	No. Having Affected Vision. Males.	NATIONALITY. Upper numbers indicate how many of each nationality. Middle numbers show number affected. Lowest row of numbers show the per cent. of each nationality affected in each school. American.	German.	French.	Scotch.	Irish.	English.	Swede.	Mixed.	Per cent. of Females Examined.	Per cent. of Males Examined.	COLOR OF EYES. First row of figures in each square represents total number of eyes of that color examined. Middle row of numbers indicates number having defective eyes of that color, respectively. Third row shows percentage. Blue.	Gray.	Brown.	Hazel.	Black.
Woodland School	8.6	150 16 10.6	59 9 15.3	91 7 7.6	127 13 10.2	10 1 10.	3 0 ...	0 0 ...	6 2 33.3	2 0 ...	1 0 ...	1 0 ...	39.	61.	51 4 7.8	21 1 4.7	32 7 21.8	0 0 ...	8 0 ...
Second Grade	9.7	38 4 10.5	13 2 15.3	25 2 8.	36 3 8.3	2 1 50.	0 0 ...	0 0 ...	0 0 ...	0 0 ...	0 0 ...	0 0 ...	34.2	65.8	Color not Taken.				
First Grade	7.6	112 12 10.7	46 7 15.2	66 5 7.5	91 10 10.9	8 0 ...	3 0 ...	0 0 ...	6 2 33.3	2 0 ...	1 0 ...	1 0 ...	41.	59.	51 4 7.8	21 1 4.7	32 7 21.8	0 0 ...	8 0 ...
Washington School	11.2	312 88 28.2	183 57 31.1	129 31 24.	221 61 27.5	40 12 30.	4 0 ...	4 1 25.	15 3 20.	9 4 44.4	3 1 33.3	16 6 37.5	58.6	41.4	62 13 20 9	36 8 22.2	50 11 22.	5 2 40.	14 4 28 5
Seventh Grade	14 7	83 24 28.9	58 16 27.5	25 8 32.	70 19 27.1	8 3 37.5	1 0 ...	0 0 ...	0 0 ...	1 0 ...	0 0 ...	3 2 66.6	69.8	30.2	25 6 24.	23 7 30.4	27 7 25.9	5 2 40.	5 2 40.
Sixth Grade	14.7	43 6 13.9	28 5 17.8	15 1 6.6	36 6 16.6	4 0 ...	1 0 ...	1 0 ...	0 0 ...	0 0 ...	0 0 ...	1 0 ...	65.1	34.9	22 4 18.2	6 0 ...	14 2 14.2	0 0 ...	1 0 ...
Third Grade	10.3	39 8 20.5	16 3 18.7	23 5 21.5	29 4 13.9	8 3 37.5	0 0 ...	0 0 ...	1 1 100	1 0 ...	0 0 ...	0 0 ...	59.	41.	15 3 20.	7 1 14.2	9 2 22.2	0 0 ...	8 2 25.
Second Grade	10.3	112 41 36.6	68 26 41 2	49 15 36.6	64 26 40.6	15 3 20.	1 0 ...	1W1 1 100.	10 2 20.	7 4 57.1	2 1 50.	12 4 33.3	61.6	38.4	Color not Taken.				
First Grade	8.4	35 9 25.7	18 7 38.8	17 2 11.7	12 6 27.2	5 3 60.	1 0 ...	2 0 10.	4 0 ...	0 0 ...	1 0 ...	0 0 ...	51.4	48.6	Color not Taken.				
Nevada Schools	10.2	197 28 14 2	100 19 19.	97 9 9.	189 28 14.8	3 0 ...	0 0 ...	1 0 ...	1 0 ...	3 0 ...	0 0 ...	0 0 ...	50.7	49.3	86 12 13.8	43 4 9.	30 5 16.6	10 4 40.	28 3 10.7
Central Primary	7 yrs.	44 5 11.3	18 3 16.6	26 2 7.6	42 5 11.8	2 0 ...	0 0 ...	0 0 ...	0 0 ...	0 0 ...	0 0 ...	0 0 ...	40.9	59.1	13 0 ...	12 0 ...	9 1 11.1	7 4 57.1	3 0 ...
Seventh Grade	13.7	37 4 10.8	21 3 14.2	16 1 6.2	35 4 11.4	1 0 ...	0 0 ...	0 0 ...	0 0 ...	1 0 ...	0 0 ...	0 0 ...	56.7	43.3	10 2 20.	8 1 12.5	6 0 00	3 0 00	10 1 10.
Fourth Grade	13.3	39 4 10.2	22 2 9.	17 2 11.7	39 4 10.2	0 0 ...	0 0 ...	0 0 ...	0 0 ...	0 0 ...	0 0 ...	0 0 ...	56.4	43.6	24 2 8.3	10 2 20.	3 0 ...	0 0 ...	2 0 ...
Third Grade	9.9	38 6 15 7	20 5 25.	18 1 5.5	36 6 16.6	0 0 ...	0 0 ...	1 0 ...	0 0 ...	1 0 ...	0 0 ...	0 0 ...	52.6	47.4	17 2 11.7	5 1 20.	5 1 20.	0 0 ...	11 2 18.2
Second Grade	8.4	39 9 23 0	19 6 31.5	20 3 15.	37 9 24.3	0 0 ...	0 0 ...	0 0 ...	1 0 ...	1 0 ...	0 0 ...	0 0 ...	48.7	51.3	22 6 27.2	8 0 ...	7 3 42.8	0 0 ...	2 0 ...
Lawrence Indian		10 8 ...	5 4 ...	5 4 ...	American or Red Race.								50.	50.					
Total Examined		2040	1162	878	1422	129	26	15	67	47	11	93	56.8	43.2	629½	364	442½	99	91
Total Affected		458	290	168	300	32	5	3	20	8	3	22			122	80½	86½	32	19
Percentage		22.4	24.9	19.1	21.1	24.8	19.2	20.	29.8	17.	27.2	23.6			19.3	22.1	19.5	32.3	19.7

Twelve Schools Examined with the different grades.

Digitized by Google

WHITE, BLACK AND RED RACES, GIVING THE PERCENTAGE OF AFFECTED ACCOMMODATION, IN BOTH MALE AND FEMALE, IN LIGHT ENT NATIONALITIES.—(*Continued*).

AFFECTIONS. Middle row of each indicates full number of each affection, while the under figures in each square show the percentage of all of the males and of the females.															Hypermetropia Compared with Myopia.						
Strabismus.			Myopia.			Hyperopia.			Astigmatism.			Spasm of Acc.			Myopia.		Hyperopia.				
Females.	No. of each Affection.	Males.	Females.	No. of each Affection.	Males.	Females.	No. of each Affection.	Males.	Females.	No. of each Affection.	Males.	Females.	No. of each Affection.	Males.	Females, per cent.	Males, per cent.	Females, per cent.	Males, per cent.	Percentage that the School is of the whole No.	Percentage of the Affected in same School.	Latent Hypermetropia.
1 .6 ...	1 .6 ...	0	2 1.3 ...	7 4.6 ...	5 3.3 ...	4 2.6 ...	5 3.3 ...	1 .6 ...	2 1.3 ...	2 1.3 ...	0 00 ...	1 .6 ...	1 .6 ...	0	28.5	71.2	80	20	7.3	3.5	
0 00 ...	0 00 ...	0	1 2.6 ...	3 7.8 ...	2 5.2 ...	0 00 ...	0 00 ...	0	0 00 ...	0 00 ...	0	1 2.6 ...	1 2.6 ...	0	33.3	66.6	...	...	1.8	.8	
1 3 ...	1 .8 ...	0	1 .8 ...	4 3.5 ...	3 2.7 ...	4 3.6 ...	5 4.4 ...	1 .8 ...	2 1.7 ...	2 1.7 ...	0	0 00 ...	0 00 ...	0	25.	75.	80.	20.	5.5	2.6	
1 .3 ...	1 .3 ...	0	9 2.8 ...	17 5.4 ...	8 2.6 ...	35 11.1 ...	55 14.6 ...	20 33 ...	3 .9 ...	5 1.6 ...	2 .7 ...	9 2.9 ...	10 3.2 ...	1 .3 ...	52.9	47.	63.6	36.3	15.3	19.5	14
0 00 ...	0 00 ...	0	3 3.6 ...	6 7.2 ...	3 3.6 ...	10 12. ...	15 .18 ...	5 6. ...	1 1.2 ...	1 1.2 ...	0	2 2.4 ...	2 2.4 ...	0	50.	50.	66.6	33.3	4.	5.3	9
0 00 ...	0 00 ...	0	0 00 ...	0 00 ...	0	1 2.3 ...	2 4.6 ...	1 2.3 ...	1 2.3 ...	1 2.3 ...	0	3 7.2 ...	3 7.2 ...	0	...	...	50.	50.	2.1	1.3	5
0 00 ...	0 00 ...	0	0 0 ...	1 2.5 ...	1 2.5 ...	3 7.6 ...	7 17.9 ...	4 10.2 ...	0 00 ...	0 00 ...	0	0 00 ...	0 00 ...	0	...	100	42.8	57.1	1.9	1.7	
1 .8 ...	1 .8 ...	0	5 4.4 ...	9 .8 ...	4 3.6 ...	16 14.2 ...	24 21.2 ...	8 .7 ...	1 .8 ...	3 2.6 ...	2 1.7 ...	3 2.7 ...	4 3.5 ...	1 .8 ...	55.5	44.4	66.6	33.3	5.	9.1	
0 00 ...	0 00 ...	0	1 2.5 ...	1 2.8 ...	0	5 14.2 ...	7 20. ...	2 5.8 ...	0 00 ...	0 00 ...	0	1 2.5 ...	1 2.5 ...	0	100		71.4		1.7	2.	
3 1.5 ...	6 .3 ...	3 1.5 ...	1 .5 ...	3 1.5 ...	2 .1 ...	10 5. ...	14 7.1 ...	4 .2 ...	0 00 ...	0 00 ...	0 00 ...	5 2.5 ...	5 2.5 ...	0 00 ...	33.3	66.6	71.4	28.5	9.7	6.2	7
3 6.7 ...	5 11.3 ...	2 4.6 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	...	...	...	...	2.1	1.1	
0 00 ...	0 00 ...	0 00 ...	0 00 00	0 00 ...	0 00 ...	2 5.3 ...	3 8.1 ...	1 2.8 ...	0 00 ...	0 00 ...	0 00 ...	1 2.9 ...	1 2.7 ...	0 00 ...	...	...	66.6	33.3	1.8	.8	1
0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	1 2.5 ...	3 7.6 ...	2 5.1 ...	0 00 ...	0 00 ...	0 00 ...	1 2.5 ...	1 2.5 ...	0 00 ...	...	...	33.3	66.6	1.9	.8	
0 00 ...	1 2.6 ...	1 2.6 ...	0 00 ...	0 00 ...	0 00 ...	4 10.5 ...	4 10.5 ...	0 00 ...	0 00 ...	0 00 ...	0 00 ...	1 2.6 ...	1 2.6 ...	0 00 ...	...	...	100	...	1.8	1.3	6
0 00 ...	0 00 ...	0 00 ...	1 2.6 ...	3 7.6 ...	2 5.1 ...	3 7.6 ...	4 10.2 ...	1 2.6 ...	0 00 ...	0 00 ...	0 00 ...	2 5.1 ...	2 5.1 ...	0 00 ...	33.3	66.6	75.	25.	1.9	.2	
Granulated Lids—Scrofulous not included in Percentage of Affections.															...	...	...	...	100	100	63 3.1
7 .3	13 .6	6 .3	52 2.5	94 4.6	42 2.	125 6.	202 9.9	77 3.7	27 1.3	42 2.	15 .7	74 3.6	99 4.8	25 1.2	2.5	2.	6.	3.7			

Twelve Schools Examined with the different grades.

Digitized by Google

In the above report (tabulated) there are 2040 pupils examined, embracing seven different nationalities, besides several of mixed nationality. Of 2040 pupils examined there are—

1422 Americans,	67 Irish,
129 Germans,	47 English,
26 French,	11 Swedish,
15 Scotch,	93 Mixed.

Of the 1422 Americans, 300, or 21.1 per cent., have some anomaly of refraction.

Of the	129 Germans, 32, or 24.8	per cent.,	are	affected.
"	26 French, 5 or 19.2	"	"	"
"	15 Scotch 3, or 20	"	"	"
"	67 Irish 20, or 27.8	"	"	"
"	47 English 8, or 17	"	"	"
"	11 Swedish 3, or 27.2	"	"	"
"	93 Mixed 22, or 23.6	"	"	"

The Irish, Swedish and Germans have the highest percentage of affections, the English, French, Scotch, and Americans the lowest percentage. Out of the whole number examined (2040) 1162 were girls and 878 boys. 458 had some ametropia (some anomaly of refraction). Of the 1162 girls 290, or 24.9 per cent., were ametropic. Of the 878 boys 168, or 19.1 per cent., were ametropic, being a larger per cent. of affection among the girls than the boys. In three grades of one school, as seen in the table, the color of the eyes was not taken, but of those taken there was—

629½ pairs of blue,	99 hazel,
364 gray,	91 black.
443½ brown,	

Of the	629½ blue 12, or 19.3	per cent.	were	ametropic.
"	364 gray 80½, or 22.1	"	"	"
"	443½ brown 86½, or 19.5	"	"	"
"	99 hazel 32, or 32.3	"	"	"
"	91 black 18, or 19.7	"	"	"

blue, brown and black having the lowest per cent. of affection, the hazel having to a marked degree the largest per cent. of affections. Calling the blue and the gray the light-colored eyes and the black, brown and hazel the dark, the light have 20.3 per cent. of affections and the dark 21.3 per cent. of affections. In this calculation the eyes of the negroes are not considered. Out of the 2040 pupils—

13,	or 0.6	per cent.,	had strabismus,
94,	" 4.6	"	were myopic,
202,	" 9.9	"	" hyperopic,
42,	" 2.06	"	" astigmatic,
99,	" 4.8	"	had spasm of accommodation, and
63,	" 3.1	"	" latent hypermetropia.

We find that hypermetropia predominates; if we add latent hypermetropia and spasm of accommodation, saying nothing of astigmatism—of which the majority was hyperopic—we have 364 hypermetropes to 94 myopes, or nearly four times as many hypermetropes as myopes, or over twice as many as all the other affections taken together. In the table, as seen, all the grades from the primary through the grammar school, high school, normal school and university are represented, but in no instance, excepting the Kansas State University, is there, as we would gather from Dr. Cohn's report, anything like a gradual increase of myopia or any of the anomalies, simply or collectively. In nearly all of the schools there seems to be a higher per cent. of affection in the first years, then a little later in the course a marked diminution, and then again an increase.

Digitized by Google

Probably many of those having some trouble, after remaining in school a short time, drop out, which would account for the diminution, and then spasm of accommodation and latent hypermetropia, becoming manifest later on, or perhaps developing into myopia, would account for the increase in this affection.

School life, however, as stated above, so far as I can gather by these examinations, has little or nothing to do in the development of these anomalies. That they *exist*, however, in a *much greater* degree than is generally supposed is very evident, and that continuous use of the eyes (having these errors of refraction), whether in the school room or out of it, if not corrected, is sure to have its evil consequences. The importance of a recognition of the existence of these anomalies, of their extreme frequency, and of detecting and correcting them, is obvious enough. We should take into consideration that spasm of accommodation and latent hypermetropia frequently exist, and that these affections often develop into myopia, and if recognized early and promptly treated by rest and glasses, much suffering and irremediable trouble is averted. Cohn and others may have been able, twenty years ago, to trace the development of myopia to badly appointed school rooms; but here in America our school rooms are so carefully arranged as to light, seat, desks, ventilation, etc., that we can scarcely attribute to the work in the school room the cause of anomalies. In a very great degree these errors of refraction are congenital; frequently they are latent, and if the eyes were not overtaxed (for near work), they would never become manifest. The evil arising from work in the school room is that these errors of refraction are not perceived, and hence not corrected. If the teacher could be made to understand that the little pupil's complaining of headache, pain through the temples and weakness of the eyes or dimness of vision, arose neither from stupidity nor desire to avoid study, but that these complaints were symptoms of some defect of the organ of vision; or, what would be better still, let a competent oculist carefully examine each child as he enters upon each year of study in the school work, and his anomaly (if he have any) corrected, anomalies would gradually diminish.

CONCLUSIONS.

1. I think that the principal information gained in these examinations is that 22.4 per cent. of the school children have some anomaly of refraction or accommodation which should be recognized and corrected early.
2. That the hazel eyes, of all the colors, seem to be the ones most affected.
3. That the light eyes, upon the whole, are less liable to be ametropic than the dark.
4. That the females have a larger per cent. of anomalies than males.
5. That there is a much larger per cent. of hypermetropia than of myopia.
6. That spasm of accommodation is a frequent anomaly.
7. That the far-seeing eye of the red man is bedimmed by syphilization in the effort of civilization. (The Indian eyes seem to be emmetropic, but invariably their corneæ were more or less cloudy, in consequence of catarrhal or granular ophthalmia, most often of specific origin.)

NOTE.—Out of 458 of the full number of pupils having defective vision, the complete record of each eye of 408, taken separately, was kept, and of these 326 had both eyes affected, although not equally so. Of the other 83 only one eye was affected. The relative vision of the right eye to the left, including all cases where but one only was affected as well as where both were, was found to be as 231 : 225, *i. e.*, Vision of R. E. : V. of L. E. : : 231 : 235.

DISCUSSION.

Dr. SWAN M. BURNETT.—According to my observation the negro is beginning, under education, to develop ametropia. There is one interesting fact, and one bear-

Digitized by Google

ing directly upon a question at present exciting attention, which I noted in my report on "the relative frequency of eye diseases in the white and colored races," published in the *Archives of Ophthalmology* in 1884, to which I have seen no allusion elsewhere, and that is the relative scarcity of strabismus in the negro, except as a result of paralysis. The fact seems to point to a close connection between ametropia, the use of the eyes for near work, and strabismus. I have operated on the negro for glaucoma, but we all know, now, that the negro does not have trachoma, a fact which I was the first, I believe, to point out, eleven years ago.

LACHRYMAL DRAINAGE AND IRRIGATING CANNULA IN THE TREATMENT OF CHRONIC DACRYOCYSTIC FISTULA.

DESSÈCHEMENT LACHRYMAL ET CANULE IRRIGATOIRE DANS LE TRAITEMENT DE LA FISTULE DACRYOCYSTIQUE CHRONIQUE.

ÜBER LACHRYMALDRAINAGE UND IRRIGATIONSCANULE IN DER BEHANDLUNG CHRONISCHER THRÄNENFISTELN.

BY ARTHUR E. PRINCE, M. D.,

Jacksonville, Ill.

In offering for your criticism the device which forms the subject of this brief paper, I am not unmindful of the prejudice which exists against all forms of styles, the use of which has so generally been superceded by the employment of large probes. With their aid and that of the galvano-cautery, and the exsection of the whole or apart of the lachrymal sac, such success has been attained in the treatment of lachrymal cystic fistulæ as to make suggestions in this direction seem of comparatively little importance; yet there still occur intractable cases, unconnected with cares, which do not yield to periodic probing, or cauterization short of obliteration of the sac. The obstruction to the natural escape of the tears recurs shortly after the passage of the probes, and the secretion is caused again to flow through the fistulous opening, defeating the natural tendency to heal.

It has fallen to my lot to meet a number of such cases which have not yielded to probing or cauterization, but have recovered promptly upon the introduction of a perforated cannula, which, in addition to carrying off the secretion, permits the patient, at will, to irrigate the whole tract with cleansing and medicating fluids.

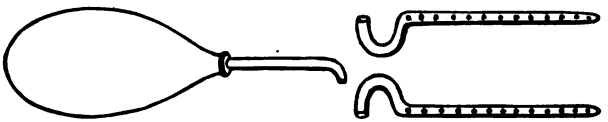
This treatment found its origin in the difficulties attending the management of the following case :—

James Young, æt. twenty-two, farmer, presented himself on account of an extensive fistulous sinus, opening in the right cheek an inch below the eye. There was a narrow stricture in the lower portion of the nasal duct and an offensive atrophic rhinitis. Hypermetropia existed of + 3 D., but with this exception the eye was normal. He was often subject to conjunctival inflammation, which materially interfered with his work. Under an anæsthetic the fistula was cauterized, and the largest of Couper's probes was passed and periodically introduced for three weeks. After this a lead style was introduced, in the hope of securing drainage, but the tendency was so strong for

Digitized by Google

the stricture of the duct to close that no good was accomplished. Failing in this manner to secure drainage and consequent healing of the fistula, the idea occurred of introducing two smaller lead wires, side by side, the upper end of each being sufficiently bent forward in the form of a spiral to enable the free end to be easily controlled by the finger. The double groove formed by the line of apposition of these two wires prevented the arrest of the secretion by the contractions of the stricture. These wires were moved upon one another daily, to dislodge any accumulations which may have formed about them. By this means complete drainage was secured, and the healing of this obstinate fistula was effected in a few days. The wires were worn for three weeks, for precaution, after which they were removed without return of trouble.

The promptness with which this obstinate case yielded after a successful drainage was established, led to the conception that a drainage cannula might be made to admit the point of an accompanying irrigating syringe, whereby the constant escape of the secretion would be facilitated by placing in the hands of the patient the means of irrigating the tract at will during the healing of the fistula.



Accordingly, in January, 1885, Tiemann made, at my suggestion, a cannula in the shape of the wires which had been employed, and perforated it along the sides and at the lower end, while the upper end was left open and turned spirally forward to admit the point of the irrigating syringe, which is made to fit the cannula.

Since devising this instrument I have used it a large number of times with variable success in the treatment of chronic blennorrhœa of the sac, the accompanying stricture being kept in a state of distention, and the medicating irrigation being effected by the patient three or more times a day. Among the above, six cases were complicated with obstinate fistula, which did not yield to the various efforts which had been employed to close them.

It seems needless to detail these cases, since they were, in a measure, similar and not connected with cares. In all the behavior was uniform. From the commencement the drainage was perfect, and the fistula healed without delay.

The irrigating solution which was used was a 25 per cent. solution of peroxide of hydrogen, the liberated oxygen from which is found to prevent, in large measure, the perforations in the cannula from closing. According to the firmness of the cicatrix, the cannula was removed in from one to three weeks. One advantage for the use of the cannula, over the other methods, even when successful, is the continuous distention of the stricture, which increases the probability of the subsequent conduction of the lachrymal secretion. They are made of silver and gold, and in pairs.

The size which I have employed has been equal to Bowman's No. 8 sound. A smaller size would accomplish the same effect with less difficulty of introduction, but would lose the advantage of distention of the stricture.

From the limited experience no generalizations can be made. We can only say that thus far their behavior has been satisfactory, and express the belief that in a large sphere of usefulness there is a limited field, in which it will be found superior to other methods.

Digitized by Google

A NEW PATTERN OF AN OPHTHALMOSCOPE.

UN NOUVEAU MODÈLE D'OPHTHALMOSCOPE.

EIN OPHTHALMOSCOP VON NEUER CONSTRUCTION.

BY PETER D. KEYSER, M. D.,

Of Philadelphia.

The nearest to perfection of an ophthalmoscope that has been given to the profession is the larger one of Knapp, with the two discs of convex and concave glasses revolving over each other. This instrument, however, is too large and too expensive for general use. Furthermore, it is not arranged so that the rotation of the discs can be made with the fingers of the hand holding the instrument up before the eye. It is so large that it will not go under the brow and close to the eye of the examiner, but rests upon the brow, which keeps it at too great a distance for proper examination and calculations in cases of refractive defects.

FIG. 1.

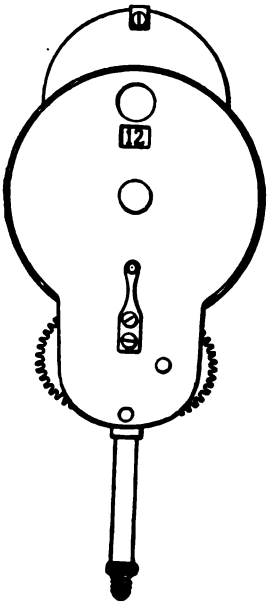


FIG. 2.

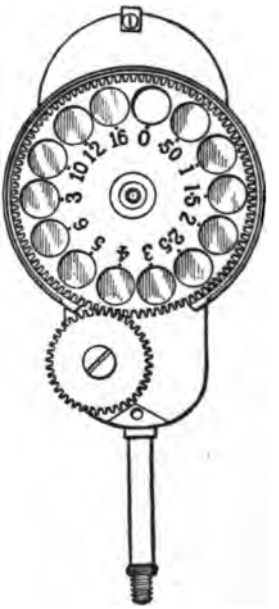
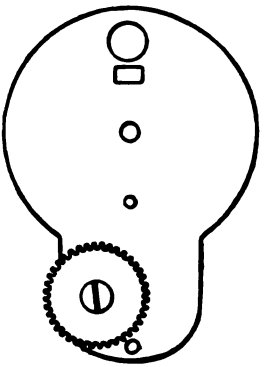


FIG. 3.



The great desideratum in an ophthalmoscope is to be able to make all the changes in the glasses during the examination without removing it from the eye, on account of the activity and ready involuntary changes of the accommodation. Most all of the later patterns of this instrument require to be removed to adjust some weaker or stronger glass to make combinations with the disc that is revolved and regulated by the finger while making the observation.

The instrument I present to you is an improvement on Knapp's original one. It is composed of two discs, containing respectively convex and concave spherical glasses, revolving immediately over each other on a common centre, and so arranged that either

Digitized by Google

or both discs can be rotated by the finger and thumb of the one hand holding the instrument before the eye. It is much smaller, so that it will go under the brow, close against the eye. It has a movable mirror, and the lenses in the discs are—

Convex—0.5, 1, 1.5, 2, 2.5, 3, 4, 5, 6, 8, 10, 12, 16.

Concave—0.25, 0.5, 1, 1.5, 2, 2.5, 3, 4, 5, 6, 8, 10, 16.

With these glasses any combination can be made, and any amount of hypermetropia, myopia or astigmatism can be measured.

It is also very handy in retinoscopy, on account of its size and close adaptation to the eye.

The accompanying wood-cuts show the mechanism and arrangement :—

Fig. 1.—Shows the instrument in its actual size ready for use.

Fig. 2.—The back plate is removed to show the lenses of one disc, the other being directly under it, and the cog-wheel attachment.

Fig. 3.—Is the back plate reversed to show the other cog wheel.

It is manufactured by Mr. E. Fox, N. W. corner Chestnut and Seventeenth streets, Philadelphia, Pa.

Digitized by Google

Digitized by Google

SECTION XII—OTOLOGY.

OFFICERS.

PRESIDENT: S. J. JONES, M. D., LL. D.

VICE-PRESIDENTS.

J. BARATOUX, M. D., Paris, France.
Prof. E. DE ROSSI, M. D., Rome, Italy.
J. J. K. DUNCANSON, M. D., Edinburgh, Scotland.
Prof. B. E. FEYER, M. D., Surgeon, U. S. Army, Kansas City, Missouri.
Prof. A. A. G. GUYE, M.D., Amsterdam, Holland.
B. LOEWENBERG, M. D., Paris, France.
F. M. PIERCE, M. D., Manchester, England.
Prof. A. POLITZER, M. D., Vienna, Austria.
Prof. U. PRITCHARD, M. D., London, England.
W. L. PURVES, M. D., London, England.
Prof. H. SCHWARTZE, M. D., Halle, Germany.

SECRETARIES.

S. O. RICHEY, M. D., Washington, D. C.
H. B. YOUNG, M. D., Burlington, Iowa.

COUNCIL.

Prof. F. Allport, M. D., St. Paul, Minnesota.
A. Alt, M. D., St. Louis, Missouri.
S. C. Ayres, M. D., Cincinnati, Ohio.
E. W. Bartlett, M. D., Milwaukee, Wisconsin.
Prof. G. E. Frothingham, M. D., Ann Arbor, Michigan.
Prof. J. F. Fulton, M. D., St. Paul, Minnesota.
Prof. C. J. Lundy, M. D., Detroit, Michigan.
Prof. W. O. Moore, M. D., New York, N. Y.
Prof. T. E. Murrell, M.D., Little Rock, Arkansas.
Prof. G. H. Powers, M. D., San Francisco, California.
Prof. G. C. Savage, M. D., Nashville, Tennessee.
Prof. A. G. Sinclair, M. D., Memphis, Tennessee.
Prof. J. G. Sinclair, M. D., Nashville Tennessee.
R. Tilley, M. D., Chicago, Illinois.
J. P. Worrell, M. D., Terre Haute, Indiana.

Digitized by Google

FIRST DAY.

THE PRESIDENT'S ADDRESS.

It is with pleasure, honored confrères, that we welcome you to this, the first, meeting of the Section of Otology in the Ninth International Medical Congress.

It is alike gratifying to us and to sufferers from ear disease that such eminent men in the medical profession should have assembled here on this occasion, from widely separated parts of the world, to consider anew, and together, the long-neglected, but important, subject of otology.

To those of us who can look back over a quarter of a century or more of professional life, the improvement that has taken place in that length of time in this one field, alone, is gratifying, and inspires a feeling of hopefulness for the future of otology, for much remains yet to be accomplished.

The little assemblage of nine medical men who, in July, 1868, met in Newport, in Rhode Island, and organized the American Otological Society, founded the first special society of co-workers of which I have knowledge, in this comparatively unexplored field. They may be regarded as practically the pioneers in scientific work in otology in America.

Following closely upon that organization came the First Otological Congress, when, in September of the same year, but thirteen of us met in Dresden, in Saxony, and held a conference of five days, and discussed some of the questions in otology that are not without interest now.

It is worthy of note that no such organized efforts as these for the advancement of otology should ever have been made until within nineteen years of the time of our assembling here to-day.

While this brief retrospect of what was not done, rather than of what was done, in the past may not be devoid of interest, and perhaps of profit, our greater interest centres on what may be done on this occasion, and in the future, for the advancement of scientific and practical otology.

It may be assumed that none present doubt the importance of the subject, or its influence upon man's well-being, from the cradle to the grave; in his physical comfort; in his mental condition; in his social relations, and as a member of the commonwealth, in determining whether he shall be a useful, productive member of society, or a more or less useless and unhappy one—too often dependent upon public or private charity.

The State, in its military and naval services, and life insurance associations, now recognize the risk to life of persons affected with suppuration within the tympanum; and yet only recently has the study of such conditions been made obligatory in medical educational institutions, which should have been of the first to recognize the importance of the subject and to have made proper provision for their study. However, a time of greater hopefulness has come. But few medical colleges now, in America, are without competent instructors in otology, and the educational insti-

Digitized by Google

tutions of other countries are adding more, and still better, facilities for the student of otology. We indulge the confident hope and expectation that the work of this Section, during the sessions on which we are now entering, will not only show a growing interest in otology, but add another link in the chain of evidence that progress is being made in removing it from the position which it so long held among the *opprobria medicinæ*.

The ophthalmoscope proved an aid to the neurologist as well as to the ophthalmologist in accuracy of diagnosis, so essential for correct therapeutics and surgery.

The advances made in otology during the last two decades have shown how critical and scientific examination of the ear may throw important light on many of the mysterious and embarrassing cases in neurology, as well as in the wide field of general medicine. From the dissimilar anatomical conditions, inspection of changes in the distribution of the optic and the auditory nerves are not alike possible, and diagnosis in affections especially of the perceptive apparatus of the ear, and in some of the defects in its conducting apparatus, must, perhaps, always remain less reliable and less certain than in affections of the interior of the eye.

Notwithstanding these embarrassments—because of the complicated anatomy of the hearing apparatus, and because, as has been so truly said, the affected part cannot, like the eye, be removed and inspected during the life of the patient, and only the death of the individual can make possible conclusive evidence of the accuracy of diagnosis in the more difficult cases of ear disease—yet great progress has been made.

With the instruments of precision which science and skill have within a few years provided, we are approaching nearer to that accuracy of diagnosis which has given to ophthalmology much of its present attractiveness.

With these advances a wider sphere of usefulness attaches to scientific otology. It is no longer restricted to the routine of unattractive manipulation, such as once characterized the treatment of ear disease. This elevation of the subject makes more manifest the incompleteness of the work of the mere specialist, who considers and treats the local disease much as if it were in a detached portion of the human body rather than in an integral part of a complete and complicated organism, ignoring the fact that the local defect may be but one of the manifestations of general disarrangement of the system showing these special characteristics.

Only when otologists shall have recognized more generally—in theory and in practice—the importance of thorough familiarity with the relation of abnormal conditions of the ear to the entire system—both as regards cause and consequence—may it be expected that otology will hold that position in the estimation of the medical profession, and of the public, to which its importance entitles it.

If, as one of the consequences of this conference, broader and more thorough study of this subject shall result, substantial progress will follow. It will, moreover, serve to counteract the tendency to view matters from a single standpoint, with the evils that follow therefrom. There will be less of the superficial work and routine practice which are responsible for the growing disposition to regard specialism in medicine with disfavor, and which led a member of our profession to say that, when he saw the great and rapid progress in the different departments of medicine which resulted from division of labor and special skill, he came to look upon a well qualified specialist as being a doctor—and something more than a doctor—but that with the recent tendency to perversion of specialism, which leads to superficiality and unreliability, he was fast coming to regard the specialist as being something less than a doctor.

If such perversion of the object of the division of labor and special lines of study

Digitized by Google

by properly qualified medical men—who have been seeking to explore the unknown or imperfectly known avenues which lead off from the lines of study that medical men are accustomed to follow—should result, specialism must soon be regarded as an impediment to the advance of scientific medicine, and the specialist, whose course of study should have fitted him to become an expert adviser of the general practitioner, must soon become a superficial imitator of others.

The needs of otology, in its present state, demand for its elevation a high order of qualifications. The possession of such qualifications, by contributors to the work of this Section, is manifest in the carefully prepared papers which are to be presented here. They embrace subjects which cannot fail to interest us as otologists and medical men, and they will elicit, in discussion, the mature views resulting from critical study and experience which will give an added interest, and, perhaps, form a stimulus to still further investigation.

In arranging the order of presenting these papers, an effort has been made to adjust the programme, that subjects of similar nature should be so placed as to admit of their discussion as a class, and thus, as far as practicable, to avoid the necessity of taking any of the limited time of the Section in avoidable repetition. It is contemplated to include some of the yet open questions in otology in the programme. Their importance is such as to make desirable careful conference regarding them by those here assembled, who are so competent, and, we hope, equally willing, to give their views upon them, in a mutual effort to eliminate them from among the doubtful ones and place them among the established facts.

STATISTICAL REPORT OF 5700 CASES OF EAR DISEASES, CLASSIFIED BY AGE, SEX, OCCUPATION AND DISEASE; CAUSATION.

RAPPORT STATISTIQUE DE 5700 CAS DE MALADIES D'OREILLE CLASSIFIÉS PAR ORDRE D'ÂGE, DE SEXE, D'OCCUPATION ET DE MALADIE.

STATISTISCHER BERICHT ÜBER 5700 FÄLLE VON KRANKHEITEN DES OHRES, CLASSIFICIRT NACH ALTER, GESCHLECHT UND BESCHÄFTIGUNG.

BY SETH S. BISHOP, M. D.,

Of Chicago, Illinois.

The following statistical tables represent the records of 5700 cases of diseases of the ear treated during the last eight years by the Staff of the Illinois Charitable Eye and Ear Infirmary, in Chicago. I have added a relatively small number of unselected cases from the records of my private practice to supply the place of those whose records were incomplete. The purpose for which the classification was originally begun was to establish a basis of calculation of the influence, if any, exerted by occupation, age or sex, in the causation of ear diseases. The condition of each patient at the time he first presented himself at the clinic was recorded, in order to determine the relative frequency of the different diseases. As is usual in charity hospitals, a very large percentage of those who applied for treatment belonged to that class of laboring people who

Digitized by Google

have no definite trade or fixed occupation. It is interesting also to note that the last decade embraced what might be called the vagrant era of the century in this country, for there never was a time in the history of America when such vast numbers of men were unemployed and leading nomadic lives. This may partly account for the fact that about one-third of the adult males are classed as being without occupation. In order to facilitate investigation and simplify the tables as far as possible, all those occupations which were closely related to each other in nature and effects were grouped under one heading, so that the twenty-four which appear really represent about double that number. For example, under the heading of clerks were embraced salesmen, book-keepers, office employés, etc.; with teamsters were grouped car drivers, peddlers, etc.; cooks and bakers were classed together; plumbers, gas- and steam-fitters appear together; brass moulders, iron moulders, etc. were classed with iron workers; such closely allied occupations as stone cutters, stone masons, bricklayers and plasterers, in which the influences and exposures are very similar, are grouped together under the heading of day laborers, a term borrowed from the laborers themselves.

The abbreviations employed are: Ac. for acute inflammation of the middle ear; Ac. S. for acute suppurative inflammation of the same; C. N. for chronic non-suppurative inflammation of the same; C. S. for chronic suppurative inflammation of the same; Ext. for diseases of the external ear; Int. for diseases of the internal ear; D. M. for deaf mutes; W. No. for whole number.

	W. NO.	AC.	AC. S.	C. N.	C. S.	EXT.	INT.
Miners	10	...	...	6	4	...	...
Firemen	10	...	...	9	1	...	...
Coopers	10	1	3	5	1	...	...
Butchers	11	1	...	6	3	1	...
Packing house laborers	12	1	1	8	2	...	...
Engineers	13	...	...	10	2	...	1
Cigar makers	15	2	1	6	3	3	...
Plumbers	16	2	1	7	1	5	...
Boiler makers	19	...	3	10	3	3	...
Tinners	20	1	4	9	5	1	...
Shoe makers	22	...	2	14	1	4	1
Bakers	22	1	2	14	4	1	...
Printers	30	...	2	10	12	5	1
Tailors	31	...	2	18	8	2	1
Blacksmiths	38	1	2	26	3	5	1
Painters	47	3	1	26	10	6	1
Sailors	47	1	2	28	9	5	2
Railroad laborers	58	2	5	35	12	3	1
Farmers	74	...	1	55	18	...	...
Carpenters	80	3	4	57	8	6	2
Iron workers	84	4	4	54	11	10	1
Teamsters	85	12	11	33	23	6	...
Factory hands	108	6	13	59	19	9	2
Clerks	232	17	19	117	39	36	4
Day laborers	496	27	26	300	77	60	6
Total	1590	85	109	922	279	171	24

Digitized by Google

	W. NO.	AC.	AC. S.	C. N.	C. S.	EXT.	INT.	D.M.
Adult males without occupation.....	810	43	31	485	197	46	7	1
Female adults.....	1662	75	63	1070	317	106	27	4
Boys, 6 to 15 years.....	557	35	28	230	205	34	19	6
Girls, 6 to 15 years.....	562	32	22	225	232	35	11	5
Boys, under 6 years.....	243	11	21	41	125	26	8	11
Girls, under 6 years.....	276	11	26	45	139	38	9	8
Occupations.....	1590	85	109	922	279	171	24	...
Total.....	5700	292	300	3018	1494	456	105	35
Per centum of whole number...		5.1	5.3	53.	26.	8.	2.	6.

The tables show that more than one-half of all the occupations were indoor. Of the 1590 males, with fixed occupations, 920 worked out of doors and 670 indoors. Add 1662 female adults who lead indoor lives to the 670 adult males with indoor work, and we have 2332 patients who spend most of their lives within doors. To the 920 men who do outdoor labor, we may add the 810 adult males who were without trades or any definite means of subsistence, and it gives 1730 men who probably pass most of their days in the open air. Thus the excess of patients of the indoor class over those who lead outdoor lives amounts to 602, or about 15 per centum of the whole number of adults. These facts are significant when we take into account the slight difference between the number of males and females afflicted under the age of fifteen. Sex seems to have no influence in the production or prevention of these diseases. Up to the age of fifteen years ear diseases are found in both sexes nearly equally. Probably a reason for the slight difference in the number of male and female cases during the first fifteen years of life may be found in the similarity of the lives and habits of the sexes during this early period. The classes of society who afford clinical material at the medical charity institutions of this country are such that necessity compels them to abandon the pursuit of an education at about the fifteenth year, and to enter upon the bread-winning avocations. From that time the divergence in habits increases. The males are either out of doors more than ever, or confined chiefly to mercantile houses or factories. The females become domestics, clerks and shop girls. American residences and business houses are heated in cold weather by dry, hot air, and kept at a temperature of 70° F., or higher. The inmates are subjected to the action of this dry, hot air, often laden with dust and noxious gases, the greater part of every day. The skin, consequently, is very active in its functions, and kept moist by free perspiration. But, though constant exposure renders the soldier, Spartan-like, indifferent to cold and storms, housing the body makes it tender, like the hot-house plant, and sensitive to sudden and extreme changes in the air. After working all a winter day in a temperature of summer heat, these people, with the powers of resistance reduced by hunger and fatigue, pass out immediately into a frigid atmosphere with the temperature perhaps from 40 to 70 degrees F. lower than that of the workshop. The skin is chilled, the perspiration checked, and a determination of blood to some internal organ occurs. Naso-pharyngeal catarrh is probably the most frequent consequence. This result is aided by high winds and the inhalation of dust. Indeed, a very large percentage of cases of naso-pharyngeal catarrh undoubtedly are due to the irritating effects of dust; this, operating in conjunction with cold, damp air, seems largely responsible for the widespread existence of naso-pharyngeal catarrh among Americans. It is probably the most prevalent disease in this country. The importance of this fact is obvious when we consider that so large a number of middle-ear affections originate in naso-pharyngeal

Digitized by Google

inflammation, which extends from that cavity, through the Eustachian tube, to the tympanum. Critical examination of the class of patients under discussion renders it evident that a considerable majority of them have throat trouble. This being the case, whatever causes a catarrh of the nose and throat is interesting to the otologist, as a proximate cause of ear disease. The exanthemata are frequent causes of ear trouble during early childhood, but youth seems also to predispose to coryza, which is often a forerunner of tubal and tympanic catarrh. Children under fifteen years of age constitute about 28.5 per centum, or more than one-fourth of the whole number of cases. A very small proportion of children were brought for treatment during the acute stage of inflammation. Only 13 per centum were acute cases, leaving 87 per centum, or more than six times as many, who were not presented for treatment until the disease had reached a chronic stage. The tables also show that only 11 per centum of adults with middle-ear diseases were seen in the acute stage.

The tables give a somewhat large percentage of external-ear cases. Since inspissated cerumen may be regarded as a symptom and a consequence of chronic non-suppurative inflammation of the middle ear, that consideration should be given due weight in this connection.

It may be permissible to cite a few facts not demonstrated by the tables, but which, nevertheless, were impressed upon me by a personal study of these patients. Although the whole state of Illinois contributed largely to the number embraced by these statistics, a large majority were residents of Chicago, a very cosmopolitan city. The foreign element predominates. The Irish constitute a very large, and the French a very small proportion of our clinical material. The north of Europe furnishes a far greater percentage of our population than the southern portions. After considering the nationalities it will not be surprising when it is stated that the blondes exceed the brunettes in number. It was noticeable, also, that the nervous temperament was the prevailing one. Another matter of interest to the etiologist, and to the student of sociology as well, was the conspicuous absence of baldness among these people, for cold draughts of air on heads deprived of Nature's covering are considered as being a prolific cause of catarrh, by eminent authorities.

This brings us to the consideration of climatic causes. In speaking of climatic conditions as standing in a causative relation to ear diseases, it should be understood that reference is made to those atmospheric conditions which are characteristic of Chicago and its vicinity, although they may not be peculiar to it. A sudden, great fall of temperature, accompanied with increased humidity of the air, is usually followed by an increase in the attendance at the clinics of new patients with acute diseases, and of old cases with acute symptoms. These climatic variations and clinical responses have recurred with such uniformity that one may predict an increase or decrease in the number of acute diseases, with a reasonable degree of accuracy, by observing the meteorological variations. Our climate is rugged, but the people born and raised in it do not appear to share its robust character. The altitude is low, in the Mississippi valley, the air moist and the thermometric changes sudden and great. It is not unusual for the thermometer to fall 20 or 30 degrees F., or more, in a few hours. Indeed, cold waves sweep suddenly over the country in summer time, cooling the heated atmosphere so quickly and thoroughly that one must needs change from summer to winter garments, with haste, or suffer from the chilling winds. Add to these causes of great circulatory disturbances the irritating effects of constantly inhaled dust which the ceaseless winds keep in never-ending motion, and the problem of the prevalence of naso-pharyngeal, tubal, and tympanic catarrh in this climate is largely solved.

Digitized by Google

DISCUSSION.

Professor G. E. FROTHINGHAM, of Ann Arbor, Michigan, said that he had been much interested in the valuable statistical report presented by Dr. Bishop. It is upon carefully collected and carefully considered statistics that we must largely rely for progress in the development of otology. The statistics presented by Dr. Bishop tend to establish the fact that those who live in large cities, especially if exposed to sudden changes of temperature, as are residents of Chicago, become more frequently affected with inflammatory conditions of the middle ear than those who reside in the country, since the institution from which the statistics were gathered receives patients from all parts of the State.

The statistics also show that a larger proportion of the foreign population were thus afflicted. He believed we might find an explanation of these facts in considering the important relation existing between naso-pharyngeal catarrh and inflammatory affections of the middle ear, and the important factors in producing nasal catarrh. The air of cities is loaded, often heavily, with germs that have a tendency to excite inflammation in mucous membranes with which they come in contact. Their power to produce inflammation of the air passages, and even to produce phthisis, has long been recognized. That they may escape from this influence, we send patients who are afflicted or threatened with consumption to a high mountain region, where the air is not only free from such germs, but is also free from particles of dust and vitiating gases. The nasal passages serve the purpose of a strainer for the air passing to the lungs, and they not only warm and moisten the inspired air, but they partially free it from solid vitiating substances. These particles, lodged upon the mucous membranes of the nasal passages and pharynx, act as irritants, and so do many of the gases that are inhaled in such an atmosphere. If we combine with this irritation the effect of a sudden change of temperature, by which the surface of the body becomes cooled and the cutaneous exhalations are checked, we have the important factors in the production of naso-pharyngeal catarrh, and the consequent liability to middle-ear disease. When such a sudden lowering of the temperature occurs, and the external surface of the body is not duly protected, the mucous surfaces most irritated are most likely to become involved in inflammatory action. Baron Larrey, who investigated the causes of the prevalence of conjunctivitis in Egypt, has ascribed it to a similar cause, namely, the irritation of the eye by exposure to dust and bright light during the day, and the exposure of the surface of the body to considerable change from the warm atmosphere commonly experienced by day, to the cool atmosphere which prevailed during the night. He declared that those who protected themselves by proper covering at night did not suffer from Egyptian ophthalmia, as did those who neglected this precaution. In the same way inflammation of the mucous lining of the intestines is produced more frequently when irritated by the ingestion of unripe fruit and other indigestible food, during that period of the year when the days are warm or hot, and the nights are cool, the body, commonly, being insufficiently protected from the chilling atmosphere.

That the germs lodged upon the naso-pharyngeal mucous membrane by breathing such an atmosphere play an important part in the production of the aural disease, Professor Frothingham is quite convinced, and that prompt and efficient treatment of the post-nasal catarrh is very important in the prevention and treatment of aural catarrh. In this treatment germicides are of great importance, as he had become convinced by resorting to them for this purpose. He had found an application, by means of an atomizer, of a solution of bichloride of mercury, 1-2000 parts, one of

Digitized by Google

the most efficient remedies for post-nasal catarrh, and he believes it acted thus beneficially by destroying germs deposited from the air upon the naso-pharyngeal surface, which, in certain conditions of the system, might excite more reaction than at other times, when the vital forces of the patient might successfully resist their morbid influence. That the foreign population furnished the larger proportion of these cases is consistent with such an explanation. It furnished the larger proportion of laborers, and those who lived under bad hygienic circumstances, sleeping in small, illy-ventilated bed rooms, in which the atmosphere literally swarmed with germs.

Dr. R. Tilley, Chicago, Illinois, said—I wish to express my appreciation of the presentation of the statistics relative to the study of otology. There is, however, in connection with this presentation, a factor which has scarcely received its due weight in the tabulation. It is stated that youth is a predisposing cause. Now, I fail to perceive that youth, in itself, can, under any circumstances, be a *cause* of any disease whatever. If, under certain circumstances, the youthful organism is ushered into the world under a decided disadvantage, and on that account certain diseases are developed, we must not say that it is the *youth* which is the cause. Relative to the percentage of cases of youth, in proportion to the cases of adult life, the important fact of the much larger proportion of children in the world, relative to adults, must not be forgotten; but for the decease of a large number of the youthful members of the race, the proportion would be different. A similar observation should be made relative to those whose occupation requires them to spend their time for the most part indoors; unless we know the relative number of the individuals engaged in indoor and outdoor work, the proportion of those afflicted is of comparatively small importance. I fail to find in the report, notwithstanding the special reference to occupation, any such occupations as unquestionably tax the aural tissues; I refer to the vocation of boiler makers. It might have been an advantage, also, to have referred to such as are engaged wholly with the telephone. If there were no such case in the table it would be desirable to mention the absence of persons so engaged. Of the influence of a mere change of temperature from warm rooms to that of the external atmosphere, those who visit and live in severe regions should furnish some evidence which would contribute to the elucidation of the question.

Professor E. De Rossi, Rome, Italy.—I agree perfectly with Professor Frothingham, that a very large part of the diseases of the ear are caused by diseases of the nose and pharynx. Therefore I desire that the Congress express wishes that studies on laryngo-rhinology be united with those on otology, rather than to unite this last one with ophthalmology, with which it has no connection, and by which nothing would be gained in the interest of scientific progress.

Dr. H. B. Young, Burlington, Iowa.—As regards the influence of change of climate in the prevalence of ear diseases, it is questioned whether the sudden changes of temperature with moisture, necessarily increases ear diseases. Pointing to this is the fact that during the past year the thermometric changes have been slight and the rainfall at the minimum, at least in the northern part of our country, and yet there has been an unusual increase, relatively, of ear troubles, both new and relapsing old cases.

Dr. C. M. Hobby, Iowa City, Iowa, endorsed the remarks of Dr. Young, and especially in the statement that the temperature was extraordinarily high and remarkably uniform in the Mississippi Valley during the last summer, and that there was a great increase in the number of diseases of the ear and nose, especially acute diseases, while the heat lasted and before the evenings became cool.

Digitized by Google

Dr. G. W. ALLYN, Pittsburgh, Pennsylvania, remarked that the warm winds from the Gulf of Mexico and the cold winds from the great lakes made of Western Pennsylvania a perpetual battle-field. As a result, there were great and rapid changes in temperature and the moisture of this region, and if such changes were directly or indirectly the cause of middle-ear disease, these troubles should prevail there, and nose, throat and ear troubles are very prevalent there.

Professor J. F. FULTON, St. Paul, Minnesota, spoke of the increasing interest which is being taken in otology. He said that climate is not only a great factor in causation of diseases of the ear, but a most important one in the treatment and cure of such diseases; that many cases which could not be benefited in one climate may be cured by a change of climate. He referred to the favorable influence of a high and dry region in many forms of middle-ear catarrh; referred to a distressing case of chronic suppurative inflammation, which could not be cured in its native climate, but which soon recovered by a change of climate.

Dr. S. O. RICHEY, Washington, D. C., said that in the moist climate of Washington during the season of high temperature catarrhs are frequent; not virulent, but also, not very tractable, though slight. In the fashion of the day bacteria are made responsible for much of which they are doubtless innocent. They are mischievous, but nasal catarrhs *may* be due entirely to chilling of the surface of the body, with determination of blood to the more central and better protected regions.

Professor FROTHINGHAM said that he wished to correct the impression, if such had been created by his remarks, that he regarded all, or nearly all cases of post-nasal catarrh, and consequent aural disease, as due to these inspired germs. What he did intend to assert was his belief that this is a frequent cause, and that germicides should be more frequently used in the treatment, and that, as a germicide for these cases, a solution of the bichloride of mercury is by far the most efficient of any that he had used.

In closing the discussion of this paper, Dr. BISHOP said:—

In answer to the doubt expressed relative to my inference from the tables, that youth is a predisposing cause of diseases of the middle ear, I will say that since it is generally conceded that youth is a predisposing cause of exanthemata, as is illustrated in the cases of scarlet fever and measles; and, as it cannot be denied that a large proportion of ear affections are directly traceable to the diseases of childhood; and, in view of the facts that children are particularly prone to attacks of coryza, which is a proximate cause of tympanic inflammation; and, that a large percentage of this class of patients refer their ear troubles to the earaches and running ears of childhood; and, that the tables show that children under the age of fifteen years constituted about 28½ per centum of the whole number of cases, it appears to be a logical deduction that *youth* is a predisposing cause of diseases of the middle ear.

Professor FULTON remarked that one-half of all cases of middle-ear diseases are associated with a catarrhal condition of the naso-pharynx. He resides in what is regarded as a high and dry part of our country, St. Paul, Minnesota; while the patients who afforded the material for this paper live in a low, moist climate, lying between Lake Michigan on the east, the Mississippi river on the west, and the Ohio river on the south. Under these circumstances it is not surprising that a larger proportion than one-half of the patients, in the statistical report, were afflicted with one or another form of naso-pharyngeal catarrh. The most prevalent form was of a hypertrophic nature. There were frequent cases of enlarged tonsils, associated with granulations on the posterior wall of the pharynx, adenoid vegeta-

Digitized by Google

tions in the vault of the pharynx, and a thickened, roughened, red and boggy appearance of the mucous membrane generally, lining the pharynx and post-nasal space. In others, the tendency was not to cell proliferation, but to an atrophic condition. The columns of the fauces were extremely thin and pale; the membrane covering the posterior wall of the pharynx pale, thin and shining, with tortuous vessels, in some cases dilated and injected with blood, and conveying the impression of a weakened state of the coats of the blood vessels. This condition is quite often found in those individuals of middle age who have suffered much in early life from coryza and pharyngitis.

ON THE TREATMENT AND THE BACTERIOLOGY OF AURAL FURUNCLES.

SUR LE TRAITEMENT ET LA BACTÉRIOLOGIE DES FURONCLES AURICULAIRES.

ÜBER BEHANDLUNG DER OHRENFURUNKELN UND DIE BACTERIEN DERSELBEN.

BY DR. B. LOEWENBERG,

Of Paris, France.

I communicated as early as in 1880 to the Second International Otological Congress (Milan, 6th September, 1880) a paper "On micrococci in the ear, and on the part they play in the generation of boils and of general furunculosis."* A short abstract of the principal results of these researches has been presented in the author's name, by Professor Marcy, to the Academy of Sciences of Paris, and published in the Transactions of that Society.† Besides, I published in 1881 an *exhaustive paper* on furuncles of the external ear and furunculosis in general, written in French.‡

In the two preceding papers, as well as in the latter work, I, for the first time, theoretically and practically utilized the discovery of the parasitic nature of the disease recently made by Professor Pasteur, and corroborated by myself, with regard to boils of the external ear.

Prolonged and careful meditation upon this subject led me to these conclusions:—

1. Boils are caused by an infection from without, viz., through the ducts of the cutaneous follicles.

2. The successive outbreak of furuncles on the same individual takes place by *autocontagion*, that is, through transport of the cocci upon the skin.

3. Infection from one person to others is possible, and originates from the same process as in No. 2.

Several years later Dr. Garré's interesting researches have furnished the experimental proof of the entire correctness of my previous statements, but my anterior claims have been completely overlooked by this author as well as by all others.

Proceeding from the knowledge of the parasitic nature of boils, I rejected all emol-

---

* B. Loewenberg. "Recherches sur la présence de Micrococcus dans l'oreille malade; Considérations sur le Rôle des Microbes dans le Furoncle auriculaire, etc." *Transactions* of this Congress, pages 11, 12 (in English, in the *Archives of Otology*, 1881).

† "Comptes Rendus de l'Académie des Sciences," 1880, Vol. II, pages 555, 556.

‡ "Le Furoncle de l'Oreille et la Furonculose." (An exhaustive abstract of this paper appeared in the, unfortunately discontinued, *American Journal of Otology*, Vol, IV., pages 139–144.)

Digitized by Google

lient treatment, considering it as contrary to the end to be aimed at, because of its tendency to favor pullulation of bacteria. I substituted for it a special modification of the antiseptic treatment, intended to act at the very outset of the boils *in an abortive way*, as well as to prevent the return of the affection. This method I will now state, as well as the results of six years' practice with it. After various experiments, I more and more confined myself to the treatment which I originally had devised for otorrhœa,* that is, instillation of an *over-saturated solution of boracic acid in strong alcohol.* The effect of this mixture is excellent, with regard both to its powerful anti-bacterial action and its *absolute painlessness.* Concerning the first of these points, I refer to what I have given in detail in a previous paper on ozæna.† I here only lay stress upon the importance of using the mixture PURE in purulent aural discharge and, *à fortiori*, in boils, having seen in publications of several authors, especially American ones, who had adopted my method in otorrhœa, that they employ the remedy diluted with more or less water. This modification is, at best, only to be used in otorrhœa with very large perforation of drum membrane; but even in such cases the mixture ought to be employed in rapidly increasing concentration, and, as soon as possible, be dropped in *pure.* With boils, this should be done at once, for here we have not to deal, as in the case of otorrhœa, with the mucous lining of the tympanic cavity, so sensitive in many people. Besides, it is most important in these cases to proceed as energetically as possible, and to open a way for the remedy into the very follicle ducts. *This result, however, is only to be expected from strong alcoholic solutions*, on the following grounds. As I formerly explained,‡ we have to do here with *capillary* canals provided with a *fatty* lining. Now the level of watery liquids *sinks* in these, while alcohol, because of its chemical affinity for fats, *ascends*—in other terms, advances—while the former only, on the contrary, recede. In the present case, therefore, only strong alcoholic solutions are able to penetrate as far as to the *sedes morbi*, the interior of the follicle. We are, furthermore, authorized to expect this penetration to be favored by the spirit of wine dissolving the fatty contents of the follicles, too. Thus the alcohol and the boracic acid dissolved in it are enabled to attack the cocci energetically. (Watery solutions could only progress in this case, when strongly alkaline, by successively saponifying and dissolving the fat, but this would take a long time, and, besides, alkaline liquids are the least apt to exert an anti-bacterial action.)

The mixture of strong (even of absolute) alcohol (100 grammes) and extremely fine powder of boracic acid (20 grammes) is well shaken, and then poured into the meatus, cold or slightly warmed, as by holding a test tube containing it in the palm of the hand. The patient keeps his head inclined toward the healthy side, so much as to place the axis of this canal, its curb apart, in a vertical position. The strict keeping of the aforesaid position is the more important as it alone enables us to fill the meatus to the brim and thus, surely, to bathe superficially situated boils.§

---

* B. Loewenberg. "On the occurrence and the significance of cocco-bacteria in purulent otorrhœa, etc.," (Knapp and Moos, *Archives of Otology*, 1881, Vol. x, page 356 and following ones).

† Same author. "De la Nature et du Traitement de l'Ozène." Paris, 1884. Published in the *Union Médicale.*

‡ B. Loewenberg, "Le Furoncle, etc.," p. 36.

§ With regard to these *instillations or ear baths*, I lay great stress upon a practically important point, which does not seem to me to have been sufficiently urged. Some physicians consider it as extremely difficult, nay, as impossible, to pour in liquids so as to fill the meatus entirely, and, furthermore, to make them remain *ad libitum* in this canal without flowing over. As to the first point, it is only necessary to pour in the liquid into the *concha* in such a way as to make it flow down slowly into the meatus. If, on the contrary, the jet is directed toward the aperture of this canal, it indeed happens that air bubbles remain in the same, and thus prevent its being entirely filled. But this is unimportant in comparison with the presumed difficulty of maintaining the

Digitized by Google

As long as we have to deal with the *unopened boil*, it is quite sufficient to employ only a *saturated* solution of boracic acid in alcohol. But if pus is already discharging, I recommend the over-saturated solution, for I deem it most important in these cases to have an excess of boracic powder, destined to be gradually dissolved in the pus, and, in this way, to exert a continual antiseptic action, as I stated in a previous publication.*

With regard to the *incision of boils*, even a superficial opening often facilitates the success of the *abortive treatment*, especially so, as I presume, when the incision just happens to divide the follicle duct. It is, however, very difficult to direct the knife just that way; in cases of deeply situated furuncles one will find this even impossible, for then we have to do with a diffuse swelling, entirely filling up the canal, and, moreover, we can only use one eye and must work in a perspectively foreshortened tube.

On the other hand, deep incisions, especially in the beginning of the disease, are extremely painful; they frequently only convey a passing relief and have often to be repeated on the same boil, the more so as renewed operations may be rendered necessary by the surfaces of the wound merely sticking together. Failing to secure relief, and the necessity of repeating a very painful operation, are liable to shake the patient's confidence, a fact I repeatedly ascertained in cases operated upon by preceding physicians.

In case the aperture of the follicle should be distinctly visible, one might try to inject the solution directly into it with a very fine hypodermic syringe.

According to my observation, the very painful inflammatory period can be relieved by instillation of cocainum muriaticum. In order to obey at the same time the antibacterial plan, I employ this preparation dissolved in strong alcohol. It is difficult to understand how the cocaïne can act through the epidermis, but I have clearly noted the good effects of its applications in several cases, even with watery solutions, a fact which removes at once any doubt as to whether the effect may not be ascribable to the alcohol alone.

RESULTS OF THE TREATMENT.

In a certain number of cases these instillations of boracic alcohol directly arrest the development of the boil, while they fail in others. I am still unable to account for this diversity of action. Very likely we have to attribute it to this: In favorably ending cases the treatment had been begun at a moment when the opening of the follicle was neither too much swollen, nor too obstructed to prevent the liquid from penetrating.

In cases of boils arrested by this treatment, their characteristic sensibility† disappears after one or more instillations. As soon as this sensibility ceases, the cause is gained and the process is arrested. In every case of aural boils it is worth while to try

---

liquid any chosen time in the meatus. Well, this, too, is very easily obtained, provided that this be poured in so as to avoid all overflowing. Should this, on the contrary, happen, the meatus will be drained more or less completely. This, in my opinion, takes place by the overflowing part of the liquid acting after the manner of the longer branch of a siphon. We, therefore, have simply to take care to avoid, during the instillation, that the level of the liquid rises above the external faces of tragus and anti-tragus, and it will remain as long as it is desirable in the auditory meatus.

* *Loc. cit.* ("On the occurrence, etc.," p. 357).

† In order to ascertain the results of this treatment, we, of course, have to make the diagnosis of the disease at the earliest period possible. This can be obtained, in my opinion, by two considerations—

(*a*) Limited acute inflammation of the skin of the meatus nearly always is of a furunculous character.

(*b*) Boils possess, especially in this locality, a striking inadequateness between the apparent slightness of the inflammation and its extreme painfulness, an inadequateness which I deem characteristic.

Digitized by Google

this abortive treatment, for a positive result is invaluable for the patient, who is spared excruciating pain, lasting sometimes for many days and nights.

Should this course of treatment not succeed in stopping the development of an aural boil, it still attains another end of primary importance, that is, *the avoiding of* what I have called *auto-contagion*, in other words, the successive forming of new boils in the affected ear. We all know this to be almost the rule in this locality, and often to render life unbearable for an incredibly long time, especially to female patients. To arrest auto-contagion is the more important in aural furunculosis, as, according to my experience, the longer this disease lasts the greater is the tendency of the boils to form in parts situated nearer and nearer the drum membrane, and consequently to prove more and more painful.

Well, *in every case I have succeeded by a persevering use of this treatment in stopping this local furunculosis*, and I consider it as my duty to lay the greatest stress upon this result, the more important, as it had never been obtained, I dare say, before my publications.

According to the same principles, and with the same happy results, I use the same treatment with female patients who before or during each *monthly course* suffer from ear furuncles. As early as in 1881,* I tried to explain the latter fact, which the discovery of the parasitic nature of boils rendered the more incomprehensive. I examined the problem in the following way: After the termination of a first boil, cocci, originated with its pus very likely, remain upon the skin of the meatus, especially in its deeper parts, which escape all cleansing of this canal, or even in follicles close to the first one attacked. But these microörganisms are prevented from multiplying, either by the want of every condition favorable to their increase, or by successful resistance of the cells, or, as I first urged, I dare say by the nature of the cutaneous, especially sebaceous, secretion. Now with certain persons troubles of the general health or disorders in special organs precede or accompany each catamenial period, and we may well suppose that, now and then, the cutaneous nutrition and secretion—for instance, in the auditory meatus—may then be altered so as to allow the cocci to multiply, and finally to produce fresh boils.† Since the idea of the latency of bacteria remaining inoffensively in or upon the body and producing trouble, only when perturbations arise in the organism, has become quite commonplace. I am sure that my explanations, given six years ago, have contributed to this result. (*Vide*, also, my remarks at the first French Congress of Surgeons, during the discussion on Osteomyelitis.)

With regard to the *treatment of this singular aural furunculosis setting in periodically with the menstrual flux*, I have adopted the following measures. Besides treating the existing boils with boracic acid and alcohol, when the patients first apply to me, I, in a prophylactic way, order the same instillations to be practiced previously to the time when the disease usually occurs. (This refers as well to each catamenial period in these cases as to the seasons, spring and fall, at which times aural boils frequently affect some persons.)

The success is very striking in many cases, and it seems to me the more valuable, as this method offers absolutely no drawback. The only caution to be recommended is, whenever the patients continue for a certain time to instill the *over-saturated* solution, to direct them to have their ears syringed from time to time with a watery solution of boracic acid, in order to remove mechanically the accumulated powder, and thus to avoid any obstruction of the meatus.

---

* B. Loewenberg, "Le Furoncle, etc.," p. 29.

† The same local troubles can account for the origin of a first boil during catamenia, by favoring the pullulation of cocci originating from the atmosphere and simply deposited in the meatus.

Digitized by Google

BACTERIA IN EAR FURUNCLES.

I have undertaken bacteriological researches in a certain number of cases of still unopened boils of the meatus. In each case I first syringed this canal, and then filled it for ten minutes with a lukewarm solution of bichloride of mercury $\frac{1}{1000}$. Immediately afterward the boils were opened with a previously well heated knife. A small parcel of the pus was inoculated into Agar-agar, or nutrient gelatine, and plate cultivations made of the whole.

I obtained the following results:—

The microörganism most frequently found was Staphylococcus albus, which was absent in only one case; then came Staphylococcus aureus, and sometimes citreus. Only in one case all these three Staph. were traced together.

These results differ from those obtained by my friend Dr. Kirchner, of Würzburg,* who only found Staphylococcus albus.

DISCUSSION.

Professor G. E. Frothingham, Ann Arbor, Michigan, thinks the subject introduced by the two papers important, and that the paper of Dr. Loewenberg suggests that the microbe is not yet "mustered out of service," as it were. Originally, Professor Frothingham said, he had been a skeptic as to the part that microörganisms played in inflammatory affections, and still he cannot give his consent to all the claims made in this direction. Careful observation of diseases and the effect of antibacterial remedies had, however, convinced him of the great influence these organisms have in the production of various affections, and he believes we have not yet fathomed the subject, and that new developments are yet in store for us. The detection of these pathogenic organisms in aural furuncles by Dr. Loewenberg constituted a positive contribution to the subject which we must accept, unless other sufficiently extensive observations should prove him mistaken. Dr. Loewenberg's theory of the production of furuncles would explain certain long-observed facts which have before received no such plausible explanation. It had long been observed that furuncles have a tendency to appear in successive outbreaks, and that the new crop commonly appears near the part originally affected. Popular belief, the result of experience, exists, that the early opening of furuncles seems to conduce to this succession. One of the most experienced surgeons of his acquaintance, and, indeed, he himself, had come to that conclusion a dozen years ago. Professor Frothingham believes that, in the absence of antiseptic precautions, such early openings may lead to auto-contagion, and such precautions have not, heretofore, been taken. Some, it is true, might resist this contagion, as they do that of syphilis, or gonorrhœa. General constitutional conditions and certain conditions of the nerve centres play, undoubtedly, an important part, resisting bacterial action in some cases, allowing even exaggerated action in others. This is true of syphilis and other well-established contagions.

Dr. R. Tilley, Chicago, Illinois, said:—In reference to the question of the influence of micrococci in the production of furuncles, we must give full importance to the observations of Dr. N. Kirchner, of Würzburg, Germany.

According to Dr. Loewenberg's note, the observations of Dr. Kirchner demonstrated only the presence of staphylococcus albus in such cases, so that we have only

* Dr. W. Kirchner, "Zur Ætiologie des Ohrfurunkels." *Monatschrift für Ohrenheilkunde, etc.*, 1887.

Digitized by Google

the testimony of Dr. Loewenberg relative to the presence of the staphylococcus aureus, which is the important organism in question. Under these circumstances, while we give due importance to Dr. Loewenberg's observations, it would be advantageous to refer to the observations of other investigators in connection with the presence of micrococci in the pus of unopened abscesses. Without looking up the question in particular, I am persuaded that the evidence is against the conclusions of Dr. Loewenberg.

The statement of the power of alcohol to dissolve the fat in the sebaceous follicles, requires more definite reference. Alcohol will dissolve fatty acid, but it will *not* dissolve ordinary fat. I have personally used alcoholic solutions of boric acid, with an addition of ether, to dissolve the fat in connection with suppurative conditions; not, however, on furuncles. I refer, with some hesitation, to the use of the term *furunculosis*. It is important, as scientific men, that we exert special care in the selection of appropriate terms. I am unable to see the special appropriateness of the term for the expression of the idea involved, and, if I am in error, I should be glad to be corrected.

Dr. L. Turnbull, of Philadelphia, Pa., said: We differ from our distinguished friend, Dr. Loewenberg, in that we believe that in the great majority of cases boils depend upon some constitutional derangement of the system, more especially the blood, and arise in the system, not, as he states, "by an affection from without; viz., through the ducts of the cutaneous follicles." Secondly, that the number of these furuncles depends upon the defective system and a loss in certain constituents of the blood, or, on the other hand, an increase of the fat globules, not alone "by auto-contagion," else the same results would follow on the skin of other parts of the body when boils are opened. Thirdly, we have never known of a case of "infection of one person by another." In a recent instance of a boy in a family of six boys, no case occurred but one, and they slept and were in constant contact with each other; and in the severe case reported by me, the girl was one of a large family of girls sleeping together, and yet no other case occurred. So far as we have been able to notice, no reports of any series of severe cases of furuncles have been given following out the treatment of Dr. Loewenberg.

In the use of boracic acid, we have found it to cause irritation, and sometimes small abscesses in and around the meatus. Boracic acid is one of the most feeble of the class of antiseptics; it does not compare with the bichloride of mercury or even with carbolic acid. Incisions of a free character, the use of iron with the salts of potash, wine and, above all, nourishing diet to complete the cure, with change of air subsequently, is the one successful method of treatment for the relief of boils of the ear. No operation should ever be attempted upon the tender and sensitive ear without the administration of an anæsthetic, like bromide of ethyl, or the use of a five per cent. solution of cocaine, dissolved in one or two per cent. of pure phenol, or crystallized carbolic acid, and applied hypodermically.

Digitized by Google

PECULIARITIES IN THE STRUCTURE AND DISEASES OF THE EAR OF THE NEGRO.

PARTICULARITÉS DANS LA STRUCTURE ET LES MALADIES D'OREILLE CHEZ LE NEGRE.

EIGENTHÜMLICHKEITEN IN DER STRUKTUR UND DEN KRANKHEITEN DES OHRES BEIM NEGER.

BY T. E. MURRELL, M.D.,
Of Little Rock, Arkansas.

The negro in this country is rapidly losing his African cast of features by miscegenation, so that the pure type is becoming comparatively rare, even in the Southern States. The mulatto partakes of many of the characteristics of his white progenitor, including that of a roving disposition. Hence he is met with in the cities and on the railroads in all parts of the country, both North and South. But the more distinct type of African, with his black skin, woolly hair, flat nose, small auricles, large mouth, thick lips, prognathous jaw and dolicocephalic head, is seen chiefly on the plantations and in the cities in the cotton and rice regions of the South, and is only occasionally met with in the Northern States.

One practicing medicine in a Southern region will have so many types of so-called negroes under his observation that he may overlook some of the characteristic peculiarities of this people. He will, most likely, enter on his record book "white" and "negro," without discriminating between grades of caste, and following the custom in the South, of classing as negro, or *colored*, to use a popular word, any person whose associates are negroes—a matter which inflexible social custom forces upon any one bearing the faintest suspicion of African lineage—he will have mulattoes, quadroons, octoroons and every imaginable shade of color and type of feature recorded in one category. The census returns are faulty in the same particular, in that they make only two general racial classes, and are, for this reason, next to worthless for our present purpose. Fully ninety per centum of the affections occurring among the colored, as ordinarily classed in the census returns, belong to the mixed types—mulattoes of various shades of color. In my own records I have entered as negroes all socially so regarded, but have carefully differentiated between the castes in my study of the peculiarities hereinafter mentioned.

In its strict sense, therefore, negro, as here used, is intended to mean the full blood, and not the mixed races; but the slight admixture of white blood is not considered where the general features are satisfactorily African.

For several years I have been impressed with certain peculiarities in the negro, both as to some features of anatomy and as to the class of diseases most and least frequent in his ears. Since medical literature hitherto has taken no cognizance of these differences, I was the more impressed on noticing them. Excepting Dr. C. H. Burnett's mention, in his "Treatise on the Ear," of the large and straight external auditory canal noticed in the negro, I have nowhere met with any special references to this race of people in otological literature. Formerly I considered the facial contour and racial features, on the one hand, and mode of life, occupation and hygienic surroundings, on the other, sufficient to account for all the differences observed; but a larger experience and more thorough study have convinced me that there exist anomalies in these people which cannot be accounted for except by some racial peculiarity. In the Southern States we know the negro is typically odd in many respects—normally, mentally, socially and physically, and that he can live in the enjoyment of health where the white man would soon succumb to mephitic influences.

Digitized by Google

It became apparent to me some years ago that ear, throat, and nose affections were very infrequently met with in a genuine negro, while eye diseases were, perhaps, more common than in the white race.

I set about to ascertain, if possible, some of the reasons for the exemption of black people from these affections, with the results which follow:—

PECULIARITIES IN ANATOMY.

The structure of the pinna, in so far as its aid to hearing is concerned, is of minor importance, and the peculiarities in this appendage in the negro belong, therefore, rather to the study of the ethnologist than the otologist. Allowing for many exceptions, it is ordinarily quite small, with smaller and less graceful development of the sinuous elevations and depressions on its anterior surface, than in the Caucasian. The large, flat surface between the helix and anti-helix, so common in white people, is usually of more limited extent in blacks. Instead of standing off from the head, it most generally adheres quite closely to it, often lying quite flatly against it. One reason for this position of the auricle may be the small size of the mastoid process, which is ordinarily but little developed and quite insignificant in external appearance. This, with the small transverse cranial measurement, presents a great narrowness to this part of the head, viewed from behind. Another reason for the closely set auricle is the predominance of the face over the occiput in the negro, by which it is thrown backward on its posterior outline. The external auditory canal presents such striking features that they cannot well be overlooked. The canal is often abnormally large in a full-sized African, sometimes almost admitting the little finger, while the largest-sized speculum in Toynbee's set can, in such cases, be introduced nearly to the drum membrane. The canal is also very straight, so much so that a speculum is seldom required to view the drum membrane. In fact, this peculiar straightness of the external auditory canal in the negro is so strongly typical that it is one of the last to be lost in approximating the Caucasian type of man, and so is to be found in nearly all mulattoes. The drum membrane can often be inspected in every detail, to its very periphery, including the annulus tympanicus, by merely turning the outer end of the canal toward a window and looking down it. The canal is also of slightly shorter average depth than in a white person, as would be inferred from the smaller transverse measurement of the negro skull. The integument lining the canal is commonly black, or deeply pigmented, but occasionally it is free from pigment in the blackest individual. The membrana tympani offers but few peculiarities, if we except the distinctness and ease with which it can ordinarily be seen. It is of necessity large in its diameters, to correspond with the size of the outer canal. The angle it forms with the axis of the external auditory canal is somewhat less than with the tortuous canal seen in many white persons, while the angle it forms with the sagittal plane does not materially differ. As is well known, however, differences in these angles occur in the white races. But actually, the angle the membrana tympani makes with the axis of the external auditory canal is due more to the direction of the canal than to differences in the position of the membrane. If its angle be measured by a line drawn straight through the head from the centre of one membrane to the centre of the other there will be really but little, if any, difference in the angle thus formed and that similarly formed in the white person. The membrana tympani in the negro, therefore, by reason of its larger size, more direct position relative to the external auditory canal, and the large diameter, less depth, and almost perfect straightness of this canal, whereby it can sometimes be seen binocularly by viewing it at some distance, so as to admit the binocular pencil into the canal, presents a most beautiful picture of this structure, exhibiting in every detail its many interesting features with a pleasing distinctness.

No measurements of the middle ear have been made, nor have any well-marked

Digitized by Google

peculiarities in this part presented themselves. The mastoid process, as has been already mentioned, is usually quite small and contains less cell surface within than is common in white adults, while the bony exterior is generally excessively thick. It is a well-known fact that all the sinuses about the face of the negro are of much smaller dimensions than in the white race, and are encased in extremely thick bone; in fact, all the bony structures of his cranium are unusually developed. The Eustachian tubes open into an extremely wide and capacious pharynx, opposite very broad and unobstructed inferior nasal meati. The pharynx is peculiarly large in the full-blooded negro, with large mouth and great width between his malar eminences, presenting an inter-faucial space of remarkable dimensions. His nose is flat and extremely broad, with flared alæ nasi and a very low septum, which is almost never deflected. The great breadth to the inferior meati gives a large area to the choanæ and allows of unobstructed respiration.

DISEASES COMMON AND UNCOMMON IN THE NEGRO.

External Ear.—The auricle is less liable to cutaneous affections than in whites, owing, perhaps, to the greater thickness and toughness of the skin covering it. Eczema, so common in the delicate integument covering the auricle in fair-skinned and light-haired children, is quite rare in black children.

Deformities are more common than in whites, as the result of traumatism, negro children being usually very rough in their plays, and disposed to quarreling and fighting, using their teeth as a weapon of defense and offense.

The external auditory canal, being large and straight, is a trap for foreign bodies, and especially for insects. Quite a large bug can find its way into the canal. I have removed such that could by no means penetrate the ordinary canal of a white person. On the contrary, impacted cerumen is very rare in the negro. I have had about ten cases in white persons to one case in a negro, in the same number of ear patients of each. It is quite common to find an excess of wax in the canal, but there is little tendency to its complete impaction.

It is probable that the straightness and more uniform size of the canal allow of easy egress of dried cerumen, and it rids itself of such accumulations.

I have never met a case of otitis parasitica in the negro. This may be due, in part, to the fact that they mostly live in small cabins, and have fires during summer as well as winter, to do their cooking, hence mould is rare in their dwellings. Nor have I ever seen a furuncle in the external auditory canal of the negro, an affection so frequent in white people, and so very painful that, in his great abhorrence of pain, the negro would be certain to seek relief.

Middle Ear.—It is in this part, where the greater portion of all aural affections are located, that we would wish to know if these people enjoy an immunity from disease. Suppurative processes occur, both acute and chronic, but are confined almost exclusively to children. Otitis media suppurativa acuta is a not infrequent affection in children, particularly those exhibiting a strumous diathesis, in whom there is often associated a catarrhal rhinitis, and rarely it goes into the chronic form, but most generally reparation takes place with little or no care. Even the chronic form in children disappears in process of time, as in the adult it is almost unknown. This is far from true in the other and more careful race, and hence there must be a reason for it. In my many examinations of the ears of negroes, I have not met an instance of chronic suppuration in the adult; but in mulattoes, who are in all respects more vulnerable than either of the pure races, it is quite common. I have never seen a case of mastoiditis in a pure negro. The rarity of suppurative processes in the middle ear, together with the small development of the mastoid process, will sufficiently account for this. The catarrhal affections are also infrequent in the typical negro. Subacute catarrh of the middle ear rarely occurs from a cold in the head, and the negro is but little subject to acute

Digitized by Google

rhinitis. Otitis media catarrhalis acuta occurs in children in whom nasal and pharyngeal disease exist, but in a far less proportion than in mulattoes and whites.

Chronic aural catarrh is so extremely rare in this race of people, that they may be considered to enjoy almost a complete exemption from it. Out of 421 aural affections among negroes, not one of chronic aural catarrh has been met. Yet, no doubt, it does occur. Certainly they do not apply for treatment, and were it as common as in the white races, there would be a far greater proportion of dullness of hearing among them. We would seek for the cause of this immunity from affections of the middle ears in the condition of the nose and pharynx.

As has been mentioned, strumous children often suffer from chronic rhinitis, with free discharge from the anterior nares, but it invariably disappears with bodily development and is not seen in the adult. In the adult, nasal and pharyngeal diseases are quite infrequent. Out of a large number of negroes treated for eye, ear, throat and nose affections, I have treated only one adult for chronic rhinitis, which speedily disappeared under very simple remedies, a success I have never attained with a similar case in my white patients.

Frequent examinations of the naso-pharyngeal structures of negroes has rarely shown any sufficiently morbid condition to call for treatment. Hypertrophic rhinitis is extremely rare in these people, and a case of adenoid growths in the vault of the pharynx has not been met with by me. In fact, as a general rule, the naso-pharynx in the negro comes as near habitually presenting a true healthy condition of these parts as is ever to be seen. In how far peculiarities in anatomy lend an influence to this exemption from disease, and in how far vigor of body, out-of-door life and plain living conduce to such end, is a question not yet fully solved. It is known that persons who live in open houses are much less prone to head colds than are those living in close houses. The negro in the Southern States commonly lives in a very open house, and burns wood for fires, so that he does not have a sore throat, or an attack of sneezing, for every change in the weather. Moreover, his life is one almost unexceptionally out of doors and engaged in some active labor. The women as well as the men venture out in all kinds of weather, and seldom know what it is to be delicate, like white ladies. In a state of slavery their lives were very methodical, but now they are far from it. They are particularly gregarious individuals, and love almost any kind of a big gathering, and as it falls to the lot of the most of them to earn their bread by their daily labor, they choose nights for their social and religious gatherings, in which they display anything but prudence in the hours they keep. Nevertheless, they seldom suffer from such dissipations. Upon the whole their lives are simple, they are a strong, hearty people, and notwithstanding their readily succumbing to certain affections that the stronger willed white man bears up under, they are particularly free from catarrhal affections of the naso-pharyngeal, or tubal region. Not that they are altogether exempt from the consequences of exposure, far from it; for the negro is very subject to pneumonia and bronchitis; but his middle ears commonly escape, by reason of the greater exemption from invasion of his naso-pharynx by such processes. It is, therefore, upon the ground of the anatomical conformation of the naso-pharynx on the one hand, and the mode of life and physical training upon the other, that these people enjoy so much greater freedom from catarrhal processes in the middle ear, and consequent deafness, than their white fellow-citizens. It may be argued that the negro manifests an indifference about his physical well-being not seen in the white American citizen, but this is not the case when any of his special senses are involved. Besides, were the middle-ear affections as common in them as in white people, we would meet with more cases of partial deafness among them than we actually find to be the case. In almost any city of a few thousand population, one can pick out numbers of persons more or less disagreeably deaf from some chronic middle ear process, but rarely, indeed, is one of them a negro.

Digitized by Google

In very old people, in whom senile changes in their ears produce more or less deafness in white persons, there are seldom such cases met with in the negro. It is true a few old negroes are dull of hearing, but not in the proportion that the same prevails in aged white people. Excluding deaf-mutes, then, there are very few negroes who have not sufficient hearing power to understand any ordinary conversation, and I have never known one have to resort to an ear trumpet or conversation tube.

Internal Ear.—Affections involving the terminal acoustic apparatus are inflammatory, central, or traumatic. The inflammatory may be primary, as in Menière's disease, or secondary by extension from an otitis media, commonly with necrosis of the petrous bone. The central causes are lesions involving the portio mollis at its origin or in some part of its course. The traumatic may be direct, or remote. As slight affections of the auditory nerve are more or less obscure, both in diagnosis and etiology, only the graver forms will here be considered, which means, generally, more or less entire deafness, commonly associated with mutism.

As previously mentioned, chronic suppuration of the middle ear is quite rare in the negro, and I have never had a case in which there was necrosis of the temporal bone; hence, I have never seen an exfoliation of the cochlea, and deafness from this cause, in this race of people. It may rarely occur, however, as I have seen no statistics on this subject. Menière's disease I have not met in the negro, but I see no reason why it should not occur, as negro children frequently suffer from convulsions. It is probable they fall into the hands of general practitioners, who overlook the nature of the disease. This is true of all the cases in white children I have had to treat, and when they have been brought to me for consultation for deafness; afterward I have made the diagnosis from the history of the case. No such case has ever been brought to me in the negro child. The only instance of very great deafness in a negro child I have had, in twelve years' special practice amid a dense negro population, was a little boy about seven years old, who was suffering from obstruction of the Eustachian tubes caused by enlarged tonsils and pharyngitis, of which he was readily cured. As to traumatic causes of deafness, the negro is on an equal footing with the white man. In affections of the auditory nerve, of central origin, however, he has many things in his favor. It is a well-known fact that nervous diseases are much more infrequent in these people than in those higher up the scale of cerebral development. There is hardly a portion of all this country that has not at some time or other been swept by an epidemic of cerebro-spinal meningitis, leaving in its wake a very large percentage of its surviving victims inheritors of blindness, or deafness, and sometimes of both. In the Southern States many such epidemics are recorded in various localities, with their thousands of victims, but so rare is the occurrence of this affection in full-blooded negroes, that physicians of large experience who have seen a single case are few indeed.

As the result of many inquiries on this subject of physicians who have practiced medicine in southern cities and on southern plantations for many years, dating back to slavery times, I have found only one who could cite a case of cerebro-spinal fever in the negro, and this was his only instance.

It has been observed in epidemics of this strange and fatal malady that those who are in a state of high nervous tension, over-anxiety, or great mental worry, are far more likely to be stricken than are the more equable-minded, and less solicitous, or care-burdened.

The negro is proverbially light-hearted and care-free, and seldom allows a responsibility to weigh upon his mind. His inferior cerebral development also doubtless protects him from hyperæmia, and acute congestions of his brain and meninges.

Deaf-mutism, however, occurs in the negro to some extent. Statistics are unsatisfactory on this subject. The only source of information outside of the testimony of individual observers, is to be had from the United States census reports. A much

Digitized by Google

larger percentage of deaf-mutism in the colored race is here given, than my observations and inquiries had led me to imagine. But the very great error undoubtedly exists here of reporting all shades of color as colored, or negro, when by far the majority are of the mixed types.

Inquiries of many intelligent and experienced persons, both white and black, have proven to me that a deaf-mute pure negro must be somewhat of a rarity. Mulatto deaf-mutes are not so rare. And as no distinctions are made in the census tables as to shades of color, or caste, it must therefore be taken for granted that all socially regarded as negroes are so classed, which, as has been shown, will include every shade from black up to almost pure white. This fact, then, for our purpose, deprives the census returns of a greater portion of their value. Examination of these reports for 1880 shows the following proportion of deaf-mutism in white and colored in eleven States, containing the greatest negro population.

Alabama,	1 white in 1358	population.
	1 colored in 2083	"
Arkansas,	1 white in 1418	"
	1 colored in 2926	"
Florida,	1 white in 2593	"
	1 colored in 2011	"
Georgia,	1 white in 1637	"
	1 colored in 2266	"
Louisiana,	1 white in 1387	"
	1 colored in 2467	"
Wisconsin,	1 white in 1328	"
	1 colored in 1938	"
North Carolina,	1 white in 1197	"
	1 colored in 1725	"
South Carolina,	1 white in 1000	"
	1 colored in 2298	"
Tennessee,	1 white in 1310	"
	1 colored in 1679	"
Texas,	1 white in 1949	"
	1 colored in 2505	"
Virginia,	1 white in 1249	"
	1 colored in 2155	"

We here see a very much larger ratio of white deaf-mutes than colored; almost double. Florida is the only State showing a greater proportion of deaf-mutism among the colored than the white. To account for this I am unable to offer an explanation. By taking an average of the whole eleven States we find the ratio to be as follows:—

White	1 in 1484 population.
Colored	1 in 2186 "

or about three white to two colored, in a given population of each.

If we leave out Florida, the other ten States give a ratio as follows:—

White	1 in 1383 population.
Colored	1 in 2204 "

or nearly two for the white, to one for the colored.

Arkansas and South Carolina, both containing a large negro population, show more than two white to one colored in the same population of each.

The causes of deaf-mutism among negroes is not sufficiently well known to serve the purpose for which it would be very desirable here to use it. Another factor in ear affections, to which attention has of late been called, should be left out almost altogether in considering aural patients in the pure type of negro; that is, malaria. If malaria

Digitized by Google

play any active rôle in the production of aural troubles, our well-favored black man could stalk the most miasmatic regions with next to perfect ears. In conclusion, I will say I have omitted tables of statistics and reports of cases from my records, in order to avoid unnecessary tediousness, but have endeavored to present facts as they have impressed themselves upon my mind from a careful study of all the cases in my private and dispensary practice, and gathered from other physicians, and from any and all sources available. The subject is a new one, and worthy of further investigation, for scientific reasons, if for no other. My investigations have been alone and single-handed; but it is to be hoped those engaged in special practice in the Southern States will give this subject careful attention in future, and make known their observations through the proper channels.

DISCUSSION.

Dr. C. M. HOBBY, Iowa City, Iowa, thinks the excellent paper of Professor Murrell suggests the necessity of a more thorough investigation into the influence of race upon the production of disease. The census reports show fifty cases of mutism among colored people to sixty-six cases among white people. Is this difference due to race influence, or to the neglect or inability of the negro to receive observation and attention? The only reports in reference to causation of mutism among the negroes show a nearly equal proportion of mutism from cerebro-spinal fever with the white race, and the same is true of all kinds of alleged causes. Professor Murrell's objection to including under the term "colored" all possessing acknowledged African blood is undoubtedly good, but even his extensive experience among the so-called "colored" people cannot have brought to his attention any very great number of those of uncontaminated African blood.

Dr. R. TILLEY, Chicago, Illinois.—I wish to express my high appreciation of the paper which has just been read, and I am sorry the author is not present to receive my compliments. The subject is so completely new to us who practice in the North that it is necessarily difficult to speak upon it. The subject is, however, peculiarly interesting to me in connection with the relative susceptibility of the negro to the ravages of syphilis. I refer to this subject in a paper which I hope to have the honor of presenting to the Section. The statistics presented to us are exceedingly interesting, but require further corroboration, and it would be important if we could bring any influence to bear upon the officers of the next census, to render its reports more exact on this question. The writer's theoretical explanation of the absence of impacted cerumen among the negroes, because of the large size of the external meatus, scarcely corresponds with my observation. For, although I have had very little experience with the negro, yet there is a marked difference in the size of individual canals, and it has been my observation that large canals are more associated with impacted cerumen than the smaller ones.

Dr. L. TURNBULL, of Philadelphia, Pennsylvania, remarked that if he understood the paper, no mention has been made of fibrous tumors of the lobe of the ear in the negro. In his manual, "Diseases of the Ear," published in 1872, he had made special mention of the great tendency of the negro to this form of disease. He had seen several cases among negro girls of one, two, and in one instance three such tumors, which he removed by operation. In his clinic he has every year several cases, in the negro and mixed races, of acute otitis media; by negro he meant, not a brown or yellow, but a *black* man.

Dr. S. O. RICHEY, Washington, D. C.—Among charity patients of this country are to be found many Irish. Those of them over forty years of age who have ear

Digitized by Google

troubles commonly have very large meati, often impacted cerumen, and persistently impaired hearing, associated together. No one of these symptoms is the cause of any other, but we have to look to a more central common cause in the nervous system. The torpid mental and nervous organization of the negro stands in strong contrast to his exaggerated emotional nature, the offspring of his ignorance and superstition. But in this torpor of his nervous system we may find some explanation of his comparative freedom from progressive deafness. Keloid of the lobe of the ear is not uncommon in the negro race.

Professor G. E. FROTHINGHAM, Ann Arbor, Michigan, said that though Professor Murrell's paper had already been quite thoroughly and ably discussed, he wished to call attention to the statements it contained that were important in connection with the report made yesterday by Dr. Bishop, of Chicago. He desired to do so because they tend to support the theory which he advocated in the discussion of Dr. Bishop's paper. Professor Murrell declares that middle ear inflammations seldom occur among the colored people, and that they nearly all live in dwellings thoroughly ventilated, not from architectural design, but from incomplete construction. They were thus freed from exposure to contaminated atmosphere, which has a tendency, through the germs which it contains, to produce naso-pharyngeal catarrhs, and acute and chronic inflammations of the middle ear, especially when the changes of temperature are sudden and extreme. The conditions under which the population lives from which Dr. Bishop has gathered his statistics, and, so far as inflammatory diseases of the naso-pharyngeal and tympanic cavities are concerned, the results, are just the reverse; Dr. Bishop finding these inflammations prevailing to a great extent; Professor Murrell finding them very infrequent. Professor Frothingham believes the infrequency of impacted cerumen can best be explained by the absence of chronic aural catarrh. He has found this to exist in a large proportion of the cases of impacted cerumen, to such an extent as to impair the hearing seriously, often leading to progressive deafness. In many of these cases, the ceruminous glands are stimulated, and in many other cases of over stimulation of glands, we get an excessive and pathological secretion. This, with the desquamated epithelium, from the same cause, also in excess, leads to impaction. He is more inclined to this view than the one offered by Dr. Richey.

Dr. H. B. YOUNG, Burlington, Iowa, agrees with Dr. Tilley in the idea that large, straight canals do not, *per se*, tend to prevent accumulations of cerumen. Experience shows that large and straight canals are quite commonly affected in this way. It has also been noticed that in some cases there appears to be an hereditary tendency. If parents have it, adult sons and daughters may expect it.

Digitized by Google

ACUTE PRIMARY INFLAMMATION IN THE MASTOID CELLS.

INFLAMMATION PRIMAIRE AIGUË DANS LES CELLULES MASTOÏDES.

AKUTE PRIMÄRE ENTZÜNDUNG DER MASTOIDZELLEN.

BY JOHN F. FULTON, M.D., PH. D.,

St. Paul, Minnesota.

This disease is so extremely rare, or so imperfectly understood, that only a few of the standard works on otology notice it as an individual disease, but speak of it as extending from the surrounding parts; as from the tympanic cavity, or the external auditory canal. It has always seemed to the author of this paper that acute primary inflammation of the muco-periosteal lining of the pneumatic spaces of the mastoid process is sufficiently common to merit a much more careful study and better classification, than it has yet received. Several cases of this disease have come under my observation within the last few years, in which, had the true nature of their maladies been diagnosed, the patients would have been relieved of pain and agony which extended over weeks and months.

The chief symptom of this malady is pain, which is mild at first, but soon increases and becomes stinging, tearing, throbbing, until it is necessary to relieve the sufferer by anodynes. Usually the pain radiates from the region of the mastoid process in all directions, but mostly to the occipital region. Sometimes it extends downward to the neck and shoulders and upward to the temples and eyes. It is constant, gives the patient no rest, day or night. Unfortunately this symptom is not always referred to the seat of disease. It is sometimes referred to spots remote from the mastoid, and thus renders an absolute diagnosis almost impossible. Typical cases of this disease are rendered all the more obscure from the fact that there may be no indication of any other trouble with the ear, and no external manifestation of the disease. These, apparently, give us but one symptom to depend upon, as not until late in the attack is there any swelling or tenderness over the external mastoid, and when this does occur, it is apt to be diagnosed periostitis, or sub-periosteal abscess. I have operated on several cases of so-called sub-periosteal abscess, and found a small fistula leading into the mastoid cavity, and careful study of the history of such cases plainly indicates that they were those of primary suppurative inflammation of the mastoid cells which had opened externally.

A very characteristic symptom of this trouble is a feeling of fullness and pressing about the mastoid region. The patients place their hands there, feeling a need of support, and obtaining some relief from the pressure. But, even with the most painstaking care, this trouble cannot always be diagnosed with certainty. Often it is thought to be neuralgia and is so treated, the patient allowed to suffer for months, to run down in general health, and finally be relieved by nature, by the spontaneous opening of the abscess through the external plate.

These preliminary remarks indicate the great importance of a careful clinical history of this disease, together with faithful reporting of cases illustrating it. The case which first drew my attention to the disease came under treatment about three years ago. The patient was a young lady nineteen years of age. She was sent to me by one of the leading dentists of this city, for whom she was working as an office girl. Her trouble began with a slight pain behind the ear, which increased in severity from day to day; at times extended over the whole side of the face and down her back, neck and shoulders. All the pain seemed to radiate from the mastoid region. The patient had been thus suffering for several weeks. She was much run down in health, low in spirits, emaciated, and had a most careworn expression. A careful examination failed to reveal

Digitized by Google

any indication of disease of the middle ear or Eustachian tube. The drumhead was normal in appearance, and the acuteness of hearing normal. There was no accumulation of wax. The mucous membrane of the posterior nares and Eustachian tube was healthy. She complained of slight tinnitus and at times became slightly dizzy, but the pain antedated all other symptoms. Her temperature was taken and found to be 99° F. An opiate was given to relieve pain. This continued every day for nearly two weeks. Inasmuch as there was no swelling or redness over the mastoid region, I could not persuade either the patient or her physician that an operation was indicated. At last, as the suffering became unendurable, and as a very slight redness, with tenderness, showed itself immediately behind the ear, an operation was submitted to. The patient was placed under ether, the usual incision for opening the mastoid process was made behind the ear, a small piece of bone chipped out with a chisel and the mastoid cavity entered with a drill. Quite a large abscess was found, and carefully washed out and emptied of all the pus. The lining membrane was smooth, red and tumefied. There was no evidence of caries. The external plate was very thin, showing that the abscess was opening in that direction. The relief obtained by this operation was most marked. All the pain and pyrexia immediately subsided, and the patient made a most excellent and rapid recovery.

All cases of primary mastoiditis interna are not suppurative, however, as we sometimes meet with cases of very severe inflammation of this part, which do not go on to suppuration. Such condition can be best illustrated by giving in brief detail the following case:—

A young man, twenty-two years of age, was referred to me by Dr. Hand, of this city, with the following symptoms: He had had vague and indefinite pain about the head for several months. It was impossible for him to sleep; he was constantly groaning with the disagreeable symptoms about the head; was confined to his room and bed for days at a time; had been treated by various kinds of home remedies for his pain, which was supposed to be of a neuralgic nature. He was unfitted for any business. Careful examination showed a perfectly normal condition of the sound conducting apparatus. There was but very slight tenderness over the mastoid. He had some naso-pharyngeal catarrh. His acuteness of hearing was normal. There was no elevation of temperature. At times he had complained of a sensation of great pressure over the mastoid, or a sensation of fullness. Primary inflammation of the mastoid cells was diagnosed and an operation for its relief advised and performed, but no secretion whatever was found in the mastoid cells. They were extremely vascular and found to be undergoing a sclerosing process. After the operation he was completely relieved of his pain, and under the influence of tonics and supporting treatment, he was able to return to his work completely cured, in a month from the time of the operation.

Three cases of primary inflammation of the mastoid cells have been reported by Dr. Webster,* of New York. Speaking of one of his cases he says, "the membrana tympani was found intact, and there was no evidence that it had ever been perforated. The primary inflammation of the cells was such that caries of the bone was produced, resulting in a permanent fistulous opening into the external auditory canal. In Case 2 it will be remembered that the mastoid inflammation existed for some time before there was any discharge from the ear, and the latter, indeed, did not make its appearance until the former had subsided; although, in the majority of cases, there is no doubt that inflammation extends from the posterior nares along the Eustachian tube through the middle ear into the mastoid cells. Yet there is no good reason for believing that inflammation cannot, in the nature of things, originate in the mastoid cells themselves, and travel, perhaps, in the opposite direction."

* *Archives of Otology*, Vol. IX, No. 1.

Digitized by Google

Dr. Roosa* also reports a case similar to Dr. Webster's. Politzer says, "the course of primary ostitis-mastoïdis is very short in the spontaneous cases and those confined to the mastoid process; as after six or eight days the acme of the inflammation is reached, and even in suppuration and external rupture, recovery takes place after three weeks.

On the other hand, the traumatic and syphilitic inflammation, which ends in caries, has a long and uncertain course, and also those forms in which the inflammation spreads at the tympanic cavity, and the membrana tympani becomes perforated. In such cases the perforation is usually found in the postero-superior quadrant of the membrane, at the apex of the nipple-shaped elevation."

The object of this paper is to draw the attention of this Section to a disease which is most frequently overlooked, as it is one most difficult to diagnose. More careful study of the clinical features of this disease, and more extensive reports of cases illustrating it, may render its diagnosis more easy in future. It produces great pain, agony, and most alarming symptoms. It can generally be relieved by a comparatively simple operation.

DISCUSSION.

Professor E. DE ROSSI, Rome, Italy, said that pain cannot be regarded as a pathognomonic symptom of primary inflammation of the mastoid. He said that on that one symptom alone he would never decide to perform an operation such as trephining the mastoid process. He spoke of a case of primary epithelioma of the mastoid, of which the principal symptom was pain. Tuberculosis of the temporal bone is also accompanied by persistent pain.

Dr. J. BARATOUX, of Paris, France, said—I believe that the mere symptom "pain" is insufficient to allow the giving of the diagnosis of inflammation of the mastoid apophysis. I would not be guided by this mere symptom to decide upon so serious a surgical operation as trephining of the mastoid apophysis, inasmuch as it is extremely rare that we do not meet some other symptom than that of pain. There exist, in fact, in these cases, other signs of the middle-ear disease which attract the practitioner's attention.

Dr. S. O. RICHEY, Washington, D. C., believes pain alone, or even in case of chronic suppuration of the middle ear, is an insufficient indication for perforation of the mastoid process in all cases. He gave a brief history of a case of chronic suppuration, of eighteen years' duration, in which there was absence of the drum membrane and of the ossicula, except the stapes. There was also persistent pain, extending from the mastoid region of that side, over the occiput to the vertex. The patient, twice in two months, fell unconscious, the fall each time being followed by an escape of dark, fetid pus from the ear. He recovered with a cicatricial drum-membrane, without trephination. Such cases make a surgeon doubtful when he should trephine.

Dr. H. B. YOUNG, Burlington, Iowa, said, concerning the unreliability of pain alone, as an indication for perforating the mastoid, his experience with at least one case will encourage conservatism. This case had the characteristic mastoid pain, without external signs of inflammation. Perforation was suggested and Knapp's position explained. It was acceded to, but before it was done the mouth was examined, and carious teeth found. The perforation was postponed, the teeth were extracted, and eventually, though slowly, the pain subsided.

---

* *Archives of Otology*, Vol. IX, No. 1.

Digitized by Google

THE INDICATIONS FOR THE ARTIFICIAL PERFORATION OF THE MASTOID PROCESS, AND THE BEST METHOD OF PERFORMING THE OPERATION.

INDICATIONS POUR LA PERFORATION ARTIFICIELLE DU PROCÈS MASTOÏDE, ET LA MEILLEURE MÉTHODE D'OPÉRER.

DIE ANZEIGEN ZUR KÜNSTLICHEN ANBOHRUNG DES WARZENFORTSATZES, UND DIE BESTE METHODE ZUR AUSFÜHRUNG DER OPERATION.

BY G. E. FROTHINGHAM, M. D.,

Ann Arbor, Michigan.

From its tendency to involve the brain and thus endanger or destroy life, inflammation of the tympano-mastoid cavity is, perhaps, the most important of aural affections, and one that, in spite of the progress which has been made in otology within the last twenty years, has many mooted points that must be settled before we can diagnose them with sufficient accuracy or lay down positive rules for treatment. Among these questions, "What are the indications for the artificial perforation of the mastoid process?" and "What is the best method of performing the operation?" are, perhaps, the most practical. They stare not only the specialist, but the general practitioner, in the face, and upon their correct answers the life of the patient will often depend. It is true, we have much more precise knowledge on these subjects than we had even a dozen years ago, but our knowledge is still so vague, and our rules for action still so indefinite, that the most accomplished aurist will often be undecided how to act. He may regret, perhaps, after the autopsy, that he did not perforate, as thereby he might have saved his patient.* On the other hand, he may regret that he did, or that he did not operate by a different method.† To a certain extent this uncertainty is due to the inherent difficulties of the subject, and will, perhaps, always exist; but to a large extent it will probably be removed by accumulated experience and wisdom, and further records of cases are desirable as contributing to this result. Some of this vagueness may be eliminated, I believe, by a more extended discussion and analysis of facts already recorded, and it is in this hope that I present this paper for the consideration of the Section.

A brief consideration of the anatomy of the mastoid and its more important anatomical relations, and also some of its more important abnormalities in form or structure, or in the relation of those surrounding parts that affect operative procedure, will be essential to the discussion of the more practical questions found in the heading. I shall, therefore, enter into a description of these before considering the main subjects before us.

The conical projection of bone situated just behind and below the external auditory canal is the prominent anatomical feature of this region, and gives the name to this portion of the temporal bone, which is called the mastoid portion, from the Greek word μαστος (a nipple or teat). The projection itself is called the mastoid process, though many limit this term to the apex or lower extremity. In this paper the term will include the whole of this prominence, which contains so important a portion of the middle ear, namely; the mastoid cells. These cells are found to occupy the whole interior of this process, and communicate with the cavity of the tympanum by means of a large cell, or space, known as the antrum mastoideus. The mastoid portion of the

* *Archives of Otology*, Vol. XII, page 55.

† *Archives of Otology*, Vol. XII, page 52; also Vol. VIII, pages 263 and 265.

Digitized by Google

temporal bone articulates by means of a rough, thick border above, with the postero-inferior angle of the temporal, and posteriorly, with the inferior border of the occipital. The outer surface is rough, and perforated by numerous openings for the transmission of blood vessels. One of these openings is of large size and is known as the *emissaurium mastoideum*, or mastoid foramen. It is variable in its situation. Generally, it is near the posterior border, though I have found it as far forward as the centre of the external surface of the mastoid process. Quite frequently it is absent from this bone, being situated in the occipital bone, near the mastoid articulation. This opening transmits a small artery and a considerable vein, which generally, though not always, communicates with the lateral sinus. When it is situated far forward, it may come in the way of the knife in making Wilde's incision, or opening the cells. The internal surface of the mastoid bone is smooth, and presents, as one of its prominent features, a deep curved groove known as the sigmoid fossa, in which is lodged a portion of the lateral sinus. In this fossa is generally found an opening for the vein transmitted to the lateral sinus through the mastoid foramen. The situation of the sigmoid fossa, with reference to the mastoid process and posterior wall of the external *meatus auditorius* is of great importance, and until recent years its variations were undetermined and little regarded. According to Hartmann's measurements, which agree with those of Bezold and others, the distance between the posterior wall of the meatus and the sigmoid fossa is often only a few millimetres. A sharp forward curve of the transverse sinus toward the wall of the auditory canal is very common. In 100 temporal bones measured by Hartmann it was but five millimetres in one case. In five cases it was only six millimetres. In six cases it was only seven millimetres, while in 41 of the cases it was one centimetre or less. The maximum was 19 millimetres, while the average was only 11½ millimetres.* In the operation of opening the mastoid process the danger of wounding the transverse sinus will be greater in proportion as this distance is smaller, and, as no operator can tell when he may chance upon one of these cases, he should avoid those methods of operating which do not allow of ready inspection of the wound as he proceeds.

The floor of the middle cranial fossa has an important relation with the superior mastoid cells that must be constantly borne in mind, while opening into them. The floor of this fossa lies but a little above the external auditory canal, being separated from it by only a thin lamella of bone. The course of the facial nerve in the *canalis facialis* turns sharply backward on reaching the inner wall of the tympanum, and, in its course from above the oval window, downward and backward to the stylo-mastoid foramen, it lies quite near the upper and deeper mastoid cells, and is in danger if these are perforated carelessly, or too deeply. It may be reached, in some cases, at a depth of five-eighths of an inch. In penetrating with a drill some observe the rule to go no deeper than this. In using the chisel, as the wound is open for inspection, this structure may be avoided. The posterior and horizontal semicircular canals have also such relations to the mastoid cells that, in penetrating with the drill at a depth of more than five-eighths of an inch, they are liable to be encountered, especially if the drill is directed too far forward. In operating with the chisel they may be more surely avoided. The occipital artery is lodged in a deep groove at the lower and inner side of the process. The apex of the process serves for the attachment of the sterno-mastoid, splenius capitis, and trachelo-mastoid muscles. The digastric muscle is inserted into a deep fossa, situated on the inner and posterior surface. A mastoid abscess occasionally makes an opening for itself at this fossa, where the outer wall is often thin, or at the apex of the process, and burrows under these muscles deep into the neck. I have met with two such cases in my practice.

---

* *Archives of Otology*, Vol. XIII, page 139.

Digitized by Google

The size of the whole process varies much in different individuals, generally being larger in those who are strong and muscular; and in males, than in females. The number and size of the mastoid cells vary greatly in different individuals, and even in the same individual, on the two sides. They are separated from each other by thin, perforated walls of bone, and generally communicate quite freely with each other, and through the *antrum*, with the tympanic cavity. In some cases a single large space is found in the centre of the bone, and the cells exist in the form of diploë, in the walls of this central space. In the child, the wall separating this cavity, or central space, from the lateral sinus is quite thick relatively to the outer wall, and hence meningitis, or cerebral abscess, as a result of inflammation of the mastoid, is comparatively rare. The outer wall is also so thin in childhood that it is often penetrated with the knife on making Wilde's incision, and is easily opened with a blunt probe or gouge in cases requiring it. The *antrum mastoideum* is situated, in the adult, at the upper and posterior part of the inner half of the osseous meatus, and is separated from it by a thin layer of bone, perforated at its anterior and upper portion, to allow communication with the tympanic cavity. This opening is variable in size and position, and in some cases is more liable to become obstructed during inflammation of the middle ear than in others.

Although it is now twenty-six years since the operation of perforating the mastoid process was revived and established as a necessary operation under certain circumstances, the indications which call for the operation are still imperfectly formulated, and even the evidences of retained pus are so imperfectly understood that the most experienced operators have frequently been mistaken in their diagnosis, and disappointed by finding no pus where they had confidently expected, and thoroughly sought for it. It is hardly sufficient consolation, under such circumstances, to find, as often happens as an outcome, that the operation has done good by the influence of the traumatism upon the diseased bone, but until very many more cases are reported and studied, such disappointments will continue frequent and the operators will be obliged to explain the mistakes, and justify the operations as best they can. Indeed, the probabilities are that a considerable element of uncertainty will always exist as to the presence, or not, of pus, or as to the exact condition of the interior of the bone, even though the indication is plain for perforation. There will probably be certain cases where the diagnosis can only be made by its aid, and where it will be called for as the only means of knowing whether, or not, to exclude mastoid disease in the diagnosis of certain cases of cranial disease in this region, associated with pain or disease in the ear.

There are many cases, however, in which little doubt need exist. In these cases there is severe pain in the mastoid, increased perhaps at night, and shooting in every direction, and accompanied by tenderness and swelling over the bone, with elevation, and forward displacement of the auricle. The patient has a weak and rapid pulse, an anxious, haggard countenance, dry and coated tongue, is unable to sleep, or take sufficient food. There is a failure of mental and muscular power, irregular chills and fever, while every effort at moving, especially every jar, causes increased pain in the region of the mastoid. Such symptoms point most positively to the nature of the affection, and constitute a positive and urgent indication for immediate perforation. Yet, such cases, not a generation ago, were generally misunderstood, disregarded, or vaguely diagnosed as cerebral disease. Dr. Roosa tells us that during his student days he had his attention called to the importance of mastoid inflammation by some cases which he saw under the care of his preceptor, Professor Alfred C. Post, and by his recital of several others, which had come to a fatal termination, as a result of the non-recognition of the dangerous nature of the affection.* Such cases are still quite frequently neglected until past

* *New York Medical Record*, Vol. v, page 436.

Digitized by Google

all help, not only from failure on the part of the attending physician to recognize the condition, but also from the dread which inexperienced operators feel to use the trephine, or perforator, in this region. Many of them, we have little doubt, would attempt, and with success, the more safe operation with the chisel and gouge, were it properly brought to their notice, and urged for adoption in the text-books which they consult. In the child, such mistakes are often remedied by nature, the pent-up pus finding its way outward through the comparatively thin and imperfect outer table of bone. In the adult, however, the pus finds a more easy exit through the thin inner wall, especially in the vicinity of the sigmoid groove, and only in exceptional cases, by necrosis of the outer and thicker wall, or by enlargement of some unusual opening or fissure, does the pus find its way to the external surface without involving the cerebrum.

In all cases in which acute pain arises deep in the mastoid during the course of suppurative inflammation of the middle ear, and is accompanied by tenderness and swelling over the bone, and leeches and other measures calculated to relieve periosteal inflammation afford no relief, or relief for but a few hours, and on making Wilde's incision a normal or nearly normal condition of the periosteum is found, the external table of the mastoid should be opened; and the opening should not be delayed, but made at once.* Even in cases where, by reason of the early swelling and tenderness of the soft tissues covering the process, and superficial character of the pain, it seems probable that the disease is mastoid periostitis, if Wilde's incision shows not only change in the periosteum, but the bone to be also changed in appearance, then the operation should be extended and the cells opened into, as the change in the bone may be, and most likely is, due to disease in the mastoid cells instead of the periosteal inflammation, the disease beginning in the bone and extending to the periosteum and other tissues over the process.

Many cases are recorded showing the danger of even a brief delay in opening the cells, and it would, in my opinion, be safer, in all cases of mastoid disease in adults, where the symptoms are sufficiently severe to call for Wilde's incision, to extend the operation, and open into the cells. The diagnosis as to whether the periostitis, or a bone affection, causes the acute symptoms cannot be made with any certainty, and it is safer to make the operation at once, than to wait for subsequent developments to decide the diagnosis. The patient is etherized usually, and *should* be, in making the incision. It adds but a trifle, if any, to the danger of the operation to open the cells, when the operation is properly performed. If only the incision is made, and the subsequent symptoms are waited for to determine whether the cells are implicated, the reluctance of the patient to submit to another operation will most likely lead to dangerous delay. As there must always be an element of doubt in these cases, we shall give the patient the benefit of the doubt, and take the course of least danger, by opening the cells at once.

In chronic disease of the ear, with symptoms of subacute mastoid inflammation, the operation may be longer delayed, but in all cases where the patient suffers much distress from pain in the mastoid, especially if there is tenderness on palpation, and the ordinary measures do not afford relief in a few days, artificial perforation is warrantable; and if these symptoms have continued for a much longer time, rendering the patient an invalid on that account, then the operation is called for, whether the disease in the mastoid cells be purulent in character, or be a sclerosis, cholesteatomata, necrosis, caries, or any other form of disease, the result of inflammation. With acute symptoms, early operation cannot be too strongly insisted upon, for the danger of delay is

* See a full discussion of this subject by F. C. Hotz, M.D., in *Archives of Otology*, Vol. IX, p. 159. See also articles and reported cases in *Archives of Otology*, Vol. XIII, p. 151, and Vol. XVI, pages 99, 110 and 111.

Digitized by Google

great in all cases where pus is contained in the mastoid cells. To operate early, is only to follow a general principle that acute abscesses should be opened early, and, considering the close proximity of mastoid abscess to the brain, this rule becomes all the more imperative, especially when we consider the slight danger of an operation and the little prospect of spontaneous cure. According to Dr. A. H. Buck's analysis of sixty-seven cases, more than half of which occurred in persons under twenty-five years of age, and two of which occurred in children ten years of age and under, only three made a spontaneous recovery, and all of these were under the age of sixteen years.* When we consider that Schwartze attributes but two per cent. of the deaths occurring after operation to the operation itself, and that seventy per cent. of the cases were cured, and only twenty per cent. died from the disease when operated upon, including those operated on at a late period in the course of the disease, we shall see that conservatism here is an element of danger, and that, when in doubt, it is best to operate. From cases that have come under my observation, I should say that it is a general custom to delay operation too long, or to omit it altogether, even in cases where it is evidently called for. There is, up to the present time, too much reluctance, even among aurists,† to resort to this operation, and it is only eight years since one of our most progressive and justly celebrated otologists, in an extended article on "Acute Purulent Inflammation of the Middle Ear," said: "*Opening of the Mastoid Process* was never resorted to in the series of cases under consideration. Of this operation, also, I am not a great admirer, and perform it only when the cerebral symptoms are threatening, especially when at the same time the secretion has ceased more or less suddenly, the mastoid region red, swollen and tender on pressure; *i. e.*, in cases that show some probability of retention of morbid substances in the cells of the mastoid process."‡ If one so progressive and eminent confessed so recently his reluctance to resort to this operation, and his delaying to do so until the cerebral symptoms were threatening, we can hardly wonder that those less progressive, and less impressed with the dangers of delay, often allow patients to pass that stage at which life may be saved, or a cure effected by its performance.

The tendency is now to extend the indications for opening the mastoid cells, and several cases have been reported in which mastoid abscess has existed, and has been evacuated by operations, in which the surface of the mastoid was free from tenderness, and presented in every way a perfectly normal appearance. This normal appearance and condition of the external surface of the mastoid may exist together with an absence of pain in the region, the pain existing in a distant part of the head, as in the case reported by Knapp, in which he was misled by this condition and deterred from operating, the patient dying from the effect of mastoid abscess. (See *Archives of Otology*, Vol. XII, pages 44 and 55.) In a similar case, N. F. Tilden Brown perforated the mastoid process, evacuated the abscess and saved the patient. (See *Archives of Otology*, Vol. XII, page 56.) Schwartze declares that if the skin covering the upper and posterior wall of the external meatus is bulging, and there is retention of fetid pus in the middle ear, with pain in the region, and general fever, the mastoid may often require perforation, even though the external surface is sound and gives no evidence of disease in the cells. In many of these cases the indications cannot be clearly made out, and a considerable degree of uncertainty must exist as to the necessity, or even propriety, of an operation.

---

* "Diseases of the Mastoid Process," by A. H. Buck. *Archives of Otology*, Vol. III, page 216.

† See discussion of Dr. C. H. Burnett's paper read before the American Otological Society, 1883. Referred to on page 215 of Vol. XIII of *Archives of Otology*. See also statements made by Strawbridge and Theobold, quoted by Roosa in *Archives of Otology*, Vol. XVI (June, 1887), page 132.

‡ Professor H. Knapp. *Archives of Otology*, Vol. VIII, page 24.

Digitized by Google

When, in the course of a suppuration of the middle ear, the temperature rises rapidly, and continues in spite of antipyretic measures, even though there is no marked pain or tenderness in the region of the mastoid, it is warrantable to perforate the mastoid, as there is reason to suspect pus; and pus has several times been found on perforation, with this as the only indication for making the operation. In such cases it will always be doubtful, whether or not pus is pent up in the mastoid cells, as the symptom of increased fever and general headache may be due either to retained pus, sinus-thrombosis, or brain abscess. I believe the opening should be made, however, as in some cases it may evacuate pus, prevent extension to the brain, and save life. If it should be brain abscess in the vicinity of the affected ear, the opening may subsequently be enlarged, and allow of evacuation of the abscess. This was done in one case by Truckenbrod, and the patient saved.* Even if it is sinus-thrombosis, the suggestion of Zaufal † to expose the sinus, ligate it, lay it open and remove the thrombus, is worthy of consideration.

In cases of relapsing mastoid periostitis, and in all cases of persistent pain in and about the mastoid, even though there seems to be an abundant discharge of pus and an unobstructed outlet for it, or in case no disease of the tympanum exists, the mastoid should be perforated, as there is probably in these cases a hyperostosis, which the operation alone is likely to relieve. If there has been long continued or repeated attacks of otitis purulenta, then the suspicion of this condition is strengthened, and if the bone is enlarged, the evidence may be considered as quite positive. In cases of long-standing suppuration of the middle ear, with frequent exacerbations attended with pain in the mastoid, the cells should be opened.

As to the methods of perforating the mastoid process, they have been quite various. When Joseph Toynbee wrote his classical work, in 1860, speaking of disease of the mastoid process, he said: "Perforation of the mastoid process also suggests itself, and this operation may doubtless be performed in those cases where the matter is pent up in the cavity of the ear, and is causing such urgent and serious symptoms as are likely, if not relieved, to terminate in death. I have never performed this operation, but I should not scruple to do so in a case where the life of the patient was threatened. Considering the large extent of the mastoid cells, it appears to me that the best plan of operating would be to use a trephine over the middle and posterior part of the process, and to remove a portion of bone three-quarters of an inch in diameter." At the present time the trephine is probably never used in this operation, but a small trephine, about one-fourth of an inch in diameter, was for some time commonly used in perforating this process. The directions given for the use of the trephine were altogether too indefinite, considering the important relations of the part. When Wilde recommended his incision in his book published in 1853, he did not specify very exactly where it should be situated. If we take his practice as a guide, it should be situated about half an inch back of the attachment of the auricle,‡ a position at first commonly selected in making the primary incision for perforating the mastoid, as will be seen by reference to reported cases.

When Dr. Roosa wrote his extensive and valuable work on "Diseases of the Ear," in 1873, after consulting all the best authorities on the subject, he could give no more definite guide for the use of the trephine in opening the mastoid process than to direct an incision, after the manner of Wilde, parallel with the attachment of the auricle, and that after the periosteum had been dissected up, "the trephine should be worked inward, forward and upward." He says: "There can be no positive directions given

* *Archives of Otology*, Vol. xv, page 176.

† *Archives of Otology*, Vol. xv, page 182; also Vol. xiv, page 230.

‡ Wilde on "Diseases of the Ear," pages 178 and 245, first American adition.

Digitized by Google

as to the depth to which the instrument should go." The operation should go on very slowly, frequent pauses being made to see how deep the instrument has gone. It is impossible to say in a given case at what depth we shall reach the cells or free spaces, and thus make an outlet for the pus. Dr. Agnew was obliged to go five-eighths of an inch in one of his cases, and then only found sclerosed bone. Dr. D. C. Ambrose removed a piece one inch long from the mastoid process of a young woman of twenty-one years of age. The cell structure will ordinarily be found at a depth of from one-sixth to one-fourth of an inch.*

We can hardly wonder that with such directions there was a hesitancy on the part of physicians to attempt this operation. Nor would the reports of cases operated upon in this manner increase their confidence, or inspire them to undertake it. In the report made by Dr. Ambrose of the case above referred to by Dr. Roosa, he says, "After making an incision two inches and a half long, down to the bone, parallel with the auricle, and half an inch from its attachment, I separated the periosteum from the bone to an extent sufficient to admit a quarter-inch trephine, and inserted that instrument on a line with the superior border of the external meatus, and about half an inch from the attachment of the auricle, directing the instrument slightly forward in a horizontal position. After the trephine had penetrated to the depth of half an inch, and finding myself on just as firm bone as at the commencement, I heartily wished the affair was over with; but remembering that Tröltsch says that 'the depth to which we must go is sometimes considerable,' I gained courage, and persevered with the operation until I felt a slight yielding beneath the instrument. I immediately withdrew it and tried with moderate force to extract the plug of bone with bone forceps, to which, however, it did not yield in the slightest degree. Again the trephine was replaced, and after a few more gentle turns there was a very perceptible sensation of further yielding beneath the instrument; and a second time the trephine was withdrawn and a second ineffectual effort made to extract the plug, though it yielded slightly to lateral pressure. The trephine was again renewed, and after a few gentle turns withdrawn; and now the plug was easily extracted. The instrument was repeatedly withdrawn and very lightly worked after the first yielding was detected, lest by a sudden giving way of parts beneath it should be suddenly plunged into the mastoid cells, and in a moment defeat all my hopes from the operation." This plug was an inch in length, and when we consider the position of the trephine with relation to the lateral sinus and middle fossa of the base of the skull, we can but congratulate the operator that no injury to these parts caused him to wish as heartily that he had never attempted the operation, as he at one stage wished it completed.

The employment of drills soon succeeded the use of the trephine in opening the mastoid cells. Those known as Buck's drills, because recommended by him, were most used, and are generally recommended even in the latest text-books by American authors. These drills are entered usually about a quarter of an inch behind the external auditory meatus, and a little below the level with the upper wall of the canal, and by pressure and a rotatory motion caused to penetrate the bone in a direction upward, forward, and inward. The pressure exercised is often considerable, and one of the best authors (Burnett) declares that a "few turns will usually be sufficient to perforate the mastoid wall, which at the point indicated is about one-fifth of an inch." The opening thus made is usually enlarged by the use of a bit, conical in form, which grinds through the external surface, enlarging the opening, it is true, by bruising the bone, and leaving a ragged surface, which is incapable of perfect cleansing, and liable to lead to absorption of poison, and consequent constitutional disturbance. Owing to the pressure that must be exercised in order to penetrate solid bone with reasonable quickness, the

* "Treatise on Diseases of the Ear." St. John Roosa, pages 418 and 419.

Digitized by Google

drill may be driven suddenly inward, as the cell structure is reached and resistance ceases, and do fatal injury to the lateral sinus, or membranes of the brain. No ordinary precaution, such as stop arrangements, extension of a finger by the side of the drill while at work, etc., etc., which are usually adopted in operating, will wholly guard against this, and in the excitement of an operation these precautions are sometimes wholly forgotten. Arthur Hartmann tells us (see *Archives of Otology*, Vol. XIII, page 139) that when he operated on the cadaver with these drills, after the manner recommended by Buck (*Archives of Ophthalmology and Otology*, Vol. III, page 212), who sets the drill a little below the upper border of the external canal and penetrates inward and a little upward and forward, he penetrated the middle cranial fossa with the drill, in three cases out of one hundred. If this happened in operating upon the cadaver, with nothing to disturb the operator, how much more frequently might it happen to an operator disturbed by the responsibility of an operation upon a patient who is, perhaps, acting badly under an anæsthetic, and where, in seeking pus, he often penetrates to the greatest depth sanctioned by the authorities? When we consider, also, the variable position of the lateral sinus as determined by Hartmann* and others, we shall not wonder that Schwartze characterizes this mode of operating as a "drill into the darkness." (*Archiv für Otologie*, Band XIV, 202, referred to in *Archives of Otology*, Vol. VIII, page 414.)

The use of a dental engine to drive the drill, by its rapid rotation, allows the operator to penetrate the bone with very slight pressure, and to determine when the cell structure or a pus cavity is reached, without so much danger of a sudden plunge of the drill, as when using ordinary hand drills, but it is still a "drill into the darkness," and the opening made needs further enlargement, which is usually made by a burr, which bruises the bone and leads to dangers already mentioned. It may be said that the sharp gouge, or chisel, may be used to enlarge the opening, instead of the burr, but in that case there is no need of a previous use of the drill, attended as it is with these dangers.

We are told by von Troltsch that the first operation of this kind ever made was performed by Jean Louis Petit, by means of a gouge and hammer. In modern times we have not only seen the operation itself firmly established, but Professor Schwartze has also plainly demonstrated that the method employed by the great French surgeon is the best and safest that has ever been devised.

The labors of Schwartze, Hartmann, Bezold, and others have so accurately settled the anatomical questions that should determine the mode of operating, and the position of the opening, that little is left for future observation upon these points. To avoid, as far as possible, the dangers which their anatomical investigations have disclosed as incident to the older methods, the operation should be thus performed: A vertical incision should be made through the soft parts down to the bone and close to the attachment of the auricle, or but one or two millimetres behind it, and so situated that its centre is very nearly on a level with the centre of the external meatus auditorius. A second incision, about an inch long, should be made backward from the centre of the first incision and vertical to it. The soft parts should be dissected up and the periosteum raised from the bone, so as to expose an oval portion of its surface to the extent of about two centimetres in vertical diameter and one centimetre in horizontal diameter. The chisel should be applied on a level with the upper wall of the meatus, at first at an angle of about 45°, and the bone chipped off in thin layers as close to the posterior wall of the meatus as possible, and extending downward well toward the apex of the process.

* A. Hartmann, in Von Langenbeck's *Archiv für Chirurgie*, Vol. XXI; also *Archives of Otology*, Vol. XIII, page 139, and "Diseases of the Ear," by A. Hartmann, page 125, first American edition.

Digitized by Google

In working the chisel, either pressure by the hand or light blows from a small wooden hammer may be used. The opening in the bone should be funnel-shaped, large externally, and made cautiously inward and forward, parallel with the axis of the meatus, and as near as possible to it, until the cells are reached, or until the opening is made as deep as prudence will permit. Five-eighths of an inch is the limit as to depth, according to many authors, and beyond that we are in danger of injuring the facial nerve, semicircular canals or other important structures. Schwartze, who at one time countenanced a penetration to a depth of three cm., or more, in certain cases of sclerosis, subsequently meeting with an accident, changed his views, and set 2.5 cm. as the greatest depth we should penetrate (see *Archives of Otology*, Vol. VIII, page 414). In making the opening, frequent examinations should be made, not only as to the character of the tissue at the bottom of the opening, but also as to the position of the opening itself in relation to the auditory canal. The insertion of a large, blunt probe into the meatus, and pressing it against the roof, will enable us to judge whether or not we are getting above it, thus endangering the middle fossa of the base of the skull, and pressing the probe against the posterior wall of the meatus will, in the same way, enable us to judge whether we are getting too far back, and thus endangering the lateral sinus. The insertion of a small cylindrical cork, just fitting the external meatus, will serve as a convenient guide while making the opening, and save frequent insertions of a probe into the meatus to determine our whereabouts. The position of the temporal ridge is too variable to serve as a landmark as to the position of the floor of the middle cranial fossa, as at one time advised; besides, it is sometimes impossible to trace it, owing to the swollen condition of the parts covering it.

The cells having been entered, they should be explored carefully, and caseous material should be removed with a small, sharp spoon, about three millimetres wide, and necrosed or carious bone should be broken up, if necessary, and removed by the spoon, or some other instrument. The chisel should have a curved cutting edge of about five mm. and be made of good steel. It is convenient to have two chisels, one with a straight shank to make the first part of the opening, and one with both edge and shank curved, or gouge-shaped, to work in the deeper portion. Antiseptic precautions should be observed, both during the operation and the after treatment. The opening should be kept patent by means of a small silver or rubber drainage tube, and the most careful attention bestowed in every respect, as the after-treatment often requires the exercise of even more knowledge and skill than the operation itself.

Though it is difficult to decide in all cases just when to remove the drainage tube, it is safer to retain it longer than it is actually needed than to remove it too soon. In the former case it would only prolong the healing process, while in the latter case it might defeat the object of the operation, which was nearly accomplished, and would have been secured but for too much hurry in removing the tube. It is best to retain the tube until all discharge from the middle ear has ceased, or until it is extremely slight. Schwartze declares that all granulation processes in the middle ear and auditory canal, and swelling of the soft parts in the external meatus auditorius, must have disappeared, and, finally, water injected through the Eustachian tube into the middle ear must continue for a considerable period of time to escape from the outer meatus, in a perfectly clear state, and without showing particles of bone-sand, before the drainage tube should be permanently removed.

Although the second division of my subject seems to me nearly equal in importance to the first, it is regarded with indifference by many authors, as evidenced not only by their lack of uniformity in practice, but by occasional open avowal in their writings.

In a paper entitled "Clinical Observations upon the Diseases of the Mastoid Process, with an Historical Sketch of the Origin of Operations upon it," read by Professor D. B. St. John Roosa, M. D., before the Section on Ophthalmology and Otology in the

Digitized by Google

New York Academy of Medicine, March 21st last, the author, in conclusion, says: "I have said very little upon methods of operations, because I lay comparatively little stress upon them. Schwartze forsook the trephine at an early day, and took up with chisels. In this he has many followers. I have continued to use a stiff probe, and a drill. Buck continues to use the drill, while Crosby preferred a gimlet."* I was once influenced by similar views, and was long deterred from adopting Schwartze's method by some harsh criticisms which I read soon after it was brought to the notice of the profession. Until within the last two years I had used Buck's drills in all cases except two. One of these I operated upon with a drill driven by a dental engine, and in one case of extreme urgency, with no better instrument at hand, I operated successfully with a common carpenter's brad-awl and gouge. Dissatisfied with the operation by means of the drill, I made numerous operations on the cadaver, and thus compared Schwartze's method with the method by drills, and became convinced of the great superiority of Schwartze's method over that of any other that has heretofore been proposed. Since then I have operated successfully twice upon patients by means of the chisel, both cases requiring a quite deep penetration of hard bone, and the ease and rapidity with which the operations were accomplished, and the sense of relief experienced during the operation, by having the parts open to inspection, were such as to cause me to regret that I had not sooner adopted this method, though I had, fortunately, escaped any accident by the other. I am now fully impressed that the method of operating is a matter upon which *great stress* should be laid, and that the method which allows of the most complete and permanent drainage, is most free from accidental injury to important surrounding structures, and generally best adapted to meet the exigencies that arise during operation, should be determined as fully as possible and urged for general adoption, to the exclusion of less safe and efficient methods. While, with great care, it may be quite safe to perforate the mastoid process with a drill, safety must often be purchased by the sacrifice of efficiency, and the method is not, as lately claimed, "as worthy to be adopted by surgeons as Schwartze's."† It is acknowledged by those who prefer the use of the drill, that there are "pathological conditions of the mastoid process in which the drill alone could not possibly suffice for the attainment of the purpose desired."‡ Even in the cases where it is regarded as applicable, the drill leaves too small an opening, very liable to close early by granulations, or cheesy pus, and thus lead to a recurrence of the symptoms, or necessitate another operation to remove the obstructions. The opening made by the drill, even when made by skillful operators, may, from its necessarily limited extent, fail to reach the pus, and patients so operated upon have died with symptoms that caused the operator to think that pus existed at the time the operation was made, but was retained in a portion of the process not penetrated.§ In chiseling, we generally lay open more of the cells, and, if we wish, can lay them open extensively, while with the drill we open into the antrum, or but few cells at most. The other cells may not freely communicate with the cells opened into by the drill, and thus retention of pus is more likely to occur than after the extensive opening of the cells and thinning of the external table of the mastoid that results in the perforation with the chisel.

Another objection of great importance, is the nature of the wound made by the drill. The blood vessels and lymphatics of the bone are bruised, and injured in such a way, that it becomes difficult to cleanse their extremities from septic material, which is more likely to be absorbed, and lead to severe constitutional symptoms, than when the wound

---

* See *Archives of Otology*, Vol. XVI, page 132.

† "Perforation of the Mastoid Process and the After-treatment," by A. H. Buck, M.D. *New York Medical Journal*, August 28th, 1886, page 229. ‡ *Ibid.*

§ See cases reported in *Archives of Otology*, for June, 1887, pages 99 and 103.

Digitized by Google

is made with a sharp-cutting instrument, leaving a smooth surface, which is easily cleansed and kept free from poisonous germs.

The method of opening the mastoid process by the chisel is adapted to all classes of cases. It is easier of execution than perforation with the drill, and, according to my experience, does not require for its execution "more time and more manual skill," as stated by Dr. Buck in his paper above referred to.* It is, I believe, also safer; for, even if Schwartze does cite one case of death from injury done by a spicula of bone during the operation, the method is not at fault, and by proper care, accidents may be more certainly avoided by operating with the chisel, since the approach to important structures can be more surely foreseen and injury to them guarded against. When an injury is done by the chisel, the nature of the opening is such that it can be detected, the extent of the injury appreciated, and the suitable treatment adopted, and, perhaps, the patient thus saved, as was done by Dr. Knapp, in the case where he acccidentally opened the lateral sinus while so operating.†

Another advantage of this method is that the opening made by the chisel may be conveniently enlarged to almost any desirable extent, to meet indications that may arise, and allow not only exposure of all the mastoid cells, but such exposure of the dura mater as to allow of detection and successful evacuation of cerebral abscess, as in the case recently reported by Professor C. Truckenbrod, of Hamburg.‡ T. H. Gluck, also, by such use of the chisel, removed the posterior wall of the meatus, a portion of the mastoid process and temporal bone, extensively exposed the dura mater, and detected an abscess situated between it and the pia mater, and opened it, evacuating about 60 grammes of thick fetid pus. His patient, however, died the next day.§

It is not a sufficient objection to this mode of operating to urge, as some have, that a larger wound of the soft parts is needed, and hence a more conspicuous scar left after recovery, or that removing a large portion of the external table of the bone leaves an ugly depression in the skin behind the ear after the parts have healed.‖ Such objections are too trivial to be considered where so important advantages are gained.

The discovery and establishment of improved methods of operating is justly regarded as among the most important steps in surgical progress, and, in ovariotomy and scores of other operations, has contributed immensely to the life saving and other objects of this great science and art. The saying that Wenzel had spoiled a hatful of eyes in order to learn the art of cataract extraction, was not so exaggerated a statement at the time it was made, before the difficult and dangerous methods then in vogue had become obsolete, and more simple and safe methods adopted. Such was the power of von Graefe that he was able to establish rapidly the improved method he devised, and thus, no doubt, he saved thousands of eyes that would have been sacrificed by a continuance of the less perfect methods which his replaced. Although the old flap operation has long been obsolete, a description of it still lumbers the pages of most of our text-books, and one of our very latest reviews contains regrets that so much space in a new ophthalmic work should be devoted to this discarded operative procedure.¶ This conservatism on the part of authors of ophthalmic works is of little moment, since the authors themselves, in the text, set forth the greater facility in performance, and also the greater safety of the modern operation, and every teacher of note impresses it fully upon the minds of his students. It is different in the case of operations on the mastoid.

---

* *New York Medical Journal*, Vol. XLIV, page 230.

† See *Archives of Otology*, Vol. X, page 365. ‡ See *Archives of Otology*, Vol. XV, page 176.

§ See *Archives of Otology*, Vol. XII, page 176, from N. Langenbeck's *Archiv*, Vol. XXXVIII, page 556.

‖ See Buck's article, *New York Medical Journal*, Vol. XLIV, pages 229 and 330.

¶ See *Archives of Ophthalmology*, Vol. XVI, page 248.

Digitized by Google

Nearly all American authors give a detailed account of the method of perforating with the drill, and figure in a conspicuous manner the instruments used, while they either omit, or briefly mention, the more easy and safe method recommended by Schwartze, and adopted so generally in Europe, and by so many leading operators on this continent. Dr. Pomeroy, in the last edition of his excellent book on diseases of the ear, I think fairly represents the teaching of most American authors in this regard. He says: "Some form of hand drill is just now very much in vogue for opening the mastoid. The one figured here is known as Buck's. I believe it to be the best instrument for the purpose I am acquainted with."*

I believe the superiority of Schwartze's method is sufficiently well established, and its advantages are of so great importance, that it should be generally urged for adoption until some still more improved method may be devised.

DISCUSSION.

Prof. De Rossi, Rome, Italy, asked if Prof. Frothingham had ever practiced opening the mastoid in cases of chronic suppuration of the middle ear. He noted that Professor F. had first indicated the operation in these cases, despite the fact that an authority such as that of the German professor was against it. It was not the custom to practice this operation in cases of simple chronic suppuration, notwithstanding they frequently do not dry up for many years.

Dr. J. Baratoux, Paris, France, said—I fully agree with the opinion expressed by Professor De Rossi relative to surgical interference in affections of the mastoid process. I believe that we are too often tempted to perforate this process for affections which can be efficiently relieved by other means. In actual practice I rarely have to resort to this operation. I almost always resort to other means, such as early ice applications, or Wilde's incision; even in those cases which exhibit fever, delirium and pain. I have often made Wilde's incision when I have felt sure that I should find no pus beneath the periosteum, but experience has convinced me that such an incision often suffices, if the precaution is taken to give a free exit to the excretions from the external auditory canal. In many cases when, according to my observation, other surgeons would have opened the mastoid cells, I have succeeded in affording permanent relief by the use of insufflations, perforating, or enlarging old perforations of the membrane, antiseptic dressing, and Wilde's incision. So far, these simple measures have given me such satisfactory results that I do not hesitate to say that cases demanding perforation of the mastoid cells are rare. I say this, conscious of the fact that many surgeons do not hesitate to perform the operation, even in comparatively insignificant affections of the middle ear. My observations are based on an experience similar to that of my confrères, and I have never yet been obliged to perform perforation of the mastoid cells when, according to my judgment, Wilde's incision and the other precautions I have referred to have been deemed at the time sufficient. I consider the opening of the mastoid cells to be rarely a necessary operation.

Dr. S. S. Bishop, of Chicago, Illinois.—We are much indebted to Professors Fulton and Frothingham for discussing a topic which has not received the attention which its importance merits. Professor Fulton treats not only of primary inflammation of the mastoid antrum, proper, but of the mastoid cells, which constitute the

* "Diseases of the Ear," by O. D. Pomeroy. Second revised edition, 1886, page 297.

Digitized by Google

whole interior structure of the mastoid process. Had he maintained that the mastoid antrum, proper, is the seat of a primary circumscribed inflammation, limited to that part only, my experience could not have corroborated his assertion, for I do not remember to have met with a case in which such a diagnosis would have been warranted. Indeed, I cannot think that primary inflammation of the mastoid cells is frequent, except as the result of traumatic injury. I have witnessed the occurrence of the latter, but am not satisfied that it occurs independently of traumatism, or middle-ear disease. I have frequently treated cases which might have been diagnosticated inflammation of the mastoid cells, but which I believe to have been periostitis, in which the inflammation was controlled by cantharidal collodion, or essential oil of mustard, or leeches, combined with anodynes. The pain, redness, swelling, and tenderness, have often entirely disappeared under this treatment without operative interference, but I have always found such cases either associated with, or following, inflammation of the tympanic cavity.

We should heartily thank Professor Frothingham, for his paper is a most valuable contribution to the history of mastoid disease, and its treatment. To be brief, I will give only an outline of my treatment of such cases as require operative procedures. If the abortive measures mentioned fail to arrest the inflammation, and the formation of pus is inevitable, it is my practice to cut down at once upon the bone. If a sinus has formed, it is rimmed out with a drill, and sufficiently enlarged to admit of a thorough examination of the interior of the cavity. All unhealthy tissue is removed; the cavity is treated with scoop and drill, on much the same principle as the cavity of a tooth is prepared by a dentist for a filling; a solution of mercuric bichloride (1–2000) is used to cleanse the cavity, and iodoform, or powdered boracic acid, is dusted into and about it. I, like the author of the last paper, use drainage tubes, but the cases in which their use has been omitted have been attended by as fully gratifying results. In closing the wound, the periosteum, which has been preserved, is replaced; the sutures are frequent and deeply inserted, and the parts are sealed with antiseptic gauze, absorbent cotton, and net bandage.

My experience is not so extensive as that of some of the authorities quoted, but eighty-five per centum of my cases recovered completely; the other fifteen per centum passed from under my observation before a fair opportunity for a cure, and they are classed as doubtful.

I wish to express my dissatisfaction with the use of the trephine in operations on the mastoid where a sinus exists. The sinus does not afford a steady support to the fixation shaft, unless the opening in the bone is very small. If the sinus is large, the superficial layer of bone is likely to be undermined, and softened to such a degree that it gives way under the trephine, letting the instrument settle into the cavity suddenly, jeopardizing the dura mater and the lateral sinus. I have never been so unfortunate as to have injured these structures, although I have removed necrosed bone until the dura mater was exposed; but too much care cannot be exercised to protect these vital structures.

Dr. C. M. Hobby, Iowa City, Iowa, called attention to the hand bone-gouge of the ordinary general operating case as possessing the requisites, when sharp, of perfect control, rapid work, and of making the best possible wound of bone.

Professor F. C. Hotz, Chicago, Illinois.—In regard to the indications for early operation in acute suppurative inflammation of the middle ear, there is one point which cannot be emphasized strongly enough. Surgeons do not see any justification for operating in such cases as long as the pus has a free outlet through the perfo-

Digitized by Google

rated membrana tympani. I have held and expressed, and I still hold the same views as Professor Frothingham, viz., that, if judicious treatment continued for several weeks has proved insufficient to control the suppuration, or if any medicinal application, even of the mildest character, aggravates the symptoms, and the purulent discharge is very profuse, the mastoid should be opened, there being no doubt that the inflammation has extended beyond the tympanic cavity, and, though the pus has a free outlet through the drum membrane, the amount of pus in the mastoid cavity is too great to be readily drained off through the perforated drumhead. I have been, quite recently, again impressed with the importance of early operation, by a case of acute suppurative inflammation of the middle ear, which had existed over two months. I saw the case in consultation; there was no tenderness or swelling over the mastoid region; no pain in a distant part of the head; the membrana tympani showed a large central perforation, through which the pus kept welling out as fast as it could be wiped off. All means had proved inefficient, the discharge being as copious as during the first week. My opinion was that the mastoid was undoubtedly involved, and should have been opened long before. The attending surgeon had not thought of doing so, nor that there was any special indication for the operation, because the discharge of pus was so free and uninterrupted through the drumhead. The operation was not done, and a week later the patient died of acute meningitis.

Dr. G. W. Allyn, Pittsburgh, Pennsylvania, wished to know if the writer would include in his indications for opening the mastoid cells cases of chronic suppuration of the middle ear which had, for years, resisted the usual means of treatment.

Dr. R. Tilley, Chicago, Illinois.—When the statement is made that suppuration of the middle ear demands operation, it is too general; we must distinguish between the kinds of suppuration. If the suppuration persist, regardless of special attention to cleanliness and the use of antiseptic fluids, and the pus wells up in great abundance, and unquestionably arises from the mastoid region, then the operation may be justifiable. But when the suppuration is from the roof of the tympanic cavity, then the perforation of the mastoid cells would be of no avail, and it is just these cases, namely, a persistent discharge from the upper and anterior part of the middle ear, which have given me personally the greatest trouble.

Dr. S. O. Richey, Washington, D. C.—Trephination of the mastoid is like tracheotomy for diphtheria, inadvisable until the last moment, and yet we must be sure not to defer the operation until too late. The Scylla of a needless operation, and the Charybdis of not performing it, or of doing it too late, must both be avoided; and the way in which this is done depends greatly upon the judgment of the individual surgeon. The treachery of chronic suppuration is well known. Often there is no symptom of the approaching fatal termination until too late for the operation. My mind is not clear as to the unquestioned indications for the operation, and these I would like to hear clearly set forth.

Dr. A. Blitz, Minneapolis, Minnesota.—As far as my experience goes in mastoid disease, I am convinced that the operation could very often be avoided if the disease could be recognized by the practitioner of general medicine, and appropriate treatment be at once instituted, as the family physician sees the patient first, and often treats the patient until the aggravated symptoms and increased suffering frighten both physician and patient into consulting some specialist, who finds the case so far advanced that nothing but perforating the mastoid process will save the patient. Often the patient neglects to attend to a suppurative otitis media; the disease

Digitized by Google

becomes chronic, the tissues more and more break down, until exposure to cold, or this gradual extension of the disease, affects the mastoid cells. In acute inflammation of the mastoid cells, I have found continued applications of broken ice, in a rubber bag, placed over the ear day and night, together with other remedies indicated by the symptoms, best. In regard to the indications for the operation, I fully agree with the author of the paper, and am glad these points are made so clear. As to the method of operating, I must beg leave to differ with the author. The use of the chisel and hammer seems barbarous, when we can use so nice and efficient an instrument as the drill—not a small, ordinary one, of perhaps one-eighth inch in diameter—but a round burr-head drill of at least one-half inch in diameter; for if the operator has mechanical dexterity he will use the drill to far greater advantage, with more nicety and certainty, and without injury to the parts. If the opening needs to be increased, this can easily be done, and all the rough edges of the wound be made smooth and clean. With our present antiseptic precautions there is no more danger from sepsis after one operation than the other.

The operation is not so dangerous in itself as was formerly believed, and there is no reason to dread it so much, because it certainly cannot do so much harm as the disease, if the operation be delayed or not done, as many such cases, unfortunately, might show.

Dr. H. B. Young, referring to Professor Hotz's remarks, said—It occurs to me that no mention was made of a post-mortem in your case. Professor Hotz replied that there was no post-mortem. Dr. Young then reported a fatal case, in which the mastoid was opened post-mortem and no pus was found, although it was, from concomitant circumstances, certainly to be expected.

Professor M. F. Coomes, Louisville, Kentucky, said—He wished to enter a protest against the use of the chisel in opening the mastoid cells, as he thought the method a rude one, to say the least, and one that ought not to be practiced when other methods are at command. He had opened the mastoid in two cases in which pus was supposed to exist; in one the cells were healthy; in the other a small quantity of sanious fluid was found. One of the patients improved for a time, after the opening was made, was able to be on his feet for fifteen days, and finally died in convulsions. No post-mortem was permitted. The other patient died in a comatose condition a few days after trephining. The autopsy showed that the pus passed from the middle to the internal ear, thence through the bony wall into the cranial cavity. The auditory nerve in the canal was in a state of high inflammatory action.

Dr. M. Toeplitz, New York, N. Y., said—In his practice opening of the mastoid has never been made without a distinct indication, *i. e.*, tenderness, pain, swelling, localized headaches, increased temperature, etc. Chronic suppurative otitis, with only a suspicion of pus in the mastoid cells, is not considered a sufficient indication. The operation was always performed with the chisel, which has proved satisfactory in every respect. Are we justified in performing the operation in cases where the brain and the meninges have been involved? I had occasion to make two observations as to this point:—

1. A patient had suffered from chronic suppurative otitis five years previously to the last affection. When I saw him first I diagnosticated sinus thrombosis, and although I had for him no hopes, the operation was performed. The patient died four days later. The autopsy confirmed the diagnosis.

2. Three and a half months after opening the mastoid the patient showed distinct brain symptoms, intense headache and paralysis of the external rectus of the left

Digitized by Google

eye. I opened the mastoid process, which had completely closed, whereupon the symptoms subsided. Cure, after two months.

Dr. W. F. HOLCOMBE, New York, N. Y.—I am firmly convinced that the drill used by dentists is the best which can be used for opening bony parts for the exit of pus. These drills are of many sizes. Great care must be used in not pushing the drill too hard, as it cuts more rapidly than the hand drill.

Dr. PADDACK, New York, N. Y.—Found the drill run by the dental engine valuable for drilling into the mastoid cells, when the operator preferred the drill to the gouge, as it is perfectly under the control of the hand of the operator, and, with a small drill, any size of opening can be cut. The small amount of pressure required makes it useful.

Professor FROTHINGHAM, in closing the discussion of his paper, said, he would be very brief, as the subject had been already very fully debated. In answer to Professor De Rossi, he had not been in the habit of perforating the mastoid for simple chronic suppuration of the middle ear, and he advised it only when an examination and study of the case revealed a greater amount of secretion than would be ordinarily furnished by the tympanic cavity alone, with a further probability that it was poured into the tympanic cavity from the mastoid cells, and not from some other direction. This is a matter for careful study and consideration, and from the result of this study of the case we should determine whether, or not, benefit would be likely to follow the operation. He had no doubt many obstinate cases of chronic suppuration of the middle ear could thus be cured. He would further remark that the situation of the mastoid cells and antrum, with reference to their communication with the tympanic cavity, is such as to lead usually to a retention of pus in the mastoid in most cases where the cells had become extensively involved in the suppurative process. In order to drain this cavity he believes an external opening preferable, more safe than any that can be made through the wall of the meatus or tympanic cavity. The external table of the mastoid in such cases should not only be opened, but the opening should be large and extend well down toward the apex of the process; indeed, the temporal bone which he had exhibited to the Section showed a distance between the posterior wall of the external meatus and the sigmoid fossa of only four and six-tenths millimetres; it showed, also, the necessity of making the perforation so low down that the cells should not be reached above the central axis of the auditory canal. An objection had been made that surgeons dread to do the operation, and that it should be resorted to only when the indications are plain. In his paper he had advocated a more frequent resort to the operation than he had been accustomed to practice. In reviewing his past experience he did not doubt that he could have safely relieved many patients by an operation—have effected a more speedy cure than nature had done without this aid. Fortunately, he had had no case of death where operation had thus been omitted in his private practice, but several cases had come to his clinic at a stage of the disease beyond any hope, in which an early operation might have saved life.

He did not wonder that surgeons dreaded the operation with drills. While engaged in general surgery, he had performed nearly all the capital operations, and had approached no operation, not even opening the sac of an aneurism of the common carotid, with more solicitude than some of these operations upon the mastoid with a drill, which he regarded, with Schwartze, as a "drill into the darkness." Who, with such a method of operating in such an important and variable anatomical region, can feel that he is not placing his patient in great danger from the opera-

Digitized by Google

tive procedure alone. The object of treatment is to *lessen* the dangers to which the patient is exposed, and not to increase them. With only unsafe methods of operating, we may rightly withhold surgical interference, when, with available safer methods, we should urge it. After careful study of Schwartze's method, he is convinced that it is attended with so little danger and difficulty that we may urge it in many cases which formerly would have been left to the expectant plan. He believes that the cases of spontaneous cure reported could have been more speedily cured by perforation, and with less danger. The subject is too extensive for exhaustive consideration at any single conference, but that the mastoid is as liable to primary inflammation as other bony structures, seems to be a fact overlooked, or but imperfectly considered by the profession. Professor De Rossi had very truly said that pain, alone, is not generally regarded as a sufficient indication for the operation. The pain that calls for perforation of the mastoid must be studied. It is generally a deep-seated, throbbing pain, more severe at night, and resists all other efforts for its relief. The patient is unable to attend to any duty. We often find it affecting other bones, without external evidence of disease. He has seen many cases in which the tibia was affected. His esteemed colleague, the late Professor A. B. Crosby, had taught him how speedily to cure many of these cases, by boring into the bone with a common gimlet, and he opened into the mastoid in this way. An opening into the bone affords most speedy relief in inflammation of the bone. He has seen operations of the kind followed with immediate and permanent relief, though no pus was found and no appearance of sclerosis was visible to the eye. There are numerous cases in which perforation of the mastoid in search of pus has led to relief without finding pus or sclerosis. He believes pain alone, under some circumstances, constitutes a positive indication for perforation.

INHERITED SYPHILIS AS A FACTOR IN SUPPURATIVE INFLAMMATION OF THE MIDDLE EAR.

SYPHILIS HÉRÉDITAIRE EN TANT QUE FACTEUR DANS L'INFLAMMATION SUPPURATIVE DE LA MI-OREILLE.

HEREDITÄRE SYPHILIS ALS EIN FAKTOR BEI EITERIGEM CATARRH DES MITTELOHRES.

BY ROBERT TILLEY, M.D.,

Of Chicago, Illinois.

In looking over the literature of inherited syphilis, of diseases in children, and special otological literature, for some information relative to the opinion of the profession as to the part which inherited syphilis may play in the production of acute and chronic suppurative inflammation of the middle ear, I was somewhat astonished at finding so little definite reference to the subject. For the past six years it has been with me a fixed conviction, which has apparently grown from personal observation, that whenever otorrhœa was associated with such other symptoms as are usually associated with inherited syphilis, although no one of these other symptoms may have been very pronounced, to regard the otorrhœa and its direct cause, inflammation of the aural tissues, as a manifestation of inherited syphilis. It is this settled personal conviction that

Digitized by Google

accounts for my surprise in finding so little reference to the subject in the collected literature referred to above.

Professor Adam Politzer, as translated by Dr. J. P. Cassels, p. 387, in speaking of the etiology of acute purulent inflammation of the middle ear, says: "Acute purulent inflammation of the middle ear either takes place primarily, in consequence of external influences, after colds, or is caused by propagation of acute, or chronic naso-pharyngeal catarrh to the middle ear. It may also be developed in the course of scarlatina, measles, smallpox, typhus, tuberculosis, diphtheritis, pneumonia, influenza, whooping-cough, and in the puerperal state. Traumatic acute suppuration in the middle ear is sometimes caused by paracentesis, or by other operations in the membrana tympani, also by contusions of the cranium, or of the ear by a blow or a fall, by forcible attempts at extraction of foreign bodies from the ear, and by scalding and cauterization of the ear. Acute suppuration of the middle ear may also be caused by Weber's nasal douche, by injections of cold water into the external meatus, and by cold river, or sea baths." He further states that it occurs more frequently in children than in adults, and that climate seems to have but little influence on its development, citing Knapp as having carefully compared statistics from European and American sources to substantiate the statement. Neither in the section on the acute form of suppurative inflammation of the middle ear, nor in the section on the chronic form of this affection, is there any allusion whatever to inherited syphilis as a possible cause of the disease.

Dr. Albert H. Buck, in his work, 1880, "Diagnosis and Treatment of Ear Diseases," does not mention inherited syphilis. He details in the main the same list of causes as Politzer. The chronic affection, he concludes, is due to neglected acute affections, practically, always. I may, however, be permitted to refer to a shorts eparate section by Dr. Buck on syphilis in the ear. He says, page 227, "Writers on diseases of the ear are, I believe, nearly unanimous in the statement that lesions characteristic of syphilis are never observed in the middle ear or membrana tympani. While I," he continues, "do not feel justified as yet in disputing this statement with any degree of positiveness, owing to the scantiness of the material which has come under my observation, I cannot help feeling that we shall learn in time to recognize in these parts textural lesions as distinctly characteristic of syphilis as are most of its external manifestations." He then assigns two cases in order to show that the lesions developed in the middle ear and membrana tympani were in all probability due to syphilis. I cite this quotation from Dr. Buck because it may be said that it would be necessary to establish, in the first place, that primary syphilis affects the middle ear before we can with prudence suspect the inherited manifestations to show any proclivity toward that region. But I think we shall see later on that such a position can scarcely be sustained.

I will make another reference to primary syphilis in the middle ear. Keyes, Wood's Library ed., p. 235, says: "There is an inflammatory condition of the middle ear due to syphilis which is not a catarrh. No suppuration occurs, but a thickening of the drumhead and of the tissues of the middle ear leading to a restraint in the movements of the ossicula. It is a sort of plastic myringitis. Schwartze and Roosa," he says, "believe that this condition originates in a periostitis of the middle ear."

The strong assertion quoted above, from Dr. Buck, may justify still another quotation. Dr. Alfred Cooper, of Lock Hospital, London, says: "The tympanum, or middle ear, is the part of the organ most frequently affected, owing to the extension of syphilitic lesions from the throat and nose. The membrana tympani and the mucous membrane lining the cavity become infiltrated and thickened, the ossicles are more or less changed, and eventually the membrane may become perforated by ulceration, the result of inflammation of the tympanic cavity. When this is the case, all the structures are more or less disintegrated and purulent discharges escape from the ear."

Digitized by Google

Dr. St. John Roosa does not mention inherited syphilis as a cause of suppurative inflammation of the middle ear, but refers to its occurrence in primary syphilis in the affections of the pharynx and posterior nares.

Dr. Thomas Barr, of Glasgow, after the general enumeration of the accepted associated affections, says: "As in nearly all the diseases of the ear, hereditary tendency plays an important part in the causation." There is no specification, however, as to the character of the "hereditary tendency."

Although confessedly a disease, par excellence, of children, Eustace Smith, in his "Disease in Children," does not mention inherited syphilis as a cause. He seems to take his etiology from the writers on otology.

Dr. P. Diday, as edited by Dr. F. B. Sturgis, p. 96, after stating that affections of the ear found to be dependent upon inherited syphilis are usually nervous diseases, admits that "Cases of suppurative otitis media are also seen associated with this type of disease, but it is open to some doubt if it is dependent directly on syphilis." Sturgis himself, in the appendix to his edition of Diday, says, p. 291, "The lesions of the ear" (he is speaking, of course, of inherited syphilis) "are even yet but little understood, and are nearly all confined to deafness which comes on suddenly and is frequently unaffected by treatment." Sir W. B. Dalby, as quoted by Sturgis, seems decidedly opposed to the supposition of inherited syphilis playing any part in middle-ear affections. He is quoted as saying: "It is of the utmost importance that this affection," inherited syphilis affecting the ear, "should be clearly recognized as having no connection whatever with changes that may be found in the tympanum." Space forbids my entering further into his discussion of the subject. In Ziemssen's Cyclopædia, however, we find a more definite positive statement of inherited syphilis as a cause of suppurative aural affections. In Vol. III, page 225, English edition, we read, "In the account of the syphilitic affections of the pharynx it was observed that it was not uncommon for them to cause a temporary or permanent occlusion of the orifices of the Eustachian tubes. This is the starting point for *catarrh of the tympanum* and a frequent cause of deafness and tinnitus aurium, which are apt to occur in the course of syphilis. Such a catarrh may have the same issues as those due to other causes, and lead to perforation of the membrana tympani, purulent infiltration of the cells of the mastoid process, etc. These conditions are especially apt to occur in consequence of inherited syphilis." On page 239, we read, "Deafness due to changes in the middle or the internal ear may also be present at this time." The time here referred to is the development of iritis from inherited syphilis.

It is fair to conclude from the above quotations that, while the writers on otology have not hitherto regarded inherited syphilis as a cause of suppurative affections of the middle ear, some of the writers on syphilis are quite positive in the expression of their convictions that inherited syphilis constitutes a definite cause of suppurative inflammation of the middle ear.

To further elucidate the question, we turn our attention for a moment to the cornea. There is scarcely a dissenting voice in the profession relative to the significance of certain pathological changes of the corneal tissue. It is unnecessary to take up your time by citing authors to show that interstitial keratitis constitutes one of the most convincing manifestations of inherited syphilis. It is claimed, of course, by some that keratitis interstitialis does sometimes exist in the offspring without any evidence of syphilis being demonstrated in the parents. Without fear of encountering serious opposition, however, the general voice of the profession may be thus stated: that interstitial keratitis in the vast majority of cases is a manifestation of inherited syphilis. So settled is this conviction, that keratitis interstitialis is sometimes called syphilitic keratis—meaning, of course, inherited syphilis.

I have referred to the cornea on account of a certain resemblance which the cornea

Digitized by Google

may be supposed to bear to the membrana tympani. Their development in the embryonic state is not dissimilar. They are both developed from the ectodermic membrane, with a contribution in both cases from the mesodermic tissue. Their period of development cannot be greatly different. Both present peculiarities of differentiation for their special functions. Their respective areas are not greatly different in point of measurement; and they are both protected in a peculiar manner by the surrounding parts of the respective organs of which they are members. With these points of resemblance, and the recognized fact that inherited syphilis plays a very important part in the development of the nervous affections of the ear, in the development of mastoiditis in eczema of the auricle, may it not be logically urged that we are not only justified in asking the recognition of its influence, but that we are even illogical in ignoring its bearing? With the evidence that exists in support of the part which syphilis plays in producing pathological conditions of other parts of the auditory organ and of the various corresponding tissues of the eye, why should we expect exemption for the middle ear?

But we must not confine our attention wholly to the membrana tympani. The mucous membrane lining the middle ear is a continuation of the same membrane in the nose. Now the evidence that keratitis interstitialis is a manifestation of syphilis is not more firmly rooted in the mind of the profession than is the conviction that an early manifestation of an abnormal condition of the mucous membrane of the nose is another form of its manifestation.

Trousseau, "Clinical Med.," page 584, Vol. II, English edition, speaking of inherited syphilis, says: "Coryza is one of the signs which appear earliest, and, also, one of those which have been the best studied. The infant breathes with increasing difficulty. . . . . Soon there is a running from the nose and a few drops of blood exude, but there is no true epistaxis. The secretions become more and more sanguinolent without being profuse; they irritate the alæ of the nose, the upper lip, etc." Now, if it is conceded that syphilis works such changes in the nose, is it not reasonable to suppose that under some conditions that same affection may continue through the Eustachian tube to the middle ear, and, under modified circumstances, may even start up independently in the middle ear itself?

In the quotation above, Trousseau was speaking of the very early manifestations of inherited syphilis. Sturgis, in his appendix to Diday's work, says: "The mucous membranes, notably of the throat, nasal cavities, and mouth, are the seat of manifestations of late hereditary syphilis." On page 293, he says: "Ozæna from ulceration of the bones of the nose is not an unfrequent lesion in the hereditary forms of syphilis." Further on he continues, "The viscera also present lesions which are to be attributed to this cause." I might refer to other authors on this subject, but I forbear, because I do not think there can exist a dissenting voice in this Section as to the possibility of inherited syphilis affecting practically any part of the human organism, and, of course, with it the middle ear and membrana tympani. It does not, however, seem to have been duly recognized in the standard otological literature.

There is a tendency on the part of some contributors to current medical literature to expatiate at length on the condition of the nasal mucous membranes and their subjacent tissues, as an original or prime cause of special pathological conditions both of the eyes and ears. My conviction is, that in a large majority of such alleged cases, the true explanation is to be found one step further removed, namely; that the difficulties of eyes, and ears, and nose are referable to a vitiated constitution from some cause; and further, that among such causes inherited syphilis stands prominent.

There is an interesting study, hitherto little prosecuted, which may be capable of throwing valuable light on the question; namely, the study of the relative extent of affections of the middle ear among the negroes. I have examined such literature as is at my disposal, in vain, for information on this point. I am not conscious that any rela-

Digitized by Google

tive statistical information has been published. Moreover, I have not been able to find any statistics as to the comparative susceptibility of the genuine negro to syphilis; but from general medical literature, I have the conviction that the ravages of syphilis among the negroes are not nearly so disastrous as among the white races. If such be the case, statistics relative to the middle-ear affections in the negro should contribute evidence either for, or against, the argument here advanced.

I urge this question more emphatically on account of its practical bearing. You are all acquainted with the various "methods" advocated in treatment of suppurative inflammation of the middle ear, antiseptic method, dry method, etc. A few years ago, some were claiming to cure all cases of otorrhœa by the insufflation of powdered alum. Little was said of the cases of death from meningitis which followed the drying of the alum. That "method" subsided, and boric-acid insufflation and packing had its day; the bichloride solution and iodoform are still exerting their influence. All of these remedies, and still others, are excellent when judiciously used, but it is only when we form a definite and fundamental idea of the causes of the real difficulty that we can act with confidence.

It is well known that almost all the manifestations of syphilis of a functional character yield under judicious use of anti-syphilitic remedies. It is further known that pathological changes of very marked character in the tissues themselves begin at once to assume a more normal aspect under similar treatment. If such good results can be obtained in syphilides on the leg, or arm, or scalp, correspondingly satisfactory results should be secured when the same affection invades the middle ear and membrana tympani.

I may be reminded here of hundreds of cases cured with boric acid, and nitrate of silver, and the question may be justly asked, if any large percentage of these cases were the manifestation of inherited syphilis, would they have yielded so readily to such remedies? To which it may be replied, is it not a fact that such cases are very frequently subject to relapses, and that some of them defy the most painstaking local treatment, and respond only when appropriate constitutional measures are associated with the local treatment? It may further be added that nitrate of silver is of recognized value in mucous patches, and boric acid is of value as a drying agent in certain syphilides.

Although Politzer, in his section on etiology, does not refer to inherited syphilis, yet, in his chapter on treatment, he says, p. 487: "Where there is constitutional syphilis the suppuration is often arrested only by a suitable anti-syphilitic treatment," and in his section on prognosis, p. 398, he says: "The prognosis is, however, unfavorable in scrofulous, tubercular and syphilitic individuals."

The difficulties associated with the demonstration of a given lesion of the middle ear or membrana tympani being syphilitic, are unusually complicated. I do not pretend to give demonstration. The demonstration of inherited syphilis, as a whole, even, would certainly afford an extensive subject for dispute. And the point which is interesting to us as otologists, the demonstration of its existence under the circumstances associated with which we see many patients, but adds to the difficulty of making a positive demonstration. Let the complicated difficulties associated with the parents be passed over in silence. Suppose, for a moment, that the membrana tympani were the seat of textural changes, similar in character to the textural change existing in interstitial keratitis, how often would it be possible for the most painstaking investigator to obtain such a view of the membrane as to give him the necessary conviction of that peculiar pathological condition? How is it possible to get a view of the mucous membrane of the middle ear to compare it with the condition of the mucous membrane in the nose, which is said by Trousseau to be characteristic? It not infrequently happens, however, that the discharges from the ear correspond, in children, exactly with the discharge described by Trousseau as coming from the nose. He says, p. 584: "Soon

Digitized by Google

there is a running from the nose (substitute ears). The secretion becomes more and more sanguinolent without being profuse; it irritates the alæ of the nose, and the upper lip (substitute the external canal and furrows of the auricle), causing ulcerations which become covered with crusts when dried by the external air. On making a more attentive examination, there will often be found at the angles of the alæ of the nose (the furrows of the external ear) small, ulcerated fissures, already characteristic, inasmuch as they exactly reproduce the special aspect of the fissures seen in the commissures of the lip."

It is not claimed, herein, that every case of suppurative inflammation of the middle ear presents the above characteristics, nor is it desired to imply that every case of middle-ear suppuration is associated with inherited syphilis, but the claim is made that the above characteristics, in acute suppurative inflammation of the middle ear, are not infrequent. I have said that I do not pretend to demonstrate this question. I do not consider that interstitial keratitis, nor the peculiar pathological condition of the nose, nor its corresponding condition in the ear, are demonstrated features of inherited syphilis. The demonstration will be possible only when the entity causing the clinical features which constitute syphilis has been isolated, and its peculiar influence in the various tissues has been demonstrated. I regard keratitis interstitialis, however, as well as the other peculiarities referred to above, as clinical manifestations, which have been so often observed and recorded by our worthy predecessors, and encountered by ourselves, that, in the absence of a more absolute method of demonstration, we are bound and glad to accept them as a guide in our efforts as healers of the sick.

My general plan in determining the presumptive cause of a case of suppurative inflammation of the middle ear is, after thorough cleansing of the affected organ, to inspect the nasal and pharyngeal spaces; to examine the condition of the teeth; to observe well the scalp; to explore the region of the cervical glands; to take a general survey of the features; to exercise judgment relative to inquiry about other members of the family, and then if numerous deaths or miscarriages have occurred in the family; if in the patient the cervical glands are enlarged; if the features are puny and shriveled, the hair thin and dry, pustules in the scalp, hypersecretion with swollen tissues in the nose, teeth irregular and decayed, especially the characteristic teeth of Hutchinson; any special disposition to disturbance of the alimentary canal, any abnormal condition about the eyes; if there be found a limited number of these conditions, together with a suppurative condition of the middle ear, to give the patient the benefit of the doubt, and if it is not concluded that he is syphilitic, it is inferred that he will thrive best, that his suppurative middle ear will heal more effectually and quicker, and the improvement be more lasting, by the use of what are called anti-syphilitic remedies.

Cases might be added to illustrate the subject, but unless they were detailed with a minuteness which would occupy too much of your time, they would be of no practical value. The personal equation of the reporter enters so largely into the report of many cases, that it is difficult to estimate the value of such reports without personal knowledge of the reporter. Moreover, if anything that has been said shall lead to belief that suppurative inflammation of the middle ear is probably more often associated with inherited syphilis than has hitherto been supposed, the associated treatment will follow independently of recorded cases.

Digitized by Google

CHANGES IN THE INTERNAL EAR IN HEREDITARY SYPHILIS.

CHANGEMENTS DANS L'OREILLE INTERNE SURVENUS À CAUSE DE LA SYPHILIS HÉRÉDITAIRE.

VERÄNDERUNGEN IM INNEREN OHRE BEI HEREDITÄRER SYPHILIS.

BY J. BARATOUX, M.D.,

Of Paris, France.

During the last few years much attention has been paid to the changes developed in the internal ear in syphilis.

It has thus been recognized that the walls of the vestibule, the semicircular canals, the lamina spiralis and the cochlea may be affected with periostitis; that the soft parts of the labyrinths become implicated with round cells; that the auditory nerve has been greatly modified in structure, and, finally, that the membranes of the internal ear have been injected and bathed in a sero-sanguinolent fluid which had taken the place of the lymph. I have myself, in a work on "Syphilis of the Ear," called attention to the inflammation of the vascular loops of the external walls of the canal of Corti on a level with the spiral angle, and of those which run along the basilar membrane. I have also demonstrated on several preparations a dilatation, and even a rupture of the walls of the vessels.

But among the new-born, afflicted with hereditary syphilis, the changes in the internal ear have been but little studied. As a fact, Hutchinson attributes the associated deafness in such cases to a lesion of nervous origin. Lancereaux, and St. John Roosa hold the same opinion. Wreden refers the lesion to a gummous degeneration of the auditory nerve. Hinton, however, demonstrated congestion of the vestibule.

In a series of autopsies, partly made at the Hospice des Enfants-assistés, and partly in the Obstetric Department of the Hôpital Cochin and St. Louis, in Paris, I have found, on different occasions, that the internal ear was the seat of important primitive lesions, or arising secondarily from lesions of the middle ear.

Among the numerous autopsies which I have made, I present only the report of 43 cases. All these 43 cases were affected with hereditary syphilis, demonstrated either by lesions on different parts of the body, or by lesions on the parents, which lesions had manifested themselves in the year previous to the birth of the child.

Of the 43 subjects, 19 were still-born, and 24 lived a period varying from several hours to four years.

Of the 19 still-born, 17 were from the Obstetric Department of St. Louis and two from the Cochin hospital. Eight presented lesions of the tympanum, three of the labyrinth, and eight of both the middle ear and the internal ear.

Of the 24 autopsies of those not still-born, 13 came from the Hospital St. Louis, three from Cochin, seven from the Service of Parrot and one from that of M. L. P. Fournier.

In 19 cases there were lesions of the middle ear; in one, alterations of the internal ear alone; and in four, lesions of the middle ear and labyrinth.

Thus, there was demonstrated 27 times, lesions of the middle ear; four times, lesions of the labyrinth; and 12 times, lesions of both parts.

I confine myself here to lesions of the internal ear.

A certain number of the changes of this part of the auditory apparatus arise from the tissue of the tympanum, by propagation to the labyrinth. It is thus that inflammation of the middle ear may reach the internal ear by destroying the windows, or even in traversing the bony wall. In cases where pus does not exist in the labyrinth, cases which we now leave out of the question, I have frequently demonstrated a vascular

Digitized by Google

injection of the soft parts of the labyrinths, with cellular infiltration of the principal membranes on a level with the cochlea, and of the ampullæ of the semicircular canals.

The walls of the ampullæ, and of the cochlea have a reddish aspect. The axis of the cochlea is also injected and infiltrated with round cells, the parts are bathed in a sero-sanguinolent liquid which has taken the place of the normal liquid of the canals of the internal ear. Such are the lesions which I have observed in the internal ear, when the internal ear was affected simultaneously with the middle ear, with an absence of pus in the internal ear.

But when the internal ear is affected alone, we find more important changes. It is thus that the vessels developed in the spiral angle, and even the vessel which follows the membranous lamina spiralis, exhibited on several occasions a cellular proliferation of their tunics, developing in this way a diminution of the caliber of the vessels, and later complete obliteration by fibrinous clots, developing, as I have also seen in the adult, an aneurismal dilation, and even more frequently a rupture of the walls and hemorrhage.

There was one case of a fœtus, the specimens of which I showed to Dr. Balzer, then at the head of the laboratory of Professor Fournier. The labyrinth presented marked congestion, with hemorrhagic points on the membranous labyrinth of the turns of the cochlea. Such was also the case in a second fœtus of the same age. I was able to demonstrate to Dr. Merklen, on service at the hospital, a blood clot filling up the tympanic scale and the canal of Corti throughout the whole length of the first turn of the left cochlea.

In a third petrous bone, taken from a full-term child, I found a hemorrhage embracing the last turn of the canal of Corti, where it was easy to demonstrate the rupture of a spiral vessel. In consequence of this effusion of blood in the canal of Corti, the canal was rendered plainly visible. In a transverse section it was visible as a round form, with bulgings in the scale of the vestibule. The ordinary inflammatory changes were easily observed in the vessels.

Finally, in the last autopsy of a child who lived several hours, the labyrinth showed, at the level of the ventricle, certain hemorrhagic points, with rupture of the spiral vessels at the level of the first turn of the cochlea, and of a vascular loop at the external spiral angle, near the insertion of the membrane of Reissner. The lungs, the only organ which I was able to examine in conjunction with the ear, were also the seat of hemorrhagic points.

The internal ear may then be the seat of hemorrhage in hereditary syphilis.

In fact, it is acknowledged at the present day that syphilis may develop in the organism of the infant a lesion of the vessels, especially the capillaries, and the small and large veins. The walls of these vessels become thickened, and the lumen of the vessel is diminished by the cellular inflammation of the tunics. This, in its turn, develops a local cessation of the circulation, which forces the blood into the neighboring parts, as has been demonstrated by the works of Epotein, Behren, Petersen, Zischl and others.

My observations corroborate the views of those authors, who have demonstrated in the new-born syphilitic child hemorrhage in the cellular tissue, in the lungs, in the pleural cavity, in the heart and pericardium, in the brain and its membranes, in the liver, in the stomach, in the pancreas, in the thymus gland, in the testicle and cords, in the bone and periosteum, in the retro-peritoneal cavity and the omentum, in the tunics of the large vessels, and in the buccal mucous membrane; which facts prove that all these lesions belong to the same cause.

Digitized by Google

DISCUSSION.

Professor G. E. FROTHINGHAM, of Ann Arbor, Michigan, thinks the papers of Drs. Tilley and Baratoux are valuable productions, since they deal with a subject hardly, if at all, discussed in the works on aural affections. To cite his own custom, for example, inquiry as to hereditary syphilis in ear diseases did not receive the attention it deserves from otologists. While acquired syphilis is duly considered, the pains of investigating the possible influence of hereditary syphilis in the etiology and progress of certain aural affections, are seldom taken. Of course, when other well-marked symptoms of inherited taint are present, they are generally weighed, and the case is treated on the general principle that any constitutional disease should be cured, if possible, as a means of combating any local expression. It is not his custom, however, to attempt any special inquiry in this direction, and he does not think many others do. He readily conceives that inherited taint might give no other manifestation than, perhaps, general debility, which might be attributed to other causes. Happily, the custom of giving tonics, and alteratives as well, in the treatment of aural affections, compensates somewhat for this neglect, since cod-liver oil and the iodides, administered for other reasons, answers the best purpose in such inherited disease, when conjoined with proper outdoor exercise, bathing, cutaneous friction and nutritious food, elements which enter into the therapeutics of most such cases. Dr. Baratoux is entitled to the thanks of the profession for his careful researches in this field, and Dr. Tilley as well, for calling attention to the subject. He thinks it will be wise to give heed to the facts they have presented, and to remember them in examinations and the treatment of ear affections.

CEREBRO-SPINAL FEVER AS A CAUSE OF DEAFNESS.

FIÈVRE CÉRÉBRO-SPINALE COMME UNE CAUSE DE SURDITÉ.

MENINGITIS CEREBRO-SPINALIS ALS URSACHE DER TAUBHEIT.

BY C. M. HOBBY, M.D.,
Of Iowa City, Iowa.

The sources of information used in this paper are, first, personal experience in general practice, with a small number of cases of cerebro-spinal fever; second, the record of cases, showing the after consequences of cerebro-spinal fever, seen during twelve years of special practice; third, observations made at the "Iowa College for the Blind," and at the "Iowa Institution for the Deaf and Dumb," the first averaging about one hundred and fifty, the last about two hundred and seventy-five inmates; fourth, the aggregation of statistics of various institutions for the deaf and dumb, and the United States Census for 1880; fifth, correspondence with those having opportunity for observation, literature as specified, etc.

Iowa being a comparatively young State, settled with the most vigorous immigrants from the older States and from Europe, having an equally distributed population of about 1,800,000, containing no large cities on the one hand, nor waste land on the other, combining the least illiteracy and the greatest diffusion of education, offers a good average field for investigation; the papers preliminary to the admission of deaf

Digitized by Google

and dumb pupils presenting a greater average accuracy of detail, while local or circumscribed influences productive of deafness are reduced to a minimum. I have therefore used the statistics drawn from Iowa as a standard with which to compare general statistics, but in other respects have given no extra weight to the results deduced from Iowa. In fact, with few exceptions, the Iowa statistics give the same results as the general average.

Cerebro-spinal fever occurs in all parts of the United States, occasionally in an epidemic form, frequently as an endemic, and sporadic cases are of common occurrence. For example, the very incomplete reports of the State Board of Health of Iowa (for 1882) give 73 cases of cerebro-spinal fever, based on 76 reports out of a possible 2000 or more. Yet these 73 cases were scattered among 27 out of 99 counties. That there were many hundred cases of cerebro-spinal fever each year would appear from the fact that from 20 to 60 cases of total deafness result from this disease each year.

Any estimate, based upon the accessible vital statistics, of the total number of cases of cerebro-spinal fever occurring in the United States each year, must be taken with a great deal of allowance. I think 15,000 may be assumed as a minimum, probably far under the actual average. In epidemics the mortality has been estimated at fifty per cent., and at least fifteen per cent. of the survivors pass into the defective classes, total deafness being the most common sequel; total blindness, deafness of one ear, blindness of one eye, insanity, dementia, epilepsy and paralysis occur in a sufficiently large proportion of cases to make this disease the most baneful to survivors of the whole list of infectious diseases. So far as deaf-mutism is concerned, it will be seen from the annexed statistics that it outranks all other causes, producing, at the lowest estimate, fifteen per cent. of the cases, and probably, with due weight given to those cases reported as congenital, or unknown, to thirty per cent. of the cases.

This disease, while occurring at all ages, is especially prevalent during infancy; thus, out of 975 cases occurring in New York City in 1872 (see Smith, Diseases of Children, fifth edition, p. 334), nearly thirteen per cent. were under one year of age, and forty-seven per cent. were under two years of age; in 83 cases of total deafness resulting from cerebro-spinal fever, which I have personally examined, fourteen per cent. were under one year of age, and forty-seven per cent. were under two years of age at the time when attacked by the disease. The census for 1880 shows that the proportion of deaf and dumb to the entire population is 66 to 100,000, 36 being males and 30 females; the distribution being very nearly uniform in all parts of the United States, the Eastern and Northern States exceeding the average slightly, and the very new States falling below—this would be expected. Race exerts an appreciable influence, the colored race having but 50 deaf-mutes to the 100,000. So far as I can conclude, cerebro-spinal fever is likewise uniformly distributed, or at least its effects in the production of deafness vary but little in the different States; not so, however, with scarlet fever, which apparently causes thirty per cent. of deafness in Rhode Island, twenty-two per cent. in Massachusetts, only seven per cent. in Illinois, and vanishes in Arkansas. It would appear that scarlet fever is not only more prevalent, but also more destructive in its effects, in the more densely populated parts of the country, especially in or near the large cities.

The value of statistics is measured by the accuracy with which they are collected; and judged by this standard, the reports of the superintendents of schools for the deaf and dumb are not as valuable as they should be, for, from the necessities of the case, the alleged cause of deafness is either directly obtained from the parent, or from a physician who obtains from the parent the supposed cause. It rarely happens that the report as to cause is from a physician who has full knowledge of the history of the child. In 8321 cases I have found "Fright" and "Teething" credited with six cases

Digitized by Google

each, and Cholera Infantum with ten, while nearly ten per cent. of the cases (825) are from unknown or not designated causes. Again, the lack of a well-understood and rational nosology materially obscures the tables published and renders careful study of each one necessary to its use in the aggregate; for example, no one would hesitate to place together under the head of Cerebro-spinal Fever (which name is not used in any report that I have seen) both "Cerebro-spinal Meningitis" and "Spotted Fever." Difference of opinion might easily arise in regard to such terms as "Meningitis," "Brain Fever," "Spinal Meningitis," "Spinal Disease," "Water on Brain," "Fits," "Convulsions," "Paralysis." Yet "Brain Fever," "Meningitis," "Spinal Meningitis" and "Spinal Disease" are used in many of the reports as synonymous with Cerebro-spinal Fever, and "Fits," "Convulsions," and "Paralysis," in some instances, at least, describe symptoms or sequelæ of Cerebro-spinal Fever.

Of the reports of thirteen of the leading institutions for deaf-mutes in this country, I find that in seven Meningitis is not mentioned as a cause, in five Spotted Fever is not mentioned, in six Cerebro-spinal Meningitis, and in one Brain Fever. Cerebro-spinal Meningitis and Spotted Fever occur separately in three, Brain Fever and Cerebro-spinal Meningitis occur separately in four, Meningitis and Cerebro-spinal Meningitis are separated in but one, indicating that these names are largely used synonymously. Nor are the reports of vital statistics from the State Boards of Health entirely free from the same kind of confusion.

It has been suggested, and with great plausibility, that Cerebro-spinal Fever, so called, is an infectious fever, accompanied by local manifestations, no one of which is invariably present; and that these local manifestations are most frequently found in the membranes of the brain and cord, together or separately, or in choroiditis of one or both eyes, or inflammation of the labyrinth of one or both ears. In this paper it is impossible to follow up the pathological considerations which intensify the belief that Cerebro-spinal Fever is of much more common occurrence than is indicated by present literature and statistics, and that its causative agent is correspondingly a more important factor in the production of deafness. Recovery from non-infectious, simple meningitis, whether idiopathic, traumatic, in connection with other febrile disturbance, or tubercular, is sufficiently rare to furnish strong ground for the belief that some other disease must have existed in the majority of forty-two cases of total deafness attributed to this cause, in a total of 338 cases reported from Minnesota, especially as the report from Illinois, with 1886 deaf-mutes, fails to mention simple Meningitis as a cause.

The value of these reports is lessened also by the personal bias of the superintendents in many instances. The questions sent forth to be answered prior to admission, by their leading character, give an undue weight to certain apparent conclusions. The attempt to establish the influence of consanguineous marriages, in the production of mute offspring, has been the most prominent factor in devising questions to show cause for deafness; for example, the questions are directly asked by the majority of institutions, "Was the applicant born deaf?" and, "What, if any, was the relationship of the parents before marriage?" As a result of the first question we have reported from the leading institution for the deaf and dumb of America forty-six per cent. of congenital mutes, and an average out of 8321 cases of 33.6 per cent. With a little care in sifting reports, I found out of 1111 cases in Iowa only 13.3 per cent. of congenital mutes, and I believe that a majority of those who have investigated the subject practically, will agree that even this percentage is too large. An illustration of the results of one-sided examination of statistics in this respect is shown by the report of the Illinois institution, one of the most thorough that I have had access to; it shows that out of 1886 cases, 110, nearly six per cent., were children of consanguineous marriages, ranging between incestuous connection and intermarriage of fourth cousins and grandchildren of first

Digitized by Google

cousins. Two families had four deaf children each. Careful examination shows that only 49 out of 110 were born deaf, and the deafness of the others was attributed to spotted fever, measles, cholera, cold, etc., causes showing no hereditary influence.

The fact that, for teaching purposes, it has been customary in many institutions to class those who have never learned to articulate at all as "born deaf," or "congenital," also gives an undue prominence in older reports to the congenital numeration, as does also the fact that when at four or five months of age the parents discover a child to be deaf, they are prone to believe it was born so; however, I believe, for reasons to be given hereafter, that nasal disease, and, consequently, middle-ear disease of babes, has but little to do in the production of mutism, and that the great majority of cases of infantile deafness proceed from intra-cranial causes. Indeed, the appended tables show that only twenty-eight per cent. of deaf-mutes give a history indicating that their deafness flowed out of middle-ear disease. Personal inquiry has shown that four cases reported from the Iowa school as congenital were, in fact, the result of cerebro-spinal fever. Two cases reported as caused by scarlet fever were also found to have had their origin in cerebro-spinal fever, but it so seldom happens that these cases can be satisfactorily traced back, that I have only attempted it in a few instances when I was personally acquainted with the attending physician.

The diagnosis of cerebro-spinal fever is difficult in the early part of life, when its occurrence is most common, and the most characteristic symptoms are, in malarial districts, frequently simulated by remittent fever, so that, unless epidemic, many physicians consider cases which recover as malarial. This was, perhaps, the case in the thirty instances where deafness was attributed to quinine, and I am certain that cerebro-spinal fever was the cause of deafness in one case personally examined, which was attributed to quinine.

In the appended tables of causation of deafness compiled from the reports of different institutions, I have condensed the list of causes as much as possible without changing anything that could be considered doubtful. For example, I have joined together "Diseases in the Head," "Sores in the Head," "Running from the Head," "Rising in the Head," "Discharge from the Ears," "Running from the Ears," etc., the meaning being perfectly clear.

The tables speak for themselves, showing that from central causes specified about eighteen per cent. of the cases of deafness proceeded, that if there be added to these those cases which are attributed to "fever," without specifying the kind, and those cases which are classed as of unknown origin, the percentage rises to about thirty per cent.

If there be included in one group those cases which are (1) of a distinctly central origin, (2) those from unspecified fevers and unknown causes, and (3) all but ten per cent. of the alleged congenital, we shall have a series aggregating fifty-three per cent., in which the probability is very strong that the deafness is the result of intra-cranial disease, or, at least, of labyrinthine disease having its origin in early, and, in many cases, obscure, febrile conditions. I have not yet made a sufficient number of examinations to compare these series of cases, one with another, as to the amount and quality of hearing, the presence or absence of lesions of the middle ear or membrana tympani, and the ages at which deafness occurred; so far as my examinations have gone, they show a very striking similarity in the result of tests for bone conduction and the appearance upon inspection of the external auditory canal, and strong contrast with the appearances observed in those cases which were attributed to scarlet fever. It will be noticed that the non-eruptive fevers are credited with only about four per cent. of the cases, that the eruptive fevers, excluding scarlet fever, were the cause of but 2.5 per cent. of cases, while including scarlet fever brings the ratio up to 20.8 per cent.

The results obtained by personal examination differ so widely from those obtained

Digitized by Google

by Dr. Roosa ("Diseases of the Ear," Sixth Edition, p. 638), both in respect to appearances observed and results of tests for bone conduction, that I cannot explain the reasons for the discrepancy. For example, in twenty-seven cases of deafness from cerebro-spinal meningitis, fifty-four ears, he found pathological conditions of the membrane in every one that was well seen; in my examinations, using reflected natural light, I found the membrane normal and the light spot in the proper position in 86 out of 166 ears, or in over fifty per cent. of cases, and pathological conditions present and seen in 61 ears, or about thirty per cent. He found bone conduction present in 39 out of 54 ears, or seventy-two per cent.; I found bone conduction present in only 40 out of 166 ears, or twenty-four per cent.

Both the aerial conduction and the bone conduction were tested with the tuning fork (C^2), the tests were in each instance repeated several times, and where there remained any doubt as to the mute's ability to distinguish between feeling and hearing, the credit was given to the hearing. The tuning fork was applied to the mastoid, to the vertex, to the wrist and hand and to the teeth. The vibration of the tuning fork applied to the hand produces pleasurable sensations to most mutes, but the sensation is not that of hearing.

The result of the examinations would indicate that while a majority of those deaf from cerebro-spinal fever have had no severe form of middle-ear disease, yet that suppurative inflammations of the middle ear frequently are suggested with the graver conditions that produce a total failure of appreciation of sound. Careful examination fails to show any relationship between the ability to appreciate sound in either of the ways and the presence or absence of pathology of the middle ear.

So far as pathological considerations are concerned, I have made no observations upon the morbid anatomy of deaf-mutes, and can consider pathological questions only from clinical points of view.

Clinical consideration of the cases observed suggests at once that the lesions producing deafness must involve the perceptive apparatus in the great majority of cases; sudden, complete, and symmetrical deafness cannot be attributed to middle-ear disease. I have, however, seen two cases in which severe deafness, apparently from middle-ear causes, followed Cerebro-spinal Fever, in both of whom the hearing became materially improved subsequently.

In a disease characterized in some epidemics by tegumentary eruptions, not infrequently accompanied by pharyngitis, middle-ear disease is likely to occur in a considerable proportion of cases. Is it not probable, then, that in those mutes (from cerebro-spinal fever) who show evidence of lesions of the membrane, or middle ear, the disease affecting the middle ear was coincident with, but not developed from, the disease causing deafness? The rapidity with which deafness is produced, the comparative immunity of the facial nerve from simultaneous lesion, the analogy presented by lesions of the eye in cerebro-spinal fever, point to the labyrinth as the seat of the pathological processes destroying the hearing. I have notes of sixteen cases in which blindness resulted from Cerebro-spinal Fever, affecting twenty-eight eyes. In eight cases both eyes were lost from suppurative choroiditis, in six cases one eye of each was destroyed in the same manner, and in two patients both eyes were blind, showing white atrophy of the disk; in both of these cases the blindness developed some time ("nearly a year") after the sickness; in one of these, a female of eighteen, the circumference of the head was twenty-four and one-half inches. In two of the above cases of blindness of one eye, there was coincident deafness of both ears. I have seen, but failed to preserve notes, one case in which blindness of both eyes and deafness of both ears resulted from Cerebro-spinal Fever.

From the foregoing considerations, and from the comparison of the annexed tables

Digitized by Google

with each other, and their interpretation in the light of general observation and experience, I think the following conclusions are warranted :—

1. Cerebro-spinal fever is prevalent throughout the United States, and is a constant factor in the production of deafness.

2. That, upon the surface, it appears equal in importance, as a cause for total deafness, with scarlet fever.

3. That when proper weight is given to the reports of causes of deaf-mutism, cerebro-spinal fever is the disease, above all others, producing total deafness, which the non-professional mind would class as "Fever," "Congenital," or "Unknown."

4. That from reasons, some of which are specified above, and others of which are apparent to any physician who has studied the defective classes, it is probable that not more than ten per cent. of the cases of deaf-mutism are actually congenital.

5. That with this assumption there appears upon the face of the reports thirty-five per cent. of cases of total deafness, for which no rational explanation is offered.

6. That the greater part of this thirty-five per cent. result from intra-cranial causes, including under intra-cranial disease affections of the labyrinth.

7. That it is important that more thorough investigation of cerebro-spinal fever be made, and especially of its relation to deaf-mutism, with the hope, in the light of more perfect knowledge of its cause and methods of dissemination, the production of deafness from this cause may be diminished, as much as the sources of blindness were cut off by vaccination.

The deaths from Cerebro-spinal Fever in 1880, 1881 and 1882, as shown by the reports of the State Board of Health of Michigan, are compared below with the deaths from allied diseases. A few of the cases reported as Meningitis should probably be excluded from the Cerebro-spinal total, but, on the other hand, many cases of Cerebro-spinal Fever are undoubtedly included under the titles "Convulsions," "Brain Disease," and "Dropsy of the Brain."

Deaths from	1880.	1881.	1882.
Convulsions	347	377	356
Brain Diseases	295	370	324
Dropsy of the Brain	34	54	35
Cerebro-spinal Fever—			
Spinal Fever	71	136	97
Spotted Fever	1	9	2
Meningitis	22	21	16
Cerebro-spinal Meningitis	5	41	84
Spinal Meningitis	58	123	84
Total	157	330	223
Mean	270		

This would indicate 16.5 deaths to 100,000 inhabitants, and extended to the whole United States would indicate a mortality from this cause of 8266 per annum. With the estimated mortality of epidemics, this would represent over 16,000 cases, but as it is well known that the fatality among sporadic cases is much less, and that many cases escape recognition, the total number of cases occurring must be much greater.

CAUSES OF DEAF-MUTISM.

TABLE I.—REPORT OF IOWA INSTITUTION FOR DEAF AND DUMB, 1885. 306 CASES.

			Per cent.
Congenital		53	17.3
Spinal Meningitis	22		
Spinal Fever	35		
Brain Fever	19		
Cerebro-spinal Meningitis	26		
Spotted Fever	13		
Cerebro-spinal Fever		115	37.5
Scarlet Fever		23	7.5
Cause unknown or not designated		52	17.
Remainder various causes		63	20.6

Digitized by Google

TABLE II.—AGGREGATED REPORTS OF 13 INSTITUTIONS FOR THE DEAF AND DUMB (EXCLUDING IOWA). 8321 CASES.

			Per cent.
Congenital		2796	33.6
Meningitis	173		
Spotted Fever	225		
C. S. Meningitis	504		
Brain Fever	464		
Cerebro-spinal Fever		1366	16.4
Scarlet Fever		1542	18.5
Causes unknown or not designated		1008	12.1
Remainder various causes		1609	19.3

TABLE III (TABLE II EXTENDED).—8321 CASES FROM REPORTS OF 13 INSTITUTIONS FOR THE DEAF AND DUMB.

Congenital	2796	Rheumatic Fever	10
Scarlet Fever	1542	Nervous Fever	7
Meningitis	173	Whooping Cough	67
Spotted Fever	225	Intermittent Fever	13
C. S. Meningitis	504	Mumps	20
Brain Fever	464	Diphtheria	34
Convulsions	57	Discharge from Ears	343
Hydrocephalus	44	Scrofula	50
Paralysis	19	Measles	201
Fever	210	Smallpox	7
Unknown or not designated	798	Chicken Pox	8
Typhoid Fever	166	Burns	4
Typhus Fever	3	Colds	110
Inflammatory Fever	18	Croup	11
Catarrhal Fever	4	Scattering	349
Lung Fever	64		

TABLE IV.—RESULT OF EXAMINATION OF 83 DEAF-MUTES, DEAFNESS ATTRIBUTED TO CEREBRO-SPINAL FEVER.

a. Age at which disease occurred.

Under 1 year	12	14 per cent.
Over 1 and under 2 years	27	33 "
Between 2 and 5 years	31	37 "
Over 5 years	13	16 "

b. Hearing power.

49 Failed to hear tuning fork.
1 Heard T. F. through the air, but not through bone, with both ears.
2 Heard T. F. through bone, but not through air, with both ears.
3 Heard T. F. with both ears, both through air and bone.
5 Heard through air with one ear, and not through bone.
10 Heard through bone with one ear, and not through air.
10 Heard through air and bone with one ear.
3 Doubtful.

c. Condition as shown by inspection.

40 Cases, 80 ears—Membranes and Canals normal.
6 Cases, 6 ears—Membranes and Canals normal on one side.
16 Cases, 29 ears—Doubtful; Canal obstructed by dirt or cerumen.
24 Cases, 48 ears—Pathological both sides.
3 Cases, 3 ears—Pathological on one side.

DISCUSSION.

Dr. L. TURNBULL, Philadelphia, Pennsylvania, inquired if Dr. Hobby had employed the hearing trumpet in testing his cases of deaf-mutism. Are there not epidemics of meningitis in the western countries? Has not syphilis an important agency in the production of deaf-mutism in young children?

Digitized by Google

Professor G. E. FROTHINGHAM said his examination of the cases of deafness produced by cerebro-spinal fever agreed with that of Dr. Hobby as stated in his paper. He had found, in a large majority of cases, a normal appearance of the drum-membrane. He had met with mixed cases, of which the history pointed to cerebro-spinal meningitis so positively that there could be no doubt that the patient had suffered from it; there had also been inflammation of the middle ear, and, in such cases, the pharynx and nasal cavities had shown an inflammatory condition; while these had been much benefited by treatment, those presenting normal drum membranes with pervious Eustachian tubes, he had found to be hopeless. He had heard of improvement in one instance only; the family of a child informed him that it had partially recovered its hearing. He had once seen an infant with double optic atrophy from cerebro-spinal meningitis. For three or four months its condition was such that its attention could not be attracted by light, while the optic papillæ were glistening white. It afterward recovered sufficient vision to reach for large objects, and seemed to delight in viewing them. These cases could be explained by supposing that the destructive change had not involved *all* the nerve cells and fibres in the rapidly growing child, but had left a small number capable of future growth.

Professor E. DE ROSSI inquired whether Dr. Hobby had made *post-mortem* examinations in any of the cases.

Dr. HOBBY had not; his study of the cases had been purely clinical. Those who have been so fortunate as to have opportunity to examine those mute from this cause have found lesion of the labyrinth.

RESEARCHES ON THE MICROÖRGANISMS IN THE EUSTACHIAN TUBE OF HEALTHY INDIVIDUALS.

RECHERCHES SUR LES MICRO-ORGANISMES DANS LE TUBE D'EUSTACHE CHEZ LES SUJETS SAINS.

UNTERSUCHUNGEN ÜBER DIE MIKROORGANISMEN IN DER TUBA EUSTACHII GESUNDER INDIVIDUEN.

BY PROFESSOR DE ROSSI,
Rome, Italy.

When it was established that there existed in the saliva and in the follicles of the tonsils of healthy men, indisputable evidence of the presence of pathogenic microörganisms, and especially of the presence of the coccus pyogeneus aureus, it seemed to me desirable to undertake certain experiments to ascertain whether, or not, there existed microörganisms in the Eustachian tube, and if so, to ascertain their peculiarities. I fully realized the difficulties of obtaining positive and indisputable results. I assure you, however, that I have taken all the necessary precautions; that I have observed all the indispensable minutiæ which present scientific observations exact.

The method of procedure has been as follows: A fine platinum wire was fastened into a silver catheter. This wire was of such a length that it could be projected from the tubal end of the Eustachian catheter to the distance of one centimetre. The terminal end of the platinum wire was bent so as to form a loop in order to retain a sufficient quantity of the mucus collected from the inside of the Eustachian tube.

Digitized by Google

Before sterilizing the instrument with the usual care, the wire was withdrawn into the interior of the sound, then the tubal extremity was sealed with a thin coating of celloidine, and every catheter so prepared was placed into the sterilizing tube.

In introducing the catheter, it was passed as far as possible into the mouth of the Eustachian tube, and by pressing on the proximal end of the platinum wire, the thin coating of celloidine was broken and the looped end of the platinum wire projected to the distance previously determined. It was then withdrawn to the inside of the catheter. The loop of the platinum was immediately employed for the culture plates of gelatine, and later the colonies which were developed were cultivated in tubes.

The experiments were begun on the 12th of April, with the assistance of Professor Guarneri, of the Institution of Pathological Anatomy in Rome. In that institution, through the kindness of Professor Marchiasafa, all the necessary appliances were at our service.

The mucus was taken from the Eustachian tubes of twelve persons, at different times between the 12th and 30th of April. From each loop of wire two attenuations were used for plate culture. Positive results were obtained from only six of the cases —six out of twelve.

Case 2 gave two successful cultivations. The planting took place on the 22d of April, and by the 1st of May the colonies were developed. In the plantings from cases 1, 7, 8, 9 and 10, the colonies were developed on the 3d of May. The plates belonging to the other cases remained free from organisms.

The examination of the colonies developed presented the following features :—

Plate 2 in the first attenuation, the colonies were of a yellow color and the surrounding gelatine was fluid. Examined under the microscope it revealed the streptococcus. Further from both the first and second cultivations a colony which was of a white color was developed, which revealed under the microscope the grape form (staphylococcus).

Plate 1 exhibited two colonies, one gray, the gelatine remaining solid ; the other white, the gelatine liquefying. Under the microscope the white colony revealed torules, and the gray colony, with the gelatine unchanged, gave the streptococcus.

Plate 7 presented a round colony of a yellow color, the gelatine fluid. The microscope demonstrated the presence of short rods (bacilli) of a length varying from 0 mm. to 1 mm. and 0.5 mm. thick.

Plate 8 gave two colonies, one round and of a yellow color, the other yellow, but irregular in form. In neither of these cases did the gelatine become fluid. The microscope revealed a staphylococcus from 1 to 1.5 mm. long.

Plate 9 revealed a colony irregular in form and liquid gelatine. It was composed of filaments of different lengths ; some from ten to twelve mm., and others 1 mm. long. The thickness varying from 0.5 to 0.7 mm.

Plate 10 revealed a colony of a yellow color and the surrounding gelatine fluid. It was composed of rods (bacilli) from one to three mm. long and from 0.6 to 0.8 mm. thick.

From all these colonies pure cultures were made in gelatine tubes and their identity established after the culture. Cultures were also made on potatoes ; and finally from both the cultures in gelatine and the potato, transplantations were made to animal tissues.

It would be useless here to detail the individual cases. Experiments were made in the peritoneum and subcutaneous tissue in the rabbit and guinea pig, but in no case was any pathological development manifest. We are not to infer from this that in the mucus of the Eustachian tube there never exists pathogenic organisms. It is necessary to investigate, and I present to the Congress these notes as an interesting study worthy of being pursued.

Digitized by Google

DISCUSSION.

Professor G. E. FROTHINGHAM said he had listened to Professor De Rossi's very interesting paper, presenting the result of his original researches in a field that had not been before investigated in this manner. The result of the investigations is such as confirms views he had for some time held from simple empirical observation. He had already expressed these views, and his reasons for them, and would not now repeat. When he had declared these views, he had no knowledge of any investigation in this direction. It is peculiarly gratifying when scientific discovery harmonizes with facts learned empirically, as did Koch's discovery, that cholera bacilli cannot live in acid solutions, harmonize with the fact observed years ago, in the Pennsylvania Almshouse, that the use of sulphuric acid lemonade would sometimes prevent the spread of epidemic cholera. We cannot say the result is negative, because Prof. De Rossi discovered no pathogenic organism. He thinks the presence of these organisms in great numbers, whether pathogenic or not, serves to irritate the membrane, and, under certain conditions of atmospheric change, and of the constitution of the patient, will lead to inflammatory action, acute or chronic. We should not forget this effect, since in respiration of contaminated air such vast numbers of germs may be lodged upon the naso-pharyngeal spaces, and thus reach the ear.

If it be urged that *these* are not pathogenic, who can say under what circumstances they may not become so? The virus of syphilis or gonorrhœa is not pathogenic for some individuals; experimenters have bathed their eyes for hours with the secretion of Egyptian ophthalmia, and after exposure to wind and dust, for some hours afterward, had suffered no attack. He thinks no one now knows nearly all the conditions that determine the question, as to whether certain of these germs are always harmless. Investigation must be made to decide this.

As the etiology of certain chronic diseases of the middle ear is so obscure, and the treatment of them so unsatisfactory that it constitutes a reproach to otology, it becomes us to hail with welcome every carefully conducted scientific investigation in this direction. Prof. De Rossi is a pioneer; he has done good work and he is entitled to our thanks.

Dr. R. TILLEY, Chicago, Illinois, thinks it interesting to observe how different listeners get consolation and encouragement. While the previous speaker derived encouragement from the paper read in support of his views of the pathological influence of these organisms, I, on the other hand, derive encouragement from the paper in support of the idea that the influence of certain organisms is, at present, greatly over-estimated. I am glad the observations have been made and reported to us, notwithstanding their negative results, even after the germs were introduced into the peritoneal cavity and into the subcutaneous tissue.

Dr. A. B. THRASHER, Cincinnati, Ohio.—I am much pleased to learn of the admirable researches of Professor De Rossi in this direction. That he has found bacteria in this region only accentuates what we have heretofore pretty well known, viz.: that all mucous membranes in contact with the external air contain bacteria. But these microörganisms are by no means all pathogenetic. It appears to me that we are especially indebted to Prof. De Rossi for his endeavors to discover which are the pathogenetic germs in this region. When the pathogenetic bacteria or bacilli have been certainly recognized, then can we say that we have made some satisfactory advance in this direction.

Dr. H. GRADLE, Chicago, Illinois, remarked that the admirable researches of Professor De Rossi showed us an analogy between the Eustachian tube and other mucous

Digitized by Google

passages, like the Fallopian tubes, in which Fraenkel had found several pathogenic microörganisms; or the mouth, in which numerous harmless and virulent bacteria are known to exist. In otology it is necessary to distinguish accurately between exciting and predisposing conditions. The only exciting causes of suppuration are some microörganisms and, occasionally, chemical irritants.

If "taking cold," that is to say, temporary foreign circulatory disturbances, be accounted a cause of sudden inflammation of the middle ear, it is really an influence only, which permits the bacteria, already present in the tube, but yet outside of the tissues, to multiply and enter the tissues and produce disease. Similarly, the presence of these bacteria in the tube explains why a nasal douche can, at times, produce acute inflammation of the middle ear, by conveying infectious material into the ear. Water alone could not give rise to such consequences.

Dr. LEARTUS CONNER, Detroit, Michigan, stated that he is pleased to know a new method for the study of aural diseases. In normal conditions we find that the bacteria cause no harm; it becomes important to study bacteria under such additional conditions as result in actual disease, and then we shall have light of added value.

Professor DE ROSSI, in closing, said, "while the experiments I have narrated gave negative results, yet it is quite possible that under the depressing influence of cold draughts or other causes these organisms may contribute the determining influence which may result in some of those outbursts of inflammatory trouble, which sometimes come on so unexpectedly."

IS GENERAL ATROPHY OF THE CONDUCTING APPARATUS OF THE EAR IDENTICAL WITH PROGRESSIVE ARTHRITIS DEFORMANS?

L'ATROPHIE GÉNÉRALE DE L'APPAREIL CONDUCTEUR DE L'OREILLE EST-ELLE IDENTIQUE AVEC L'ARTHRITE DIFFORMANTE PROGRESSIVE?

IST ALLEGEMEINE ATROPHIE DES LEITENDEN APPARATES DES OHRES IDENTISCH MIT FORTSCHREITENDER ARTHRITIS DEFORMANS?

BY S. O. RICHEY, M.D.,

Of Washington, D. C.

The name general atrophy of the conducting apparatus may not be better than the numerous other names by which this affection is designated, but it has the merit of describing the result of the process as we see it, instead of indicating it by some particularity of its course. Some attempt will be made, herein, to show its probable neurotic origin in the spinal system, by its similarity to a more general affection which has been supposed to find its source there.

Atrophic degeneration of the conducting apparatus of the ear may not be, to any great extent, inflammatory in any part of its course, is *not* "preëminently local in its character," *is* influenced by constitutional dyscrasia, probably begins at the cervico-spinal nervous centres, and is propagated through the sympathetic nervous system or the sensory spinal nerves, interfering with local trophic action.

Many pathological changes have been observed in the cavity of the middle ear at

Digitized by Google

its examination after death, but we are not assured thereby that any given structural variation has been a result of this affection alone, as we are denied the opportunity of observing it during the progress of the disease. For many reasons, we must think it has a broader pathogenesis than that usually accredited to it, in the exposition of which one may be aided by the processes of analogy and induction.

Garrod says, of progressive arthritis deformans ("Reynolds System of Medicine," vol. I, page 555), "it is much easier to prove what rheumatoid arthritis is *not*, than to give the slightest clue to what it is . . . . ; it appears to result from a peculiar form of malnutrition of the joint textures, *an inflammatory action with* defective power . . . . ; it usually occurs in weakened subjects, and exposure to cold in many cases is the exciting cause of its development." Weber (*Journal of Mental and Nervous Diseases*, vol. VIII, 1883, page 630) considers it of neurotic origin. In its entire history, except in the functional peculiarities of the locality attacked, it is almost a complete analogue of atrophy of the middle ear, in causation, symptoms, progress, and therapeutics. It would be an advantage to study the process, as we cannot, in the ear.

Herewith is a parallel of the two affections—

Progressive Arthritis Deformans.	*Atrophy of the Conducting Apparatus.*
1. It is seldom fatal.	1. We do not know it ever to be fatal.
2. At an early stage, swelling and the appearances of ordinary inflammation are prominent.	2. At an early stage this may be the cause of symptoms of inflammation.
3. When the effusion into the joint is absorbed, the capsule is commonly found thickened, the cartilages are sometimes absorbed, and the ligaments so much lengthened as to allow unusual mobility and dislocation.	3. When this happens it would be liable to cause tinnitus, or impaired hearing, or both: a flapping m. t., and disarticulation of the ossicula.
4. At the commencement of the process, slow absorption of the cartilages takes place, often followed by fatty degeneration, and the formation of ligamentous bands.	4. The result of this change has been seen in anchylosis of the ossicles, especially of the stapes; retraction of the m. t., and bands of adhesion in the cavity.
5. Heredity does not seem to influence the affection, for one member of a family may be affected, and the rest be free.	5. Complete correspondence.
6. Is frequent among women, and rare among men.	6. Is more frequent among women than among men.
7. It occurs at any age, and individuals of weak frames whose extremities are cold are most liable to the disease.	7. The symptoms are manifest at middle age, or just before, and at any later period. It may begin in the earache of children. Cold extremities are common.
8. Everything debilitating, as uterine hemorrhages, prolonged grief, persistent mental distress, loss of rest and dissipation, damp dwellings, poor food, and all rheumatic influences, are supposed active causes.	8. Idem.
9. By test, no uric acid, or urate of soda, *thus removing rheumatism* and *gout* from consideration as causes: reduction of phosphoric acid in the urine.	9. No tests, so far as I know.
10. The disease is slowly but steadily progressive. It may be stationary for a time, but exacerbations are sure to follow (Weber). There is slight remission, but no intermission, during the rest of the patient's life (Haygarth).	10 There are long intermissions in progress, judged by the impairment of function.

Digitized by Google

Progressive Arthritis Deformans.	*Atrophy of the Conducting Apparatus.*
11. It usually begins as a subacute disease.	11. An open question.
12. It is very intractable. When the disease is not advanced, *the affected joints few in number* and progress slow, the prospect is more hopeful, especially if there is no disease to keep up the impairment of the general health.	12. Idem.
13. There is generally aching of the affected joints, prophetic of an increase of pressure in the atmosphere.	13. Any existing impairment of hearing, or tinnitus, is increased under the same circumstances.
14. Frequent mental depression without a known sufficient cause.	14. Idem.
15. It does not lead to suppuration, but to atrophy and more or less deformity.	15. Idem.
16. The treatment must be sustaining. Local treatment by blisters, iodine paint, and croton oil in the beginning. Later use counter-irritation; later still, friction and slight motion. Living in a moderate winter climate, nutritious food, warm clothing, etc.	16. This general line of treatment is the best with which we are acquainted.

Arthritis deformans begins in the smaller joints of the body, is symmetrical in appearance and progress, with lesions of the tissues surrounding the joints, atrophy of the muscular tissue, and, in old cases, a state of fatty and connective tissue degeneration (Weber). The Lilliputian joints of the ossicula auditûs are peculiarly exposed to atmospheric changes by their location, and are in one of the extremities of the body, for which reasons they would seem to be more liable to an attack of this affection than even the joints of the hand or foot. Rheumatoid arthritis, beginning in the small joints of the extremities, advances to the larger joints of the body, in which fact we may find an explanation of the pressure and pain about the head, and the diminution of intellectual apprehension, so common in cases of profound deafness in advanced aural atrophy. It may furnish a better demonstration of the deafness of boiler makers, ship caulkers, and locomotive engineers. Taking its symmetrical onset and advance as a point in evidence of its neurotic origin, it may also explain the change in voice so commonly met with among the profoundly deaf, who have become so by slow and progressive stages, for the recurrent laryngeal nerve makes the connection between the cerebro-spinal nerve centres and the vocal cords very intimate. The recurrent laryngeal is supposed to get its motive power from the pneumogastric, and irritation of the pneumogastric in the upper part of the neck has been proven by experiment to cause heat and tingling of the ear. Jewell (*Journal of Mental and Nervous Diseases*, 1874, 426) "looks upon articular rheumatism, as well as certain painful affections of the joints simulating rheumatism, as produced . . . . by disease of the nerve trunks or nerve centres, leading to decided local irritation at the peripheral termination of certain nerves," and Brown-Séquard has shown that nerve fibres going to the blood vessels of the various parts of the head come out chiefly from the spinal cord by the roots of the last cervical and first dorsal nerves. Leloir and Degerine * observe that, in a case of chronic rheumatism with considerable muscular atrophy and rapid eschars, they found the cutaneous nerves adjacent to the eschars affected with atrophic parenchymatous neuritis, which seemed to have existed previous to the eschars. Acute atrophy of the muscles has occurred without *lesion* of the cord. Those suffering with arthritis

* *Le Progres Médicale*, April 2d, 1881.

Digitized by Google

deformans appear to be emaciated and neurasthenic, as a rule, complaining of pains over a great part of the body, associated with periodical failure of control or power in the muscles or tendons. The pain in the joints, weakness in the muscles and tendons, and some emaciation often precede the manifest changes in the size and formation of the joints.

Women, according to Rosenthal, are more subject to prosopalgia in early life than men; neuralgia is most frequent between the thirtieth and fiftieth years of life; is sometimes accompanied by *inability to fix the mind on any subject or attend to business, and this effect is not due to pain.* Arthritis deformans is often introduced by hemicrania, and lean persons have a more decided predisposition than the stout, as in neuralgia of the trigeminus. The temporo-maxillary and the upper cervical vertebræ are joints particularly likely to be affected. No one questions the character of neuralgia, and arthritis appears to have a similar neuropathic origin and similar favoring causes.

Weber-Liel * has seen thirteen persons affected by progressive deafness presenting the symptom of spontaneous pain over the tracks of the cervical and brachial plexus, associated with pain in the ears and disagreeable tinnitus, varying from that habitual to the case. Otalgia is often met with in the later stages of atrophy, generally uncomplicated with neuralgia elsewhere, but among individuals of the neurasthenic type.

Arthritis deformans, nervous exhaustion and aural atrophy (progressive deafness) very greatly resemble each other. Each follows causes exhaustive in character; does not terminate fatally; most of the symptoms are subjective and functional, and often unaccompanied with apparent structural variation. In each there is periodical hopelessness and discouragement. In nervous exhaustion and the ear affection there is diminished ability to fix thought on any subject (lack of mental control) and change in the voice; and Garrod claims that the irregular form of arthritis sometimes attacks the internal (middle?) ear and the larynx, and causes hoarseness and a peculiar dry cough.

Mr. R. W. Parker † reports a case of rheumatoid arthritis; the girl, aged fifteen years, whose father died of phthisis and whose mother died of chalky rheumatism, had, in six months, become almost quite deaf. She had double keratitis and enlarged joints. No examination of the physical condition of the ears appears to have been made, unfortunately; only the reference above given to the disturbed function. C. H. Burnett ‡ mentions a woman, aged twenty-six years, well nourished, who, six years before, had an attack of probable rheumatic facial paralysis. Two or three years later she noticed singing in her ears and impaired hearing. Lustre of the mtt. good; ett. pervious; when excited or fatigued there was a flush of the cheeks and neck and increased tinnitus.

In the deafness of boiler makers, undisturbed control of equilibrium and the absence of vertigo argue against a theory of labyrinthine trouble. Buck (*New York Medical Record*, July 5th, 1875) thinks the peculiarities of these cases due to rigidity of the ligament at the base of the stapes, or to some change in the membrana secondaria, which, to my mind, is the most natural explanation. The fact that individuals who have had acute or subacute catarrhal inflammation of the middle ear present the feature of hearing better in a din of some kind, does not invalidate Buck's theory, as, in even acute suppuration, the membrana secondaria may undergo changes calculated to produce this effect; persistent thickening, calcareous deposits, adhesive secretions, etc., for which reasons some of these cases can be comparatively promptly improved. "Pathological alterations take place in the stapedio-vestibular articulation in the

* *Monats. für Ohrenheilk.*, Aug., 1874.

† *Transactions* International Congress, 1881, Vol. I, p. 128.

‡ "A Treatise on the Ear." 1st Ed., p. 391.

Digitized by Google

course of chronic inflammation of the middle ear, *sometimes also with a perfectly normal state of the lining membrane.*"*

Among boiler makers the *continuous* action of the ossicula auditus renders them more liable to arthritis; and the exposure to draughts, lack of exercise of most of the other joints of the body, and irregularity in taking food, which is often less assimilable than it should be, furnish other sufficient factors in the causation of this affection. These same influences obtain among ship caulkers and locomotive engineers. That the disease may be manifest in no other joint is no sufficient reason against its attacking the ossicula when they are most used. It may extend to the sutures of the cranial bones and cause disturbance in their relation to each other, followed by a feeling of pressure or "weight on the head," or "as if there was an iron band around the head," or "as if there was an iron axle between the ears," resulting from even slight distortion. It may thus so derange the cranial contents as to interfere with normal mental alacrity and the memory, of which some people with "progressive deafness" are so acutely conscious in the later stages. It may alter the size and shape of the cranial foramina to such an extent as to cause pressure upon the nerve thus finding an exit, and result in neuralgia in the region supplied by the nerve; this being one of the ways in which persistent neuralgia is supposed to be produced.

Arthritis deformans may occur at almost any age; at first, in the most exercised small joints, and, if neglected, it will progressively attack every joint in the body. It would rarely be recognized in the ear before the age of thirty, when the true function of the ear begins to be impaired in the late stage of atrophy, though it might have existed from the age of four or five years, at which time it would have been in its inflammatory stage. This period of the affection would be marked by sudden onsets of pain of spasmodic or neuralgic character, causing at short intervals sharp, quick cries, followed by a period of ease and quiet. Though during the day there is entire comfort, the attacks are disposed to recur at night, the child sometimes waking from a sound sleep with a cry of distress, and falling to sleep again in a short time. These attacks of "earache" are supposed to be harmless because they do not result in suppuration, immediate deafness, or any other material change in function or structure, for they pass off after several hours of intermitting pain, leaving some tenderness to touch, to recur again the next night, to follow much the same course. The mt. may be hyperæmic, but is not thickened; the et. is as patulous as usual, and there is no perceptible increase of secretion; it may recur every evening of several days with entire subsidence of pain for the greater part of the twenty-four hours.

These attacks differ in several particulars from the catarrhal affection resembling it, which causes almost continuous pain, thickening of the lining membrane, diminution or closure of the et., and increase of secretion with bulging of the mt. on this account. Sometimes suppuration occurs if the case is not promptly and properly handled; nearly always there is more or less impairment of hearing from congestive thickening of the tissues or the presence of fluid in the cavity. After one catarrhal attack there may never be another. The same cause seems immediately productive of each, because each is more liable to happen at the change of the seasons, the child being more exposed to the cold and the damp air at these times.

Thus, even in childhood a differential diagnosis might be made from the catarrhal affections, and we may reason that the disease at the foundation of the atrophic process may *begin at any age*, although the atrophy is a malum senilis.

Von Tröltsch thought the disease without catarrhal symptoms should be given a different classification, but it is yet generally classed as a catarrh by authorities, though Pomeroy ("Diseases of the Ear," page 148), in a cursory way, says, "I believe that the

* Politzer, "Diseases of the Ear." Am. Ed., p. 86.

Digitized by Google

rheumatic diathesis, in many instances, has much to do with the obstinate character of this affection; the rheumatic inflammation, according to its well-known predilection for fibrous tissues, finding a lodgment in the muco-periosteal lining of the drum."

Whether or not atrophy of the middle ear is of the same origin as arthritis deformans it has a more extensive pathology than that allowed to it.

TREATMENT.

Arthritis is introduced by a chill (Bruce), followed often by hemicrania, indicating depression of the nervous and circulatory systems. This action may be induced by cold, emotional disturbance, or physical shock: it concentrates at the cerebro-spinal nervous centres, and radiates therefrom, to express itself in the organ of least resistance in an individual, in the form of pain and trophic changes. The views of the writer in regard to local treatment in aural atrophy may be found in the *American Journal of Medical Sciences*, April, 1887, pp. 413–423. Iodine vapor is our sheet-anchor for topical application, but our efforts may be materially aided by constitutional and hygienic influences, under which head come climate, clothing, food, and other items of general treatment.

Climate should have special consideration in the choice of a winter residence. This should be moderate in temperature, and as dry as possible. The sudden changes of temperature in the higher latitudes are more deleterious, because they take place through a lower thermometric range, and passing from the inside to the outside of the house may produce violent circulatory disturbances during cold weather unless the cold is moderate. A climate distinguished by a decided difference in temperature between day and night is undesirable, unless this variation is guarded against by fire and clothing, which means thought and care on the part of the individual not likely to be taken.

Clothing is very important, as it should be of such character as to afford protection against the depression of climatic variation, that next the body needing most thought, though generally it has least.

Three areas of the body are especially sensitive to changes of temperature, and seem, to a great degree, to influence the comfort of the whole body. Such spaces are the *cervical region of the spine, the posterior aspect of the arm just above the elbow, and the nates.*

The ordinary dress of men protects them very well, and the buttocks are most exposed; from this region the body may be chilled or warmed. The exposure of this part of a chilled body to the grateful influence of radiated heat diffuses more general composure than warming the extremities, and every *man* will receive this suggestion feelingly. The effect is probably due to the superficial location here of the sciatic nerve, and to its short cutaneous branches.

Among women the cervical region of the spine and the arms are least covered, especially when in "evening dress." When entering a cold bath the body may be more quickly adjusted to the lower temperature by dipping the elbows and the nates, than by wetting the head and neck, according to the usual custom; and in this we may find some proof of the statement. Women, and especially neurasthenics, often complain at the menstrual period of cold on the posterior face of the arm just above the elbow. When one has his arms bared he may be seen to hold his elbows with his hands unconsciously, unless at work. This habit may be observed among workmen and washer-women, and sometimes among fashionable dames in bare arms. This part is supplied with cutaneous branches from the brachial plexus, and thus has more than local influence; to protect it from cold is instinctive. Women ordinarily have one thickness of dress upon the arm and neck; sometimes two on the arms; and, in the coldest season, often *none* on arms or neck. The dress may be worn high, and covered with wraps during

Digitized by Google

the warmest part of the twenty-four hours, to be exchanged frequently, for a décollete habit when the temperature is the lowest (slippers are not forgotten). May we not find in these facts some of the predisposing causes of the greater frequency of this aural affection among women? Fashion is without discretion, and is a Moloch to which health is unpityingly sacrificed (self-sacrifice). Intelligent advice may be given in regard to dress, but Fashion scorns it, and medication must be to little purpose without rational precautions on the part of the patient.

GENERAL MEDICATION.

Avoidance of shock, mental distress, damp cold, pregnancy, and whatever else tends greatly to disturb the balance of circulation, is to be advised. Nutritious food is to be taken regularly, and in such quantity as can be digested and assimilated. The moderate use of red wine is beneficial. Liq. potas. arsenit., in drop doses, taken for some months, promotes digestion and assimilation, in addition to its specific action upon the mucous membrane. In the same way it is probably useful in anæmia and certain forms of neuralgia. Syr. ferri iod. is serviceable in cases dependent upon impoverished blood, and may be associated with arsenic in the same prescription. Any gain from the administration of cod-liver oil has not been apparent to me. The salicylates have had manifest influence in some cases. Due attention should be given to the proper performance of its functions by every organ of the body, and particularly to the action of the bowels. A habit of constipation must be corrected, to aid nutrition.

The above outline, taken with the local manipulations heretofore described, are of more certain value in the treatment of aural atrophy than any other known to me. It is needless to say that a number of cases exist in which the structural injury is of such character and so established that nothing short of re-creation will restore to the organ the conditions necessary to its intended duties, as, for instance, osseous anchylosis or disarticulation of the ossicula.

DISCUSSION.

Dr. C. M. Hobby, Iowa City, Iowa.—I can recall seven cases of arthritis deformans in persons related to each other not more remotely than second cousins, and occurring in the ramifications of a large family, and among more than thirty members of this family that I can now recall there has been but one case of deafness.

Dr. L. Turnbull, Philadelphia, Pennsylvania, saw a case of this disease, arthritis deformans, on Canonicut Island this summer, every one of whose joints was immovable; he was like a chalk man. When I called to treat him for ulceration of the cornea his hearing was perfect. In another case that I saw every joint was out of its natural position; the hearing was perfect. (Inquiry being made whether these gentlemen had examined the ears, and if they found any structural change, they answered they *had not examined* the ears.)

Professor G. E. Frothingham, Ann Arbor, Michigan, said he had listened to Dr. Richey's paper with much interest, and all would admit that he had presented the theory with sufficient show of argument to challenge attention. While Professor Frothingham could not say that he is ready to accept the views presented, they are worthy of more careful consideration than could be given in an off-hand discussion of the subject, which, in the aspect in which Dr. Richey presented it, is new, at any rate to him. It is true, we have long acknowledged certain changes in the drum membrane and the articulation of the bones of the ear as due to a gouty or rheumatic condition, but the claims made in the paper he has not met with before. It is a subject upon which he must reserve his opinion until further consideration, as it

Digitized by Google

does not fully accord with his present views. He is all the more glad, on that account, to hear this view presented, as progress is best made by interchange of views, and stimulation to research grows out of opposing theories. He will repeat, "that man is a public benefactor who makes *two* blades of grass grow where one grew before." On the same principle, that man is a medical benefactor who gives us two ideas where we had but one before.

In obscure subjects, like the disease under consideration, we should take into respectful consideration any plausible theory, and, for one, he is willing to devote to it careful study.

Professor E. De Rossi, Rome Italy, inquired in what way Dr. Richey would distinguish between atrophy due to the cause suggested by his paper, and atrophy of the ear consecutive to hypertrophic processes.

Dr. Richey stated that in the published abstract of his paper he had stated that the question of atrophy of the ear consequent to hypertrophy would not be raised in the paper. In answer to Professor De Rossi, however, he said the *history* of the case would suggest an antecedent hypertrophic condition, by increased secretion and diminution of the upper air passages at some time; periods of greatly impaired function with relief, often without foreign agency; a subacute catarrhal condition, with thickening of the membrane involved, due to passive congestion, the impaired hearing being noticeable, especially during the exacerbations, and the absence of neuralgic pain. In atrophy following arthritis deformans, on the other hand, there is, if any, very slight thickening of tissue; no perceptible increase in secretion; very slow and progressive impairment of hearing, intractable in character; and neuralgic pain.

He did not anticipate immediate acceptance of the views offered. How can anchylosis, with prominence of the malleus and incus, be better explained? Is *dry* catarrh an inflammation? Mr. Henry Power, of London, England, in his remarks upon bacteria, in the Section on Ophthalmology, had intimated that the germs might, by migration, produce the joint affection, and Dr. Richey could not see that such a supposition would invalidate the theory of a neurosis, but, on the contrary, would do much to support it. He believes the bacterial theory, in explanation of this obscure affection, to be the *other* idea in Professor Frothingham's mind.

As arthritis deformans begins in the smallest and most used joints in the body, it might exist in the ossicula auditus, and be absent elsewhere. Drs. Turnbull and Hobby had not examined the structures of the ears in the subjects mentioned by them; they *did not notice impairment of hearing.* Their observations are therefore negative, for how often do we see distortion of the malleo-incudal articulation, without perceptible impairment of hearing?

Digitized by Google

PHYSIOLOGICAL RESEARCHES ON NASAL VOWELS.

RECHERCHES PHYSIOLOGIQUES SUR LES VOYELLES NASALES.

PHYSIOLOGISCHE UNTERSUCHUNGEN ÜBER NASENVOCALE.

BY B. LOEWENBERG, M. D.,

Of Paris, France.

A. ACOUSTIC RESEARCHES AND THEORY.

Under the denomination of "nasal vowels," two kinds of phonemata are generally confounded, notwithstanding their considerable diversity with regard as well to the nature of their sonority as to the physiological conditions of their formation.

In my book on "Adenoid Growths of the Upper Pharynx,"* I have been led to study this subject while examining the troubles of function caused by these vegetations. Among the perturbations they produce, by obstructing the posterior opening of the nasal fossæ, we find modified pronunciation of all the letters which implicate the free passage of the expired air and its resonance in the nasal cavities.

After having first investigated the physiological formation of *m* and *n* and their transformation into *b* and *d* by the pathological obstruction of the naso-pharyngeal passage, due to these tumors, I then examined the changes undergone by the nasal vowels in the same affection, a subject which had not been studied before.

I begun by investigating the physiological position of the phonetic organs while pronouncing these sounds, and expressed myself in the following way (translation of my book, by Professor M. Jones, page 362) :—

"There now remains for our examination a group of phonetic sounds, . . . which have not been studied from this special point of view. These are the nasal vowels as they are pronounced in France† and in certain portions of Austria.

"Here the narrowing (viz., the straightening through which the air escapes while we form these letters) is effected by the elevation of the base of the tongue, on the one hand, and on the other, the depression of the soft palate. The special aperture which produces the characteristic sound peculiar to each letter lies, for the nasal vowels, still further back than in the case of lingual letters (*n*, for instance). The nasal vowels are formed while the column of air dividing between the buccal and nasal channels escapes, partly through the aperture restricted by the soft palate and the back of the tongue, which approach one another, and partly at the same time through the nasal fossæ, thus giving rise to the characteristic resonance of French pronunciation. The vowels *a*, *e*, *o* and *u*,‡ pronounced in this position, become nasal, and are then *an*, *ein*, *on* and *un*.

---

* B. Loewenberg. "Les Tumeurs adenoïdes du Pharynx nasal; leur influence sur l'Audition, la respiration et la phonation; leur traitement." Paris, 1879. This work has been translated into English by Professor Macnaughton Jones, and published in *The Medical Press and Circular*, 1879, and in an abridged way by my lamented friend, the late Dr. James Patterson Cassells, in the *Edinburgh Medical Journal*, same year.

† *Note of the Author.*—We have been surprised to find that the so-called French nasal sounds are not peculiar to French pronunciation, as is generally believed (for instance, by M. Brücke, "Grundzüge der Physiologie der Sprachlaute," second edition, 1876). We have met with them in certain dialects of the South of Germany, among others in that of Frankfort-on-the-Main.

‡ *Note of the Author.*—The vowels, after the French pronunciation, are written a, e, i, o, u, and *un* is considered to be the nasal sound of *u*; but, according to my researches, the nasal sounds

Digitized by Google

"We shall take this opportunity of referring to a subject which requires some explanation. We often confuse the nasal sounds of the French language (and which for this reason we shall call French nasal vowels) ("sons nasaux français") with the sounds similarly called "nasal," but of a different sonority, which are contained in the other Latin languages (Spanish, Italian, Portuguese*). We may call these latter (ang, eng, ing, ong, ung) "foreign or not-French nasal vowels" ("sons nasaux non-français)."

"The difference between these two classes of nasal sounds consists, as we think, in this: that in the above described narrowing the back of the tongue and the soft palate approach much more nearly to one another for the not-French nasal sounds than for the French pronunciation of these sounds . . . , the closure of the buccal cavity is more complete and the nasal aperture still more indispensable than in the case of French nasal sounds."

The preceding remarks, though now of nine years' standing (the contents of my book having first been published in the *Gazette des Hôpitaux*, as early as in 1878), seem to have been entirely overlooked, as well by linguists as by physiologists. It may be that this omission arises from my observations having been inserted into a book of practical surgery, in which they form one of the scientific applications of the subject (I take the opportunity of reminding you that the same ill fate happened to other new points forming part of the same work, as, for instance, to a study on the part the sense of smell plays in that of taste).

Since the publication of my book I have by different means corroborated and amplified my researches on this subject. Having previously proceeded only, as mentioned above, by examining the different positions I gave to the vocal organs while pronouncing the different nasal sounds, I now resorted to more vigorously scientific methods, as, for instance, to the graphic one. The results of these, however, shall be published in a more exhaustive paper on the subject, their place not being in a short communication as the present one must necessarily be.

I actually intend to expose the results of the application to my subject of an acoustic method intended to elucidate another point of this complicated study.

Among the different problems this matter involves, arises the question of the musical notes belonging to or characteristic of each nasal vowel. In other terms, we must ascertain which is the sound or which are the sounds the vocal cavities are tuned to for each of these.

Researches of an analogous kind are known to have been made with regard to the pure (not nasal) vowels by as eminent men as Donders, Helmholtz, Koenig and others, and, as far as concerns the consonants, by Dr. O. Wolf†. But no investigations, at least to my knowledge, have been made concerning the notes proper to the two distinct groups of nasal vowels, and I, therefore, have undertaken to study this point.

---

of i and u do not exist in French, and what is generally spelt "un" is the nasal sound of "*eu*." I, therefore, shall write it "*eun*" and the corresponding pure vowel "*eu*."

For those who may not have a thorough knowledge of foreign languages, I wish to add a

COMPARATIVE TABLE OF PRONUNCIATION.

French	*a*	corresponds	to the	*English*	sound of	*a*	in "father;"	*German*,	*a*.
"	*e* (*é*)	"	"	"	"	*e*	in "get," "let," *a* in "fate;"	"	*e*.
"	*i*	"	"	"	"	*i* in "still;" / *ee* in "heel;"		"	*i*.
"	*o*	"	"	"	"	*o*	in "before;"	"	*o*.
"	*eu*	"	"	"	"	*u*	in "curl;"	"	*ö* or *ü*.

* *Note of the Author.*—I since ascertained, from a reliable medical friend, that in the Portuguese language, especially patois, there exist other modifications of nasal vowels, still to be studied.

† Dr. O. Wolf. "Sprache und Ohr," Braunschweig, 1871.

Digitized by Google

Different methods have been employed in order to ascertain the notes peculiar to the buccal cavities when pronouncing the vowels a, e, i, o, ou (French pronunciation). The best way, in my opinion, is the one first followed by Prof. Helmholtz, and which consists in utilizing the phenomenon of resonance. A simple estimation of the musical note by the ear alone leads to errors, especially with regard to the octaves. This indeed happened to as eminent an observer as Prof. Donders, who noted the sounds produced by whispering the vowels.

I therefore determined to employ the method of resonance, which leads to the end slowly, but surely.

I happily was able to secure the use of an apparatus of the utmost perfection, belonging to and executed by the celebrated acoustician, Dr. R. Koenig (Paris). I gladly seize the occasion of publicly thanking my eminent friend for the devotedness with which he has placed his apparatus at my disposal. His tonometer, that I had the use of, consists of a great series of tuning-forks, differing from each other by only a few vibrations, tuned with the utmost precision. I was thus enabled to ascertain the different sounds to which the vocal cavities are tuned in each case.

I proceeded in the following manner: In order to get an approximate idea of the position of the nasal vowel sounds in the musical scale, I first pronounced a pure vowel and afterward one of the corresponding nasal ones (French or not-French) and tried to ascertain in an average way the interval between their two notes. Knowing from the above mentioned researches the exact sounds of a, e, i, etc., it was very easy immediately to determine the average notes of the nasal vowels. Once these had been found, I pronounced one of these nasals, stopped, and maintained the position of the mouth which I had found necessary to produce this letter. I then placed in front of my mouth vibrating tuning-forks chosen above and beneath the average note previously ascertained, till I had found the one whose vibrations were the most reinforced by the resonance of the mouth.

The results of these researches were as follows:—

MUSICAL NOTES I FOUND FOR EACH NASAL VOWEL.

PURE VOWEL.	NOTE FOUND.	FRENCH NASAL VOWEL.	NOTE FOUND.	NOT-FRENCH NASAL VOWEL.	NOTE FOUND.
o	$B\flat^3$ (896 V. S. *).	on	G^3 (768 V. S.).	ong	B^2 (480 V. S.).
a	$B\flat^4$ (1792 V. S.).	an	$F\sharp^4$ near G^4 (1470 V. S.).	ang	B^3 (960 V. S.).
e	$B\flat^5$ (3584 V. S.).	en	Beneath G^5 (3008 V. S.).	eng	B^4 (1920 V. S.).
eu	Between $F\sharp^3$ and $G\sharp^3$ (728 V. S.), besides $C\sharp^4$ (1100 V. S.,) was found.	eun	Toward A^3 (858 V. S.).	eung	E^3 (640 V. S.).

* V. S. signifies "simple vibrations" ("vibrations simples," in French). The sounds found by me, and contained in this Table, may be expressed by the accompanying musical signs or notes, with the exception of a few vibrations.

A glance at the preceding Table shows at once that the proper sounds of the French nasal vowels of the first three series are very nearly the minor thirds inferior to the notes for the corresponding simple vowels, while those of the not-French ones approach by about half a tone the lower octave of these.

Digitized by Google

The results are different for the series: "en, eun, eung." This difference is extremely remarkable, and is perhaps due to the intermediate nature of *eu*, which, the same as the German *ü* or *ue* (French u) (a sound for which the English language has no equivalent), combines the lingual position of one vowel with the buccal one of another. For the latter reason one could call these two "mixed" or "intermediate" vowels (Vermittlungs vocale, as Lievers calls them).*

The results found with the not-French nasal vowels are still more striking, for we find the not-French nasal sound of a vowel to be distant by half a tone only from the proper sound of another simple vowel; for instance, the proper note of A is near $B\flat^4$, the one of O near $B\flat^3$, and we have found as the sound for the not-French nasal "ang" B^3, and so on.

The results of my preceding researches are not only very interesting themselves, but they open new fields to physiologists as well as to linguists.

THE TECHNIPHONE SNAP—A SUBSTITUTE FOR THE WATCH, AS A TEST FOR HEARING.

LA PERCUSSION DU TECHNIPHONE—UN SUPPLÉANT POUR LA MONTRE COMME UN CRITÉRIUM.

DER TECHNIPHONSCHNAPP—EIN STELLVERTRETER DER UHR ALS GEHÖRPROBE.

BY HENRY BYRD YOUNG, A.M., M.D.,

Burlington, Iowa.

The insufficiency of the watch as a test for hearing is well known, and the desirability of something which will more nearly approach that point of excellence reached by Snellen's types as a test for vision, need not be discussed. This ground has been often traversed, and recently again in a very thorough manner by a special committee of the American Otological Society. It is, therefore, only necessary to say that such a test must be some device which will be simple of construction, cheap, easily managed and reasonably uniform. And combining these conditions very happily and completely is the little device herewith presented and known as the *techniphone snap*—the sounder of the stringless piano called the techniphone.

It is simple of construction. Consisting merely of a piece of strap steel about ¼ mm. in thickness, 3 cm. in length and 1 cm. in width, dished (dented) slightly at its centre in such a way that the edges are not stretched.

It is cheap. This is self evident.

It is easily managed. Placing the ends of the strip against the thumb and a finger respectively, and preferably of the right hand, the centre is seized edgewise by the thumb and a finger of the other hand and made to spring against its concavity. When an excursion of about 2 mm. has been made, the part that is stretched reaches its point of reversal and changes with a distinct snapping sound. Released, it springs back and the sound is repeated. This sound can be produced rapidly, slowly or irregularly, as the operator may desire. If the sound is louder than necessary, it can be nicely and

* Lievers. "Grundzüge der Lautphysiologie," Leipzig, 1876, p. 43.

Digitized by Google

definitely muffled by seizing the centre flatwise instead of edgewise—the thumb or finger resting on the point of greatest convexity.

It is reasonably uniform. Experience has shown that in the manufacture of these snaps there is no appreciable difference, either in pitch or intensity, among those having identical measurements. To get a change of pitch there must be change of dimensions; and, other things being equal, the longer strip usually means a lower pitch and the shorter a higher pitch.

Taking it all in all, it is a more convenient test than the watch (a stop-watch), and, being just as good, should be substituted for it. That it does not fully reach that point of excellence found in Snellen's types as a visual test, is conceded.

It is possible to show a relationship between the image of 5′ angle and satisfactory vision by a careful computation of the retinal end organs involved. To establish a gauge of such mathematical exactness for the hearing power, a better knowledge of the acoustic nerve and its physiology is yet required. If, however, it is borne in mind that the test is not intended to decide how much a patient *should* hear, but merely how much is heard at one or another time, and that with trifling expense and little trouble the patient may be subjected to a number of tests by snaps of different dimensions with their corresponding differences of pitch, the comparison with Snellen's type is not unwarrantable.

One great advantage which it possesses over the watch is to be noted in the possible improvement of case reports; and for this, if nothing else, it should be generally adopted. How often, for instance, is it seen recorded that a patient's hearing, "as tested by the watch, was $\frac{20}{50}$ in the diseased ear? and what real information is to be gotten from the statement? It is supposed, of course, that the patient heard at twenty inches only that which had been heard at fifty. But the reader is uninformed as to the character of the sound, and is left to wonder whether the hearing must be extraordinarily acute generally, or particularly susceptible to such sounds, to perceive it at the greater distance.

With a snap of given measurements there goes a sound of definite pitch and intensity; and it becomes unnecessary to try a number of what may be estimated as normal ears with the watch (as recently advised by the committee of the American Otological Society) in order to get the so-called average maximum distance at which it can be heard. It only needs be noted that the patient heard the snap *free* at such a distance or muffled at such another distance; and the remote reader testing his own hearing by the same sound may form a judgment accordingly.

In conclusion, the writer desires to say that his attention was called to this snap quite accidentally, through a personal meeting, at the house of a mutual friend, with the invention of the techniphone. Its adaptability for the purpose now proposed instantly suggested itself. The snap was procured, used, and found so satisfactory that it was deemed proper to call the attention of others to it. It is therefore before you.

DISCUSSION.

Professor E. De Rossi, Rome, Italy.—We have not, at present, any accurate tests for hearing, to determine the condition of the power of hearing. Not even the acoumeter of Politzer is as serviceable as the watch, which we always carry in our pockets, and the human voice. The watch constitutes a very fair test of the perception of sound, and the human voice is practically the most important sound that the patient desires to perceive. Until such time as we have some more nearly perfect instrument, we must rely on the old instruments which have hitherto served us.

Dr. S. O. Richey thought improvement in any affection of the ear should be judged by structural changes, and that the surgeon should not rely on improvement

Digitized by Google

in function alone to decide the more healthful condition of the ear. When the structure is made normal, proper functional activity follows. Tests are chiefly valuable in differentiating the parts of the ear affected, and for indicating the final result, by comparison with the original indications by the same test.

THE VALUE OF NITROGLYCERINE IN TINNITUS AURIUM.

VALEUR DE LA NITRO-GLYCÉRINE DANS LE BOURDONNEMENT DES OREILLES.

DER WERTH DES NITROGLYCERINS BEI OHRENSAUSEN.

BY LOUIS J. LAUTENBACH, M.D., PH.D.,

Of Philadelphia, Pa.

After the usual experience in ear-work, and a gradual accumulation of unimproving cases of tinnitus aurium, I began to study the general effects of nitroglycerine, and to use it in these cases. It had been used by others in tinnitus, both with and without success, but I knew of no way of recognizing the cases in which it would be most likely to prove serviceable. In order to learn when to use it, I began to give it, in private practice, to all cases of tinnitus in which I had found no improvement under other treatment, and, in public practice, in all cases of tinnitus. In some cases there was improvement, in others there was none.

In the patients where improvement had occurred there was found to be present a similarity of conditions, and I soon satisfied myself that there was a class of patients in which the nitroglycerine treatment was valuable. I found it most serviceable in patients having tinnitus aurium, without much impairment of hearing, and where but little change had occurred in the naso-pharynx, and where it was found, on examination, that some abnormal condition of the heart existed, either functional or organic.

In many of these cases more or less structural changes, from catarrhal inflammation of the middle ear, were present, among them changes in the shape and translucency of the drumhead, with accompanying change in appearance or position of the triangular light spot. Follicular pharyngitis was present in some of the cases. The tinnitus was generally constant, or nearly so. It was not, as a rule, more marked when the patient was in a recumbent position; occasionally there was some remission in that position. The thermometric and barometric conditions of the atmosphere influenced the tinnitus. Damp weather, with low barometer, usually increased it.

Dull, heavy headache, more or less persistent, and most frequently located in the parietal regions, though sometimes located in the frontal region, was of frequent occurrence.

In these cases I used the nitroglycerine in pill form, and in doses of one hundredth of a grain. At first but one pill a day was given, generally in the morning. The amount given later was increased, enough of the pills being given to diminish the tinnitus, or to cause headache. As many as six of these pills were given in a day, though usually two were found to produce a beneficial effect. Improvement sometimes was manifest within a day or two after beginning the use of the remedy. In cases of long standing the remedy was sometimes continued for a period varying from one to three months before a satisfactory result was obtained. Cases, in which there was recurrence of the tinnitus, seemed to yield more readily on resuming the treatment than

Digitized by Google

when the remedy was first administered. The conclusion which I reached, after a fair trial of this remedy, was, that it is of value in certain cases of tinnitus aurium, especially in those where cardiac lesion exists, functional or organic, and where there is little or no loss of hearing.

EXHIBITION OF COLORED PLATES,

PREPARED BY PROFESSOR ADAM POLITZER,
Of Vienna, Austria,

SHOWING VIEWS OF THE MEMBRANOUS LABYRINTH.

EXHIBITION OF INSTRUMENTS AND APPARATUS.

EXPOSITION D'INSTRUMENTS ET D'APPAREILS.

AUSSTELLUNG VON INSTRUMENTEN UND APPARATEN.

CLOSING REMARKS BY THE PRESIDENT.

It becomes my duty to announce that the hour for final adjournment of the Section has arrived.

Before doing so, I wish to return thanks to the colleagues who have contributed to the work of the Section, and who have so fully coöperated in the endeavor to have the occasion one which might result in substantial progress in the general effort that is being made for the advancement of otology.

We all share in the regret at the absence of many eminent otologists who expected to have been present, and whose letters of regret have been presented here, in which they express their continued interest in otology, and their wish to have been present, and to have participated actively in the work of the Section, but who, from various and unavoidable causes, have been prevented from doing so.

The time allotted for the work of this Section has not afforded opportunity for full discussion of some of the questions in otology which, as announced at the opening session, it was designed to have presented during the progress of the work, and to have invited full consideration of them. The active interest which has been awakened in the whole subject of otology in the last few years, and of which this occasion has given further evidence, is justification for the expectation that before the time for the meeting of the Tenth Triennial Congress, at least some of the questions will have emerged from the uncertainty which yet surrounds them.

With thanks to all who have aided in the work of the Section, and especially to those honored confrères who have come from distant lands to do so, I now announce the final adjournment of this Section.

Digitized by Google

INDEX TO VOLUME III.

Abadie, Dr. Charles, 681, 684, 704, 730, 749.
Acetonuria in children, 596, 597.
Acid, hydrobromic, dilute, formula for, 74.
Address by Pres. of Sec. VI (Dr. Traill Green), 2.
VII (Dr. Wm. H. Pancoast), 122.
VIII (Dr. J. H. Callender), 232.
IX (Dr. A. B. Palmer), 352.
X (Dr. J. Lewis Smith), 438.
XI (Dr. J. J. Chisolm), 656.
XII (Dr. S. J. Jones), 802.
Agencies, natural, inhibiting pathogenic organisms, therapeutic employment of, 396.
Alcohol in medicine and disease, 91.
Allyn, Dr. George W., 785, 810, 841.
"American" hip-splint, 642.
Ametropia in schools, 787.
table of cases of, 790.
Anæmia, pancreatic, 409.
Anatomico-mechanical treatment in spinal diseases, 211.
Anatomy and pathology, photo-micrography in, 432.
human, didactic teaching of, 147.
methods of teaching, 224.
pathological, of laryngeal diphtheria, 508.
of superior maxillaries, 416.
(Section VII), officers, list of, 121.
Andeer, Dr. Justus, 100, 102.
Ankle joint, retro-calcanean bursæ in amputation, 175.
Appliance for reduction of contracted knee-joint, 646.
Arteries, main, surgical, collateral branches of, 194.
Artery, middle thyroid (anomalous), 165.
Arthritis deformans, identity with atrophy of conducting apparatus of ear, 862.
Artificial vitreous, use of the, after evisceration, 762.
Ashby, Dr. Henry, 583.
Aspiration in detachment of retina, 747.
Atkinson, Dr. George A., 57.
Aural furuncles, bacteriology and treatment of, 811.

Bacteria in dejecta of summer diarrhœa, 598.
Baginsky, Dr. Adolph, 596, 597.
Baker, Dr. A. R., 707, 765, 774.
Baldwin, Dr. B. J., 685.
Dr. D. J., 735.
Balkwill, Mr. Wm. E., 623.
Baratoux, Dr. J., 827, 839, 850.
Bermann, Dr. Isidore, 662.
Beyer, Dr. H. G., U. S. N., 62, 70, 102.
Bilharzia hæmatobium, 420.
Bishop, Dr. Seth S., 804, 839.
Blackburn, Dr. I. W., 407.
Blitz, Dr. A., 681, 841.
Blood, comparison of coagulation in rigor mortis, 251.
red corpuscle, function of, 285.
Boenning, Dr. H. C., 178, 246.
Bone, embryonic, development of, 151.
Booker, Dr. William D., 598.
Botany, medical, in the United States, synopsis of, 7.
Bouchut, Dr. E., 513, 515, 552.
Brackett, Dr. John E., 18, 43, 50, 55, 63.
Bradford, Dr. E. H., 646.
Brain, basal ganglia of, as psychic centres, 238.
Brains, etc., preparation of, for anatomical and pathological demonstration, 407.
Brown, Dr. Dillon, 531.
Brazil, hereditary syphilis and rachitis in, 575.
malaria of children in, 580.
Brugsch-Bey, Dr. A., 752.
Bull, Dr. Ole, 663, 745.
Burnett, Dr. Swan M., 708, 779, 785, 795.
Bursæ, retro-calcanean, in amputation through ankle-joint, 175.
Bush, Dr. Lewis P., 381.

Calcium sulphite in diphtheria, 574.
Calhoun, Dr. A. W., 681, 751.
Callender, Dr. J. H., 232, 251.
Calomel, antiseptic action of, 51.
Cannes, France, treatment of scrofula by sea baths at, 620.
Canton, Dr. Richard, 246.
Carter, Dr. J. M. G., 734.
Cascara sagrada, 43.
Cataract, extraction of without iridectomy, 692.
extractions and iridectomies, best after treatment of, 698.
operation for, 695.
operations, methods of, 688.
Cerebral function, 339.
gray matter, electrical phenomena of, 246.
irritation in young children, 452, 454.
Cerebro-spinal fever as a cause of deafness, 852.
Charlton, Dr. S. H., 455.
Chemical philosophy in remedy, 19.
Chlorosis, pigmentation of skin in, 436.
Children, acetonuria in, 596, 597.
diseases of (Sec. X), 437.
hallucinations in, 552.

Digitized by Google

Children, headaches in, 490.
rate of growth in, 446.
tables of growth and weight in, 450–452.
weight in, 447, 448.
young, cerebral irritation in, 452, 454.
Chisolm, Dr. Julian J., 655, 656, 662, 708, 714.
Cholera, supplemental respiration in, 108.
Cities, milk supply of, 477.
Clark, Dr. Daniel, 288.
Club foot, congenital, treatment of, 623.
Cobra De Capello, venomous secretion of, 63.
Cohen, Dr. J. Solis, 24.
Coghill, Dr. J. G. S., 9, 23.
Collinsonia canadensis, 76.
Conner, Dr. Leartus, 673, 862.
Contractured tissues, section of, 624.
Convulsions, infantile, etc., 540.
Coomes, Dr. M. F., 842.
Cornea, abscess and ulceration of, jequirity in, 781.
Corson, Dr. Hiram, 79.
Coutts, Dr. J. A., 540.
Cranium, skin-grafting upon, 166.
Cross, Dr. Richardson, 750, 764, 770, 780, 785.
Curtis, Dr. Lester, 381.
Cystotomy, suprapubic, 181.

Dacryocystic fistula, lachrymal drainage in, 796.
Danforth, Dr. I. W., 394.
Davis, Dr. N. S., 389, 395.
Day, Dr. W. H., 490, 503.
Deafness, caused by cerebro-spinal fever, 852.
Deformity, congenital, of spinal column, 160.
Dennett, Dr. Wm. S., 503, 506.
De Rossi, Dr. E., 809, 827, 839, 859, 862, 869, 874.
D'Espine, Dr. Adolph, 559.
Diarrhœa, bacteria in dejection of, 598.
Dibble, Dr., 713.
Dickinson, Dr. Fannie, 780.
Dr. William, 662.
Dickson, Dr. Jos. N., 175.
Digestion, cellular, its utility in pathological processes, 389.
Dight, Dr. C. F., 420.
Diphtheria, laryngeal, pathological anatomy of, 508.
turpentine and sulphite of calcium in, 574.
water pollution, sewerage, influence on, 440.
Dissecting room offal, disposal of, 178.
Doses, maximum, of medicine, 67.
table of, 69, 70.
Dott, Dr. D. B., 47.
Dronke, Dr. F., 103.
Drugs, action on kidney, 35.
progress in science of, 10.
Duncan, Dr. A. M., 55.

Ear, atrophy of conducting apparatus, identity with arthritis deformans, 862.
diseases of, in negro, 817.
diseases, statistical report of, 804.
internal, inherited syphilis in, 850.
middle, inherited syphilis in suppuration of, 844.
Earle, Dr. C. W., 409, 440.
Eckhard, Dr. C., 342.
Edson, Dr. Cyrus, 477.
Electrical phenomena of cerebral gray matter, 246.
stimulation of mammalian heart, 253.
Electrolysis in erectile tumors, 577.
Elliott, Dr. George R., 363.
Embryo, human, cerebral vesicle in, 138.
Embryonic bone, development of, 151.
Entropium, treatment of, 715.
Ergot of rye in ophthalmic practice, 760.
Eustachian tube, microörganisms in, 859.
Everts, Dr. O., 339.
Evisceration, use of artificial vitreous after, 762.
Eye diseases, hot water in, 673.
microbes in, 682.
troubles, in relation to occipital disease, 658.

Fats, crystallography of, in health and disease, 361.
Fermentation, alcoholic, of hepatic sugar, 293.
Fibres, elementary, physiology of, 290.
Fistulæ, urinary, anatomy and surgery of, 192.
Force, organic, transmutation in psychic, 339.
Ford, Dr. W. H., 293.
Frothingham, Dr. G. E., 714, 715, 808, 810, 815, 824, 828, 843, 852, 859, 861, 868.
Fulton, Dr. J. F., 728, 735, 810, 825.
Furuncles, aural, 811.

Galezowski, Dr., 686, 687, 692, 747, 752, 759, 765, 779, 782.
Ganglia, basal, of brain, as psychic centres, 238.
Gerlach, Dr. Jos. V., 138.
Dr. Leo, 144.
Gifford, Dr. Harold, 766.
Glands, sudoriferous (twin), 144.
Glaucoma in the negro, 755.
predisposition to, 752.
Glioma of retina, etc., 756.
Gnezda, Dr. Julius, 55, 63, 66.
Gradle, Dr. Henry, 662, 861.
Grauer, Dr. Frank, 591.
Green, Dr. Traill, 2, 6, 19, 34, 55, 67, 119.
Growth, rate of, in children, 446.
Gubb, Dr. A. S., 119.

Halliburton, Dr. W. D., 243, 251.
Hallucinations in children, 552.
Hamilton, Dr. Hugh, 19.
Hare, Dr. H. A., 33.
Headaches in children, 490.
Hearing, techniphone snap, test for, 873.
Hemorrhage, post-partum, supplemental respiration in, 108.
Herdman, Dr. W. J., 223.
Heyl, Dr. A. G., 672, 684, 738, 745.
Hip-splint, American, 642.
Hobby, Dr. C. M., 809, 823, 840, 852, 868.
Holcombe, Dr. Wm. Fred., 751, 843.
Hot water in eye diseases, 673.
Hotz, Dr. F. C., 681, 710, 714, 840.
Huckins, Dr. P. T., 681.

Digitized by Google

Hull, Dr. Geo. S., 50, 51, 55, 56.
Hypodermic injection of sterilized cultures, production of immunity by, 403.

Ice and cold water in inflammations, 79.
Infancy, formation of tumors in, 568.
tumors, mediastinal, non-congenital, in, 568.
Infantile convulsions and later neurotic tendencies, 540.
marasmus, 617.
pneumonia, 559.
Infants, use of cows' milk in, 485.
Inoculation, preventive of rattlesnake venom, 388.
International pharmacopœia, necessity for, 103.
Intubation, in laryngeal diphtheria, 508.
of larynx, history of, 531.
in croup, etc., 513, 516, 527.
tables of, 529, 530.
Iridectomies, etc., best after treatment of, 698.

Jackson, Dr. S. K., 396.
Dr. Edward, 785.
Jennings, Dr. C. G., 538.
Jequirity in abscess, etc., of cornea, 781.
physiological action of, 97.
Joint affections, anatomical points in, 199.
Jones, Dr. S. J., 713, 800, 802, 876.
Judson, Dr. A. B., 635, 642.

Keyser, Dr. P. D., 672, 680, 685, 686, 706, 714, 745, 749, 758, 765, 798.
Kidney, action of certain drugs on, 35.
Knee-joint, contracted, appliance for, 646.
Knight, Dr. James, 641.

Lachrymal drainage in dacryocystic fistula, 796.
Landolt, Dr. Edmund, 685, 704, 718, 750, 785, 787.
Langdon, Dr. F. W., 147.
Larynx, intubation of, 516, 527.
history of, 531.
in croup, etc., 513.
tubage of, in stricture and croup, 515.
Lateral curvature of spine, treatment of, 640.
Lautenbach, Dr. Louis J., 875.
Lee, Dr. Benjamin, 160.
Leeds, Dr., 484.
Lenses, necessity for reform in manner of designating, 783.
Leuf, Dr. A. H. P., 224.
Lewin, Dr. L., 66, 67, 101.
Liell, Dr. Edward N., 91.
Love, Dr. I. N., 244, 283, 285, 475, 617.
Loewenberg, Dr. B., 811, 870.

Magruder, Dr. G. L., 34.
Malaria in children in Brazil, 580.
Mammalian heart, electrical stimulation of, 253.
Manganese preparations, emmenagogue action of, 71.
Manolescu, Dr. N., 684, 695, 733.
Marasmus, infantile, 617.
Marmion, Dr. Wm. V., 662, 705.
Martin, Dr. Sydney, 97.
Mastoid cells, acute primary inflammation in, 825.
process, perforation of, artificial, 828.
Materia medica, history of progress in the United States, 2.
investigation of, by U. S. Government, 10.
Matthews, Dr. Joseph M., 140.
McWilliam, Dr. J., 253.
Medicines, maximum doses of, 67.
Memoriam, in, Dr. Alonzo B. Palmer, 350.
Microbes in eye diseases, 682.
Microörganisms in Eustachian tube, 859.
Milk, cows', use of, in infants, 485.
supply of cities, 477.
Moncorvo, Dr., 546, 575, 580.
Mooren, Dr. Albert, 658, 662, 688.
Morphia, pharmacology of bodies derived from, 47.
Motility, ocular, derangements of, 730.
Moura, Dr., 755.
Mules, Dr. P. H., 762.
Murrell, Dr. T. E., 698, 817.
Dr. William, 9, 22, 34, 41, 46, 50, 61, 62, 63.
Muscles, ocular, insufficiency of, 723.
recti, insufficiency of, 732.

Nasal vowels, physiological researches on, 870.
Negro, diseases of the ear in, 817.
glaucoma in, 755.
Nephritis, scarlatinal, 583.
anatomical characters of, 591.
Nerve and muscle, physiological doctrines in, 255.
Nervous system, trophic influence of, 249.
Newman, Dr. Robert, 460.
Nitrites and nitroglycerine, pharmacology of, 57.
Nitroglycerine, value of in tinnitus aurium, 875.
North, Dr. John, 424.
Northrup, Dr. Wm. P., 508, 539.
Nutrition, teeth as indicators of, 335.

Occipital disease in relation to eye troubles, 658.
Ocular muscles, insufficiency of, 723.
motility, derangements of, 730.
O'Dwyer, Dr. Jos., 516, 540.
Ophthalmia, sympathetic, 766.
Ophthalmology (Sec. XI), officers, list of, 655.
Ophthalmoscope, new pattern of, 798.
Organisms, pathogenic, inhibition of, by natural agencies, 396.
Orthopædic surgery, 623.
principles of progress in, 637.
Otology (Sec. XII), officers, list of, 800.

Paddack, Dr. H. Clay, 760, 843.
Palmer, Dr. A. B., 350, 352, 412.
Pancoast, Dr. Wm. H., address of, 122.

Digitized by Google

Pancreas, cirrhosis of, 409.
Parker, Dr. M. G., 432.
Pathological demonstration, preparation of brains, etc., for, 407.
Pathology of retinal vessels, 663.
(Sec. IX), officers, list of, 351.
Pelvis, male, frozen sections, showing relations, etc., 181.
Peritoneum, relations of, 181.
Pharmacology of bodies derived from morphia, 47.
Pharmacopœia, international, necessity for, 103.
Phillips, Dr. C. D. F., 2, 6, 9, 35, 41, 42, 46, 62, 63, 66, 119,
Photo-micrography, in anatomy and pathology, 432.
Physiological doctrines in inter-relations of nerve and muscle, 255.
researches on nasal vowels, 870.
Physiology (Sec. VIII), officers, list of, 231.
Pitner, Dr. T. J., 538.
Pitts, Dr. Barton, 715.
Pneumonia, infantile, 559.
Points, anatomical in joint affections, 199.
Politzer, Dr. Adam, 876.
Pollock, Dr., 34.
Poole, Dr. Thomas W., 255.
Potash, chlorate of, 23.
Pott's disease, pressure paralysis of, 363.
Poussié, Dr. Emile, 574.
Pouzet, Dr. Paul, 436.
Power, Dr. Henry, 680, 682, 703, 734, 752, 759, 762, 765, 787.
Prepuce, narrow, and preputial adhesions, 461.
Preputial dilatation, 457, 458.
President of Sec. VI (Dr. Traill Green), resolution of thanks to, 119.
Pressure paralysis of Pott's disease, 363.
Prince, Dr. A. E., 796.
Ptomaines and leucomaines, pathological relations of, 424.

Ramsey, Dr. W. M., 55.
Randolph, Dr. R. L., 759.
Rattlesnake venom, preventive inoculation of, 388.
Raynaud's disease, pathology of, 412.
Recti muscles, insufficiency of, 732.
Rectum, anatomy of, in reflexes, 140.
Redard, Dr. P., 577.
Red blood corpuscle, function of, 285.
Reflex secretion, salivary, 342.
Remedy, chemical philosophy in, 19.
Resorcin and preparations, 100.
Retina, detachment, curability of, 747.
glioma of, etc., 756.
Retinal vessels, pathology of, 663.
Retinoscopy, 770, 774.
Reynolds, Dr. D. S., 685, 705, 735, 759, 783.
Rhoads, Dr. J. N., 161.
Richey, Dr. S. O., 810, 823, 827, 841, 869, 874.
Roberts, Dr. M. G., 211.
Rockwell, Dr. A. D., 460.
Rückhard-Rabl, Dr., 136.
Rusby, Dr. H. H., 18.

Saint-Germain, Dr., 457, 458.
Salmon, Dr. D. E., 403.
Sayre, Dr. Louis A., 461, 476, 624.
Scarlatinal nephritis, 583.
anatomical characters of, 591.
Schaefer, Dr. F. C., 167.
Scrofula, treatment of, by sea baths, 620.
Secretion, salivary, reflex, 342.
Section VI, Therapeutics and Materia Medica, officers, list of, 1.
VII, Anatomy, officers, list of, 121.
VIII, Physiology, officers, list of, 231.
IX, Pathology, officers, list of, 351.
X, Diseases of Children, officers, list of, 437.
XI, Ophthalmology, officers, list of, 655.
XII, Otology, officers, list of, 800.
of contracted tissues, 624.
Servais, Dr. L., 416.
Sewall, Dr. Henry, 388.
Sewerage, etc., influence on diphtheria, 440.
Shakespeare, Dr. E. O., 373, 381, 395, 732, 734.
Shoemaker, Dr. John V., 76.
Simon, Dr. J., 452, 454.
Sinclair, Dr. A. G., 756.
Skin grafting on cranium, 166.
Skin, pigmentation of, in chlorosis, at phalangeal articulations, 436.
Smith, Dr. Eugene, 681, 686, 708, 751, 765, 781, 782.
Dr. J. Lewis (President, Sec. X), 437, 438, 445, 456, 475, 489, 506, 512.
Dr. Theobald, 403.
Mr. Noble, 637.
Souchon, Dr. Edmond, 194.
Spinal column, congenital deformity of, 160.
Spine, anatomico-mechanical considerations in diseases of, 211.
treatment of lateral curvature of, 640.
Stamm, Dr. M., 199.
Statistical report of ear diseases, 804.
Stellwagen, Dr. T. C., 335.
Stevens, Dr. Geo. T., 723, 735.
Stephenson, Dr. Wm., 446.
Sterilized cultures, production of immunity by hypodermic injection of, 403.
Stern, Dr. Max J., 165.
Stewart, Dr. F. E., 9, 34, 41.
Stockman, Dr. Ralph, 22, 41, 47, 50, 57, 71, 242.
Strabismus, advantage of operating early for, 728.
operation for, 718.
Stricture, anatomy of, 161.
Strong, Dr. Albert B., 181.
Sudduth, Dr. W. X., 151.
Sudoriferous glands (twin), 144.
Sugar, hepatic, alcoholic fermentation of, 293.
Summers, Dr. T. O., 249.
Superior maxillaries, pathological anatomy of, 416.
Supplemental respiration in cholera, etc., 108.
Surgery, orthopædic, 623.
principles of progress in, 637.
Sympathetic ophthalmia, 766.
Syncope, local (Raynaud's disease), 412.
Synechia posterior, operative treatment of, 686.
Syphilis, hereditary, and rachitis, in Brazil, 575.
inherited, in suppuration of middle ear, 844.
in internal ear, 850.

Digitized by Google

Tarsal border in trichiasis, restoration of, 710.
Taylor, Dr. Thomas, 361.
Techniphone snap as a test for hearing, 873.
Teeth, indicators of nutrition, 335.
Tetanus, table of cases, 374–378.
traumatic, infectious nature of, 373.
Therapeutics and Materia Medica, officers, list of, i.
Thompson, Dr. Jas. L., 680, 707, 713, 785.
Thrasher, Dr. A. B., 861.
Thyroid artery, middle (anomalous), 165.
Tiffany, Dr. F. B., 787.
Tilley, Dr. R., 809, 815, 823, 841, 844, 861.
Tinnitus aurium, value of nitroglycerine in, 875.
Toeplitz, Dr. M., 842.
Tonsils, ignipuncture of, 457, 458.
Torus longitudinalis in teleostians, development and homology of, 136.
Trichiasis, restoration of tarsal border in, 710.
Trophic influence of nervous system, 249.
Tubage of larynx in stricture and croup, 515.
Tumors, erectile, electrolysis in, 577.
formation of, in infancy, 568.
mediastinal, non-congenital, in children, 568.
Turnbull, Dr. L., 816, 823, 858, 868.
Turpentine in diphtheria, 574.
Tyrotoxicon, nature, chemistry and action of, on animals, 382.

Upshur, Dr. John N., 55, 62, 71.
Urinary fistulæ, anatomy and surgery of, 192.

Valcourt, Dr. De, 620.
Valk, Dr. F., 708.
Vaughan, Dr. Victor C., 382, 485, 489.
Vesicle, cerebral, in human embryo, 138.
Visual axis, 738.
Vitreous, artificial, after evisceration, 762.
Vowels, nasal, physiological researches on, 870.

Wade, Dr. De Witt C., 18, 62, 63, 74.
Ward, Dr. William, 108.
Water, cold, and ice, in inflammations, 79.
Waxham, Dr. F. E., 444, 527, 540.
White, Dr., 734.
Whooping cough, nature and antiseptic treatment of, 546.
Wile, Dr. Wm. C., 192.
Willard, Dr. De F., 473.
Wiltshire, Dr. J. G., 505.
Woodbury, Dr. Frank, 10, 34, 42, 47, 63, 67, 70, 119.
Wyss, Dr. Oscar, 568.
Wythe, Dr. J. H., 243, 290.

Young, Dr. H. B., 809, 824, 827, 842.

Digitized by Google

Digitized by Google

Digitized by Google

Digitized by Google

Digitized by Google

Digitized by Google

Digitized by Google

Digitized by Google

COUNTWAY LIBRARY
HC 36YI 4

418895

DATE DUE

19 MAR 75 CLM			

DEMCO 38-297

Digitized by Google

Digitized by Google